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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 19, 1857 *to April* 14, 1859 *inclusive.*

(BEING A CONTINUATION OF THE SERIES ENTITLED "ABSTRACTS OF THE PAPERS COMMUNICATED TO THE ROYAL SOCIETY OF LONDON.")

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VOL. IX.

ERRATA.

Page 538. Mr. Babbage was of the year next below Sir J. Herschel and Dr. Peacock—not of the same year, as stated in the text.

Page 584, line 10, for Dec. 19 read Dec. 9.

Page 632, line 2, for 137 yards read 274 yards.

Page 651, line 8, for No. V. read No. IV.

Page 718, line 10 from bottom, for b_x read l_x.

Page 719, line 4 from top, for $dy=ymr^z dz$ read $-dy=ymr^z dz$.

Page 721, line 7 from top, for y read Y.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 19, 1857.

Dr. W. A. MILLER, V.P., in the Chair.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting for the election of Council and Officers.

Mr. Thomas Davidson, Mr. George Bowdler Buckton, and Mr. Joseph Whitworth, were admitted into the Society.

Mr. Gassiot, Mr. Hardwick, Mr. Horner, Dr. Percy, and Mr. Archibald Smith, were elected by ballot as Auditors of the Treasurer's Accounts, on the part of the Society.

The following communications were read:—

I. "On the Anatomy of *Tridacna*." By J. D. MACDONALD, Esq.

(For Abstract, see vol. viii. p. 589.)

II. "Summary of a paper on the Spinal Cord as a leader for Sensibility and Voluntary Movements." By E. BROWN-SÉQUARD, M.D.

(See vol. viii. p. 591.)

III. "Summary of a paper on the resemblance between the effects of the section of the Sympathetic Nerve in the Neck and of a transverse section of a lateral half of the Spinal Cord." By E. BROWN-SÉQUARD, M.D.

(See vol. viii. p. 594.)

IV. "Experimental Researches on the Influence of Efforts of Inspiration on the Movements of the Heart." By E. BROWN-SÉQUARD, M.D.

(See vol. viii. p. 596.)

V. "Summary of a paper on the Influence of Oxygen on the vital properties of the Spinal Cord, Nerves, and Muscles."

(See vol. viii. p. 598.)

VI. "Summary of a paper on the Power possessed by Motor and Sensitive Nerves of retaining their vital properties longer than Muscles, when deprived of Blood." By E. Brown-Séquard, M.D.

(See vol. viii. p. 600.)

VII. "Ocular Spectres, Structures, and Functions, Mutual Exponents." By James Jago, A.B., M.B. &c.

(For Abstract, see vol. viii. p. 603.)

VIII. "On Hourly Observations of the Magnetic Declination made by Capt. Maguire, R.N., and the Officers of H.M. Ship 'Plover,' in 1852, 1853 and 1854, at Point Barrow, on the Shores of the Polar Sea." By Major-General Edward Sabine, Treas. and V.P.R.S. &c.

(For Abstract, see vol. viii. p. 610.)

November 26, 1857.

Major-Gen. SABINE, R.A., Treasurer and V.P., in the Chair.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President—The Lord Wrottesley, M.A.

Treasurer—Major-General Sabine, R.A.

Secretaries—William Sharpey, M.D.; George Gabriel Stokes, Esq., M.A.

Foreign Secretary—William Hallows Miller, Esq., M.A.

Other Members of the Council.—James Moncrieff Arnott, Esq.; George Busk, Esq.; Arthur Farre, M.D.; Edward Frankland, Ph.D.; John Peter Gassiot, Esq.; William Robert Grove, Esq., M.A.; Philip Hardwick, R.A.; Joseph Dalton Hooker, M.D.; Leonard Horner, Esq.; James P. Joule, Esq., LL.D.; Richard Owen, Esq.,

LL.D.; John Percy, M.D.; Lyon Playfair, Ph.D.; The Rev. Bartholomew Price, M.A.; Archibald Smith, Esq., M.A.; Charles Wheatstone, Esq.

Mr. Henry Clifton Sorby was admitted into the Society.

The following communications were read:—

I. "On the Expansion of Wood by Heat." By J. P. Joule, LL.D., F.R.S. &c. Received November 5, 1857.

In pursuing the researches of which abstracts have been given in the 'Proceedings' for January 29 and June 18, the author found that the heat evolved by compressing wood, cut either in or across the direction of the grain, was nearly that due to the application to the particular case of Professor Thomson's formula. Exact agreement could not be expected, on account of the discordant results arrived at by different experimenters on the expansion of wood. On investigating the subject, the author finds that the expansion of wood cut in the direction of the grain, is greatly influenced by the tension to which it is exposed, as well as by its humidity. A rod of well-seasoned and dried bay-wood, $\frac{3}{8}$ths of an inch in diameter, and exposed to the tension of 26 lbs., gave an expansion of ·00000461 per degree Centigrade, but when a weight of 426 lbs. was hung to it, its coefficient of expansion was increased to ·00000566. In conformity with this result, it was found that the elasticity of the rod was considerably diminished by an increase of its temperature. On investigating the effect of humidity, the author found that it occasioned a diminution in the expansibility by heat. After the rod of bay-wood with which the above experiments were made had been immersed in water until it had taken up 150 grains, making its total weight 882 grs., its expansion with a tension of 26 lbs. was found to be only ·000000436. Experiments with a rod of deal 33 inches long, and weighing when dried 425 grs., gave similar results. Its expansion when dry, with 26 lbs. tension, was ·00000428, and with 226 lbs. ·00000438; but when made to absorb water, its coefficient of expansion gradually decreased, until, when it weighed 874 grs., indicating an absorption of 449 grs. of water, expansion by heat ceased altogether, and, on the contrary, a *contraction* by heat equal to ·000000636 was experienced.

II. "On the Partitions of the r-Pyramid, being the first class or r-gonous x-edra." By the Rev. T. P. Kirkman, M.A., F.R.S. Received October 14, 1857.

(Abstract.)

Partitions proper of the r-pyramid are made by drawing diagonals none crossing another in the r-gonal base, and diapeds (intersections of non-contiguous faces) none enclosing a space, in the r-edral vertex. The object of the memoir is to enumerate the number of such partitions that can be made with K diapeds in the vertex and k diagonals in the bases of the pyramid. By the drawing of k diagonals, the pyramid becomes a $(r+1)$-acral $(r+k+1)$-edron, which by the introduction of K diapeds becomes a $(r+K+1)$-acral $(r+k+1)$-edron. Such a figure is termed an r-gonous $(r+K+1)$-acral $(r+k+1)$-edron of the first class. The definition of an r-gonous x-edron of the first class is that it contains a *discrete r-gony*, i. e. K diapeds and k diagonals of which *no diaped meets a diagonal*, and such that the convanescence of the K diapeds will form an r-ace, and the evanescence of the diagonals forms an r-gon.

If the summits upon the k diagonals be, one or more of them, partitioned by K$'$ diapeds, or the faces about the K diapeds be partitioned by k' diagonals, there arises a *mixed r-gony*, in which are one or more angles made by a diaped and a diagonal. If such a figure has not a discrete r-gony as well as that mixed one, and has no $(r+r')$-gony, by the vanescence of which the $(r+r')$-pyramid can be obtained, it is an r-gonous x-edron of the second class. And r-gonous x-edra of the third class can be obtained by partitioning the faces about the K$'$ diapeds and the summits upon the k' diagonals, in such a manner that no $(r+r')$-gony shall be introduced; and so on for higher classes of r-gonous x-edra.

It is proved that every partition proper of the r-pyramid, that is, any $(1+\text{K})$-partitioned r-ace laid on a $(1+k)$-partitioned r-gon, is an r-gonous $(r+k+1)$-acral $(r+k+1)$-edron. The number of the $(1+k)$-partitions of the r-gon, and of the $(1+\text{K})$-partitions of the r-ace is known by the formulæ given in the author's memoir "On the partitions of the r-gon and r-ace," in the Philosophical Transactions, 1857. The present memoir gives the formulæ whereby the

partitions of the pyramid are determined in terms of those of the *r*-gon and *r*-ace.

Thus the entire first class of *r*-gonous *x*-edra is enumerated, without descending to any classification of polyedra according to the rank of their faces and summits. The enumeration of the second and higher classes will require such classification, which will introduce so vast a complexity as to render the further prosecution of the theory of the polyedra, in the opinion of the author, practically impossible by any method deserving the name of scientific generality.

III. "Researches on the Cinchona Alkaloids." By W. Bird Herapath, M.D. Lond., F.R.S.E. Communicated by Prof. Stokes, Sec. R.S. Received June 19, 1857.

(Abstract.)

Part I.—Critical examination of the ordinary methods employed for the discrimination of the Cinchona Alkaloids, viz. Quinine, Quinidin, and Quinicine and Cinchonine, Cinchonidin and Cinchonicine; together with the optical and chemical characters of their Iodo-Sulphates, upon which new methods are founded.

In consequence of the gradually increasing scarcity of the *cortex cinchonæ calysayæ* and its chief product quinine, many other barks have been introduced into commerce, which furnish alkaloids having a strong general resemblance in the physical characters of those preparations of them more commonly employed in medicine, but differing widely in medicinal properties and commercial values.

In order to prevent fraudulent adulterations, it has long been highly desirable to have some ready methods of detecting admixtures of these alkaloids and their salts. The author having discovered several optical salts of these vegetable alkaloids, proposes to make their well-marked optical characters the means of such detection, and in the second part of this paper has fully developed his views upon this ready method of analysis, whilst in the present part he has passed under review the various existing tests for the different cinchona alkaloids, and the results of his investigations may be enumerated under the following conclusions:—

The following different methods of detecting the various cinchona alkaloids have been proposed :—

To Bouchardat and Pasteur we are indebted for the use of polarized light as a means of discriminating these alkaloids by the rotatory power which they exercise upon its plane.

Liebig employs the difference of their solubility in ether for the same purpose.

Almost all the other tests proposed have for their object only the discovery of quinine.

Professor Stokes employs fluorescence, combined with the peculiar reaction, in respect to this phenomenon, of hydrochloric acid, alkaline chlorides, &c. Brandes, the green reaction produced by the successive addition of chlorine and ammonia, whilst Vogel has modified this latter test in several ways.

Pelletier has employed the agency of a stream of chlorine gas, and Marchand uses nascent oxygen, obtained from puce-coloured oxide of lead and sulphuric acid for the discovery of quinine.

Leers first proposed a combination of Liebig's ether test, with that of Brandes's chlorine and ammonia reaction, as a means of establishing the purity of cinchonidin (miscalled by him quinidin, in common with all German chemists).

De Vry has advised the employment of hydriodic acid or iodide of potassium in order to discover the quinidin of Pasteur.

Van Heijningen depends on oxalate of ammonia to discriminate quinine from quinidin.

All these different tests the author has examined most critically, and, as far as it is possible to do so, determined the absolute numerical value of each method experimentally with the following results :—

He first explains MM. Bouchardat and Pasteur's researches on these remarkable alkaloids, from which it appeared that quinine and cinchonidin are powerfully lævogyrate, quinidin and cinchonine pre-eminently dextrogyrate, and that quinicine and cinchonicine are only slightly dextrogyrate upon plane-polarized light. These eminent experimenters determined also with accuracy the amount of these molecular rotations for each alkaloid. Yet the expensive nature of the apparatus, the complex formula requisite to reduce the observed amount of angular rotation to the normal molecular standard, and

the many interfering actions necessary to be guarded against, effectually prevented this from ever becoming a process for general adoption, either among chemists or manufacturers.

Another method of recognizing the presence of quinine is founded on the optical phenomena of fluorescence, which have been investigated by Professor Stokes. Whilst endeavouring to turn this process to account in the quantitative estimation of quinine by means of excessive dilution, and marking the points at which the various phenomena of "epipolism," "fluorescence," and "internal dispersion" vanish, the author arrived at the following extraordinary results; premising that he employs the term "internal dispersion" to mean the positive, "fluorescence" the comparative, and "epipolism" the superlative degrees of the same optical power:—

I. Solutions containing 1 grain in 35,000 of either quinine or quinidin of Pasteur, exhibit epipolism and fluorescence; solutions with 1 grain in somewhat less than 140,000 grains of water are still fluorescent, with slight internal dispersion.

When diluted with from 3 to 10 gallons of water, these alkaloids continue to exhibit internal dispersion.

Solutions of quinicine are only slightly epipolic, and if the change has been perfect, scarcely at all fluorescent, but nevertheless strongly absorptive of rays of high refrangibility.

Cinchonidin also exhibits optical phenomena, but in a much slighter degree; about $\frac{1}{100}$th part of that of either quinine or quinidin.

Cinchonine is also fluorescent about $\frac{1}{120}$th part of the same alkaloids.

II. That on mixing fluorescent solutions of quinine, quinidin, or other cinchona alkaloid with the soluble chlorides, although all traces of optical phenomena are lost to the eye, yet the media still possess powerfully absorbent powers on the rays of high refrangibility, and, if sufficiently concentrated, are wholly opaque to them, without exhibiting any of the phenomena of dispersion, and greatly impede chemical action.

This was proved by three methods of observation:—

1st. By introducing vessels containing fluorescent solutions of quinine into other vessels filled with non-fluorescent solutions of the alkaloids, produced by previous admixture with chloride of ammonium, when all optical phenomena disappeared from the inner vessel.

2ndly. By surrounding fluorescent specimens of fluor-spar with these prepared solutions of the alkaloids, when the blue colour in the spar immediately disappeared.

3rdly. By photography; employing concentrated solutions of quinine mixed with chloride of ammonium in troughs to intercept the incident light from any object anterior to the camera, when it was found almost impossible to obtain any image upon the sensitive collodion plate, although the intensity of the visible image received on the ground-glass screen did not suffer any apparent diminution.

4thly. By photographic printing; troughs containing these solutions obstructed the chemical rays very considerably, thus interfering with the production of a positive picture from the negative, much longer exposure being necessary to produce any chemical effect.

III. That certain reagents do not destroy fluorescence; others only mask its appearance by their own colour; whilst some destroy it by neutralizing the excess of acid; others do so by producing salts which are themselves non-fluorescent media. Whilst a third class destroy it by really modifying the alkaloid itself.

IV. That as so many reagents of common occurrence interfere with the manifestation of fluorescence, and as it is also a property common to all the cinchona alkaloids herein described, its appearance becomes no longer of any value as a test for quinine.

V. Brandes's chlorine and ammonia test will discover 1 grain of either quinine or quinidin in 1 gallon of water, but shows no difference between these alkaloids, except in very concentrated solutions, when there is a precipitate with quinidin, but not with quinine.

Quinicine is also influenced by this test, but less extensively.

VI. Dr. Vogel's first modification of this test is of no apparent value; but by also employing ammonia, the author has found that it will indicate both quinine and quinidin, detecting readily 1 grain of either in a pint, and showing slight evidence with 1 grain in 10,000 grains of water.

There is scarcely any reaction with quinicine.

VII. Dr. Vogel's other modifications of Brandes's test are unimportant, with the exception of the fourth, viz. excess of chlorine, and very little ammonia. This detects 1 grain in about 2000 grs. of fluid very readily, if excess of acid be avoided at first. The test,

however, is equally indicative of quinidin; it gives scarcely any perceptible reaction with quinicine.

VIII. Pelletier's chlorine gas-test succeeds very well with the free alkaloids, but does not show any indication with their salts. It is equally capable of detecting quinidin, and gives the same phenomena.

IX. Marchand's test is not a delicate reaction.

X. All the foregoing tests, although specially proposed for the discovery of quinine, possess equal powers and show the same appearances with quinidin. But they have no reaction on cinchonine, cinchonidin, or cinchonicine.

XI. Van Heijningen's test by oxalate of ammonia, produces, after some hours, a crystalline oxalate of quinine, when using a fluid containing only 1 grain of alkaloid in 800 grs. of water, and very readily detects immediately 1 part in 350. It does not precipitate quinidin or cinchonidin, but it produces a white precipitate in concentrated solutions of cinchonine.

XII. De Vry's test for quinidin by hydriodic acid, or iodide of potassium in neutral solutions, produces a well-marked crystalline precipitate as a colourless salt, when one part of the alkaloid is present in 1000 of the fluid; the crystals, being short hemihedral prisms, are readily recognized; the neutral hydriodates of cinchonidin are colourless, silky, prismatic needles, and much more soluble. If to a solution of the sulphate of quinidin in dilute spirit ($\frac{1}{3}$) we add hydriodic acid, and expose to the action of light during some days, there is formed the red iodo-sulphate of the author.

The neutral hydriodate of quinine appears as lemon-yellow prisms. The neutral hydriodate of cinchonine appears as long, thick, colourless prisms, and is very soluble.

XIII. Liebig's ether test dissolves quinine, quinicine, and cinchonicine, and therefore does not discriminate between them, as they are all uncrystallizable. It dissolves also a portion of the quinidin and cinchonidin. Should the proportions of these alkaloids not exceed the solvent powers of the ether employed, they will not be indicated by this test. When crystallization occurs, the rhombic prisms indicate cinchonidin; the long slender aciculæ, quinidin; whilst an amorphous powder is demonstrative of cinchonine. Ether also extracts cinchonidin from cinchonine; but its sparing solubility in ether necessitates the employment of warmth, and a large quantity of ether.

XIV. Leers' combination of the ether test with that of Brandes can readily detect small portions of quinine, quinidin, or quinicine in cinchonine or cinchonidin, especially when used in the manner as modified by the author.

Part II.—On the Optical and Chemical Characters of the Iodo-Sulphates of the Cinchona Alkaloids, Quinine, Quinidin and Quinicin, and Cinchonine, Cinchonidin and Cinchonicine; together with the Chemical Analysis of many of the Salts, and new methods of discriminating those Alkaloids, founded upon the production of these remarkable compounds, and the recognition of their optical characters.

In the former part of his paper, the author examined the existing tests for discriminating between the various cinchona alkaloids, and pointed out their insufficiency. In the present part, he shows that the optical characteristics of the iodo-sulphates of the alkaloids quinine and quinidin are sufficiently well marked to render the existence of *either one* of these alkaloids *certain,* and that although the iodo-sulphate of cinchonidin is very closely related optically and chemically to the homologous salt of quinine, yet there are sufficient points of dissimilarity to enable us to diagnose between the two; and, moreover, that the production of this salt is a beautiful means of deciding readily whether cinchonidin is present in specimens of cinchonine or cinchonicine; all evidence of quinine or its allies having been decided in the negative by the results of the previous tests, as proposed by Brandes, Vogel, Pelletier, Leers, or the author.

The cinchonidin of Wittstein has also, by the same method, been proved by the author to be totally different from the cinchonidin of Pasteur.

Acetic acid and chloroform may also be employed for discriminating between cinchonine and cinchonidin.

The *chemical characters* of all these iodo-salts furnish no means of discrimination, for as a class they all agree in being more or less soluble in spirit, giving a deep sherry-brown solution, from which water precipitates them in an amorphous form, as dark brown, cinnamon-brown or purplish-brown coloured precipitates; they are

only very slightly soluble in dilute spirit, and scarcely at all in water, ether, turpentine, or chloroform: acetic, dilute sulphuric, or hydrochloric acid have but little action upon them, whilst concentrated hydrochloric or sulphuric acid decomposes them. Nitric acid rapidly acts upon them, even in the cold, with violent evolution of nitrous acid and production of heat, iodine being oftentimes liberated in the crystalline form.

Alkalies also decompose them.

Sulphuretted hydrogen, soluble sulphides, sulphurous acid and sulphites, together with chlorine-water, instantly decolour their alcoholic solution, with the production of hydriodic acid.

In dilute alcoholic solutions, starch gives immediate evidence of iodine, and nitrate of silver gives a yellowish-white precipitate of iodide of silver, and some organic basic compound which can only be removed by the action of concentrated boiling nitric acid; this reaction, although commencing at the ordinary temperature, with violent disengagement of nitrous acid vapours, must be perfected by boiling.

Baryta salts exhibit the existence of sulphuric acid, which in all instances is an essential constituent in their formation.

The quinidin and cinchonine salts dissolve with more difficulty, in consequence of their greater thickness and less extent of surface.

Since the author had the honour of communicating his discovery of the optical salt of cinchonidin to the Royal Society (a preliminary notice of which was published in the 'Proceedings,' vol. viii. No. 24), he has ascertained that its primary form is, like that of the quinine salt, that of a right rhombic prism, and usually very thin, but having for its acute angles 43°, and 137° for its obtuse, with the rectangular axes $M^a_{\overline{2 \cdot 482}}$; $T^a_{\overline{1 \cdot 000}}$; $P^a_{\overline{\cdot 00001}}$—the quantity for P^a being variable and very minute. In a former communication to the Royal Society, published in the 'Proceedings' (Feb. 16, vol. vi. No. 24, 1854), the quinine salt was shown to have a primary rhombus, having 65° for the acute, and 115° for the obtuse angles, with the three rectangular axes, thus related:—$M^a_{\overline{1 \cdot 57}}$; $T^a_{\overline{1 \cdot 000}}$; $P^a_{\overline{\cdot 00001}}$.

In both salts the optical characters are usually examined through the shortest axis, P^a: in some recent observations on the quinine salt, the author has discovered that it transmits a *blood-red* beam of plane-polarized light through the axes M^a and T^a, and this is also a beam polarized in a plane parallel to that of the axes M^a and T^a.

TABULAR VIEW.

Both the quinine and cinchonidin salts are derivable from the primary rhombic prism, and crystallize as rhomboids and β-prisms	which obstruct plane-polarized light, when their longer diameters (M^a) are parallel to the plane of the polarized ray.
and α-prisms	which obstruct the same beam when their longer diameters (T^a) are perpendicular to the plane of the polarized ray.

Quinine salt	Rhomb obtuse 115°	1·57 $=M^a=$ 2·482	Cinchonidin salt	Rhomb 137°
	„ acute 65°	1·00 $=T^a=$ 1·000		„ 43°
		·00001 $=P^a=$ ·00001		

	Quinine salt	Cinchonidin salt
Transmitted rays or body-colours.	1. *Polarized parallel to axis* (T^a). P^a, colourless, greenish-white, yellowish-green. .	1. P^a, greenish-white, yellowish-green, dark olive-green.
	2. M^a and T^a, blood-red, *polarized in the plane of those axes*	2. M^a and T^a not observed.
	3. *Polarized perpendicular to axis* (T^a). P^a, pink, ruby-red, blood-red, sienna-brown . . .	3. P^a, violet, light blue, indigo-blue.
Reflected rays or surface-colours.	1. *Polarized perpendicular to axis* (T^a). Cantharidin-green, blue-green, grass-green	1. Brassy-yellow, golden-yellow, orange.
	2. *Polarized parallel to axis* (T^a). Dull olive-green, or vitreous and colourless on a dull black surface.	2. Dull olive, or vitreous and colourless on a dull black surface.

In the foregoing comparative Chart of the physical properties of the two salts, the axis has been assumed to coincide with a line drawn through the short diagonal of the primary rhombic crystal, which will coincide with the long diameter of the α-prism, and the plane of the breadth of the β-prism, and is therefore the T^a of the three rectangular crystallographic axes.

It has been compiled from the observations of Professors Stokes and Haidinger and the author. It appears to form a complete optical description of the two salts, as far as they are at present known.

Whilst in both salts the *indicative body-colours,* or those due to the more absorbed pencils (3), are only to be seen in the thinnest crystals, it is evident that the reflected rays may be seen indifferently in crystals of all thicknesses; and the author is inclined to believe that the cinchonidin salt possesses even greater tourmaline absorbent powers upon ordinary light, inasmuch as much thinner plates are required in order to obtain the indicative body-colours,—perfect absorption, and therefore total obstruction, being more early arrived at than in the case of the quinine salt.

The author's more recent analyses of the cinchonidin salts have produced the following results :—

Sulphate of Iodo-Cinchonidin.

	I.	II.	III.	IV.	Mean.
Iodine......	39·727	39·462	39·246	38·488	39·478
Sulph. acid..	8·390	8·673	8·882	8·593	8·701
Carbon	34·936	35·73	35·792	..	35·486
Hydrogen ..	4·321	4·301	..	..	4·311
Nitrogen....	2·976	..	..	..	2·976
Oxygen	9·650	..	..	..	9·048
	100·000				100·000

which lead to the following composition :—

				Theory.	Mean of Experiments.
57 Carbon	× 6	=	342	= 35·367	35·486
40 Hydrogen	× 1	=	40	= 4·147	4·311
2 Nitrogen	× 14	=	28	= 2·884	2·976
12 Oxygen	× 8	=	96	= 10·052	9·048
3 Iodine	× 127	=	381	= 39·297	39·478
2 Sulph. acid	× 40	=	80	= 8·294	8·701
			967	100·000	100·000

which probably give the following formula :—

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^{2}\,O^{5} \\ I^{3} \end{matrix}\right\} 2SO^{3}, HO+5HO=967.$$

One other remarkable difference exists between the quinine and cinchonidin salt, which is, that the optical crystals of the last salt, if allowed to remain in the mother-solution with an excess of less than 1 per cent. of sulphuric acid, undergo a transformation, and become long, golden, silky aciculæ, radiating in beautiful globose tufts: this salt has some doubly absorbent powers also, but very feeble. When this salt is attempted to be redissolved in boiling spirit, in order to be recrystallized, it does not re-form, but the optical crystals are then produced; when the silky crystals are carefully air-dried, they retain their yellow colour, but if exposed over sulphuric acid at 62° Fahr., or if attempted to be dried at 212° Fahr., they lose 5·32 per cent. water=6 atoms, and become a dark greenish-black residue, which is a tri-hydrate and contains the following by analysis :—

	I.	II.	III.	IV.
Iodine	40·504	40·407	..	..
Sulph. acid....	9·064	8·324	..	..
Carbon	..	..	36·082	35·689
Hydrogen	..	36·404	36·28	36·583

numbers which very closely correspond with the following :—

						Theory.	Experiment.
57 Carbon	× 6	=	342	=		36·037	35·835
38 Hydrogen	× 1	=	38	=		4·004	4·047
2 Nitrogen	× 14	=	28	=		2·950	2·851
10 Oxygen	× 8	=	80	=		8·433	8·063
3 Iodine	× 127	=	381	=		40·147	40·455
2 Sulph. acid	× 40	=	80	=		8·429	8·699
			949			100·000	100·000

and the formula may be provisionally given as—

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^{2}\,O^{5} \\ I^{3} \end{matrix}\right\} 2SO^{3}, HO+3HO=949,$$

which closely corresponds with the optical salt, but contains 2 atoms less water.

If this olive-coloured residue be boiled in dilute spirit, the optical crystals deposit on cooling.

From the addition of 5·32 per cent. water to this dry residue, we find that the silky crystals contain dry residue,—

	94·678	= 949	= 1 atom
Water	5·322	= 54	= 6 atoms
	100·000	1003	

and we have thus the following formula for the silky salt, which corresponds most closely with the result of analysis,—

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^2\,O^5 \\ I^3 \end{matrix}\right\} 2SO^3\,HO + 9HO = 1003,$$

as may be seen by the following comparison :—

				Theory.	Experiment.
57 Carbon	× 6	= 342	=	34·097	33·947
44 Hydrogen	× 1	= 44	=	4·386	4·423
2 Nitrogen	× 14	= 28	=	2·791	2·700
16 Oxygen	× 8	= 128	=	12·764	12·800
3 Iodine	× 127	= 381	=	37·986	37·914
2 Sulph. acid	× 40	= 80	=	7·976	8·216
		1003		100·000	100·000

giving—

Carbonic acid........	125·22	124·472 per cent.
Water..............	39·474	39·807 „

Consequently the silky salt will be the optical salt + 4 atoms of water, which, under the influence of excess of sulphuric acid and prolonged delay at 62° Fahr., are assimilated by that salt; and which additional water, on boiling in spirit, is lost, and the optical salt recrystallized on cooling.

If the temperature be not too high *at first*, the silky crystals may be produced without the appearance of the optical. And the silky crystals at 212°, or at 62° Fahr. over sulphuric acid, become the dry residue or tri-hydrate, which, when boiled in spirit, becomes the optical, by assimilating 2 atoms of water, as may be seen by comparing the three proposed formulæ :—

a. Optical.

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^2\,O^5 \\ I^3 \end{matrix}\right\} 2SO^3\,HO + 5HO = 967$$

β. Silky Salt.

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^{2}\,O^{5} \\ I^{3} \end{matrix}\right\} 2SO^{3}\,HO + 9HO = 1003.$$

γ. Dry Residue.

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^{2}\,O^{5} \\ I^{3} \end{matrix}\right\} 2SO^{3}\,HO + 3HO = 949.$$

The results of the analysis of the cinchonidin salt having been so remarkably different from those of the formulæ generally adopted for the pure alkaloid, the author was induced to prepare some perfectly pure quinine, taking especial pains to exclude all cinchonidin; and having from that prepared some iodo-sulphate of quinine, to submit it to equally rigid analysis.

The results are the following:—

These optical crystals lose 2·49 per cent. water by prolonged drying at 212° in Liebig's drying apparatus.

The residue contains the following:—

	I.	II.	III.	IV.	V.
Iodine......	30·195	30·033	30·50	31·729	31·69
Sulph. acid..	10·246	9·352	..	9·631	9·854
Carbon	41·554	..	41·34	41·456	41·048
Hydrogen ..	4·766	4·762	..	4·54	4·7375
Nitrogen....	3·711	..	..	3·380	..

Carbonic acid mean of 4 = 151·614
Water .... = 42·326

leading to the following composition:—

			Theory.	Experimental means.
57 Carbon	= 342	=	41·606	41·3496
38 Hydrogen	= 38	=	4·623	4·7025
2 Nitrogen	= 28	=	3·409	3·5455
10 Oxygen	= 80	=	9·730	9·8024
2 Iodine	= 254	=	30·900	30·8925
2 Sulph. acid	= 80	=	9·732	9·7705
	822		100·000	100·000

which, with 2 atoms water, constitute the optical salt dried over sulphuric acid at 62° Fahr., thus :—

1 atom dry residue	822
2 atoms water	18
	840

and these results may be expressed by the following formula :—

$$\left.\begin{matrix} C^{57}\,H^{33}\,N^{2}\,O^{5} \\ I^{2} \end{matrix}\right\} 2SO^{3}\,HO+5HO=840,$$

which appears to be the constitution of the optical salt dried at 62° Fahr. over sulphuric acid.

From this it appears that the optical salt of quinine differs in chemical atomic numbers merely in the possession of 1 atom less iodine, the cinchonidin salt having 3, the quinine salt 2 atoms iodine; but in each case 2 atoms of sulphuric acid, and 5 water, with an organic base of $C^{57}\,H^{33}\,N^{2}\,O^{5}$ common to both. How this is derived from $C^{40}\,H^{24}\,N^{2}\,O^{4}$ in the one case, or $C^{40}\,H^{24}\,N^{2}\,O^{2}$ in the other, it is difficult to point out in the present state of the question.

Were these views correct, it might naturally be imagined that the two salts may be mutually convertible. The author has undertaken numerous experiments with this object in view; and whilst he has proved that it is possible (by boiling the quinine salt in spirit surcharged with iodine) to communicate the golden tint of the reflected ray and the blue tint of the body-colour to the crystals on their re-formation, yet this modified salt retains the crystallographic forms of the true quinine salt; whilst, by treating the cinchonidin salt by spirit and aqueous sulphurous acid, that salt is modified also, becomes fibrous in character, and assumes the red body-colour of quinine salt, yet is at once to be distinguished from the true quinine salt even by the naked eye alone; and on redissolving these in spirit, the blue body-coloured salt again recrystallizes with its ordinary golden reflected tint. The effect of diluted sulphuric acid in converting the cinchonidin salt into the golden silky fibrous variety, is a striking distinguishing characteristic between the two alkaloids.

These facts lead to the conclusion, that the grouping of the constituent molecules in the two salts differs materially; that closely as the quinine and cinchonidin salts agree amongst themselves, they differ widely from the quinidin and cinchonine compounds.

The quinidin salt, after recrystallization, presents itself as long

quadrilateral acicular prisms, having a deep ruby or garnet-red colour, with a bluish-violet or light purplish reflexion-tint; it is sometimes deposited in thin flat plates, or long, flat, acicular prisms; these, when thin, transmit a pure yellow colour, but in thicker plates it becomes reddish, with a tinge of brown.

There is scarcely any appearance of double absorption in this salt; the thicker crystals alone exhibit it, when their usual tint becomes darkened on analysis with a Nichol.

This salt requires 31 parts of boiling spirit, and 121 parts at 62° to dissolve 1; water precipitates it as a cinnamon-brown powder.

Its deep marone-coloured large aciculæ had a specific gravity of 1·7647 at 62°.

These large crystals, exposed whole to a temperature of 212°, decrepitate afterwards on exposure to the air, but dried at 212°, they do not appear to lose further water after prolonged exposure to the drying bath.

The author having supplied Dr. Sheridan Muspratt with a quantity of this salt, has been most obligingly furnished with the results of his examination; from which it will be seen that those previously obtained by the author have been confirmed in the most satisfactory manner by that experienced analyst.

	Herapath.				Muspratt.	
	I.	II.	III.	IV.	I.	II.
Iodine	39·665	39·570	39·740	39·88	39·73	39·131
Sulph. acid	6·273	6·390	6·326	6·302	6·263	..
Carbon	32·890	32·615	32·787	..	31·998	32·311
Hydrogen	3·960	3·958	4·028	3·985	4·001	3·937
Nitrogen	4·400	4·440	4·440			
Oxygen	12·812	13·027	12·697			
	100·000	100·000	100·000			

The formulæ derivable from these analyses are the following:—

			Theory.	Herapath.	Muspratt.
35 Carbon	× 6	= 210	= 32·967	32·764	32·154
25 Hydrogen	× 1	= 25	= 3·924	3·984	3·969
2 Nitrogen	× 14	= 28	= 4·395	4·44	..
10 Oxygen	× 8	= 80	= 12·559	12·743	..
2 Iodine	× 127	= 254	= 39·874	39·73	39·78
1 Sulph. acid	×	40	= 6·274	6·339	6·263
		637	100·000	100·000	

and give—

$$\left.\begin{matrix} C^{35}\,H^{19}\,N^2\,O^4 \\ I^2 \end{matrix}\right\} 1SO^3\,HO+5HO=637,$$

which differs from Gerhardt's formula for quinidin by the loss of $C^5\,H^5$; but at this stage of the question it is scarcely possible to arrive at a solution of the manner in which it is produced.

The cinchonine salt differs much from all those previously described; it exists in long, acicular, quadrilateral prisms, of a deep purplish-black colour, like that of elder-berries.

Thin crystals transmit a yellow tint—pure gamboge-yellow when very thin; soon passing through a deep sherry-brown to a blood-red colour, then a deep port-wine colour, and then becoming opaque.

These crystals reflect a deep steel-blue colour when analysed with a Nichol's prism, and generally across the short diameter of the prism, which is the analogue of the a-prism of the quinine salt. The cinchonine salt possesses doubly absorbent powers, much more powerfully so than the quinidin salt, but inferior to all the others; the body-colour is deep sienna or bistre-brown.

This salt furnished the following analytical results:—

	I.	II.	III.
Iodine	50·34	50·587	50·302
Sulphuric acid	5·247	5·217	..
Carbon	28·156	27·57	27·37
Hydrogen	3·523	3·485	3·454
Nitrogen	3·306	..	..

which lead to the following composition:—

				Theory.	Experiment.
35 Carbon	× 6	=	210	= 27·7410	27·698
26 Hydrogen	× 1	=	26	= 3·4346	3·487
2 Nitrogen	× 14	=	28	= 3·7000	3·306
9 Oxygen	× 8	=	72	= 9·5103	9·8674
3 Iodine	× 127	=	381	= 50·3301	50·4096
1 Sulph. acid		=	40	= 5·2840	5·232
			757	100·000	100·000

giving—

$$\left.\begin{matrix} C^{35}\,H^{19}\,N^2\,O^2 \\ I^3 \end{matrix}\right\} 1SO^3\,HO+6HO=757,$$

which, on comparison with the quinidin salt, will be found to possess 1 atom additional iodine, and 1 atom more water, but a deficiency of 2 atoms oxygen, the latter, apparently, in consequence of the original difference in the type of the alkaloids employed; and, like that salt, it differs in its organic base by the loss of $C^5 H^5$ from the constitution of the alkaloid originally employed, if we take the formula $C^{40} H^{24} N^2 O^2$, as given by Gerhardt, for that of cinchonine.

The cinchonine and quinidin salts further agree in containing only 1 atom sulphuric acid, whereas the quinine and cinchonidin salts contain 2 atoms.

These investigations appear to show that the alkaloids in each instance undergo some modification, but not analogous to substitution; it appears more like a splitting-up into different molecular groups, and a rearrangement of these amongst themselves, as the formulæ of the organic bases differ much from those of the original alkaloids.

All these iodo-salts possess double refractive properties.

When the acid sulphates of the mixed alkaloids, quinine, quinidin, cinchonine and cinchonidin, are dissolved in dilute spirit, and the temperature increased to 80° or 120°, treatment with tincture of iodine readily separates the quinine salt first.

Subsequent further treatment in the same manner produces the cinchonidin salt, more or less mixed with the quinine salt.

On still further treatment, the quinidin salt is formed with its well-marked characters.

The cinchonine salt is by far the most soluble in spirit; and when a large quantity of cinchonine exists, this compound will also appear along with the quinidin salt.

This test is a beautiful and ready method of proving the presence of cinchonidin in cinchonine, which would otherwise be considered pure, Brandes' test having shown the absence of quinine and quinidin. In the same way, this test is an easy method of detecting mixtures of quinine and quinidin, the optical characters of the two salts being so well marked, that no difficulties can exist in their discrimination.

It does not offer such facilities for the separation of quinine from cinchonidin; the two salts go down together, especially if large quantities of cinchonidin exist with mere traces of quinine.

For the success of this test, a small portion only is necessary: with quinine and quinidin $\frac{1}{200}$th part of a grain has furnished evidence of the two alkaloids; one grain would be abundant to detect all the alkaloids.

The foregoing method of examination has enabled the author to prove that the substance which Rosengarten, of Philadelphia, called quinidin, was really the cinchonidin of Pasteur, and the details of his cures of fever, therefore, by quinidin are rather to be ascribed to cinchonidin.

The cinchonidin of Wittstein, of Munich, is a totally different alkaloid, giving, with sulphuric acid and iodine, a salt at once to be distinguished by the eye from either of the two iodo-sulphates described, but yet possessing optically doubly absorbent powers. This salt has a deep orange-yellow colour by transmitted light, merging into sienna-brown in thicker plates, which are generally flat and much imbricated in the method of crystallization, and also derived from a rhombic prism. The reflected tints are brownish-olive, not unlike dead leaves, or brown beech-leaves. These crystals are more doubly absorbent than either the quinidin or cinchonine salt, but less powerfully optical as tourmalines than the quinine or cinchonidin compounds. When polarized, they transmit a sienna-brown body-colour if moderately thick, and thicker plates are bistre-brown, but when sufficiently thick, they are wholly impervious to plane-polarized light. The substance was not in sufficient quantity to admit of any analysis.

All the alkaloids were furnished in a most obliging manner by Mr. John Elliott Howard, to whom the author is deeply indebted for them, and thus publicly desires to express his acknowledgments; many of these various alkaloids having taken more than ordinary trouble in the preparation and purification.

It is well known that quinine and quinidin, under the continued effect of heat and dilute sulphuric acid, undergo a molecular change into quinicine, which M. Pasteur has asserted to be isomeric with the original alkaloids, but hitherto no complete analysis has been made of the metamorphosed alkaloids.

The author has produced an iodo-sulphate of quinicine, but it is no longer a crystalline compound; it presents itself as a deep blood-coloured resin, very soluble in spirit and readily precipitated by water

from its spirituous solution. This substance has not yet been submitted to analysis. During the production of the iodo-sulphate of quinidin a certain portion of the alkaloid becomes converted into quinicine, as may be demonstrated by the production of this resinous compound from the mother-liquid on the addition of further proportions of iodine.

Cinchonine and cinchonidin become converted into cinchonicine by similar treatment, and this amorphous uncrystalline alkaloid also forms a resinous iodo-sulphate; its colour is deep purple-black, and it deposits itself on spontaneous evaporation of the spirit, or on the cooling of a highly concentrated spirituous solution, in small drops, highly tenacious at 100° Fahr., but becoming solid at 60° Fahr. This compound has, in a fine state of division, a beautiful purplish-blue colour, and such a film generally forms around the edge of the vessel in which it is produced.

Cinchonicine appears to be one of the products during the manufacture of the iodo-sulphate of cinchonidin, but there is a much larger production of it during the formation of the cinchonine salt.

From the foregoing reactions, the author appears to be justified in asserting that eventually it will be found, when we know more of the rational grouping of the constituent atoms of the vegeto-alkaloids, that the construction of the formula for the cinchonidin of Pasteur will have a much greater similarity to the arrangement of the molecular groups of quinine than of cinchonine. And there is also great probability that the grouping of the atoms of cinchonine and the quinidin of Pasteur will be found to present more points of similarity; but in each case he sees no reason to doubt the existence of more oxygen in the cases of both quinine and quinidin than there is in cinchonine and cinchonidin. He also ventures to suspect that cinchonicine and quinicine will eventually be found to contain more carbon than the original alkaloid, the elements of water probably being separated by the sulphuric acid during the process of formation.

November 30, 1857.

ANNIVERSARY MEETING.

The LORD WROTTESLEY, President, in the Chair.

Mr. Gassiot reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including proceeds of the sale of £1500 Consols and a balance of £47 9*s.* due to the Society's Banker, amount to £4841 18*s.* 4*d.*; and that the total expenditure during the same period amounts to £4814 12*s.* 8*d.*, leaving a balance in the hands of the Treasurer of £27 5*s.* 8*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The President announced that Sir Philip de Malpas Grey Egerton, Bart., had been re-elected by the Council a Trustee of the Soane Museum.

List of Fellows deceased since the last Anniversary.

On the Home List.

Henry James Brooke, Esq.
Sir Charles Mansfield Clarke, Bart., M.D.
Very Rev. William Daniel Conybeare, Dean of Llandaff.
Right Hon. John Wilson Croker, LL.D.
John Disney, Esq., LL.D.
The Earl Fitzwilliam.
Marshall Hall, M.D.
James Holman, Lieut. R.N.
James Horne, Esq.
Thomas Best Jervis, Col. R.E.
James Adey Ogle, M.D.
John Ayrton Paris, M.D.
Rev. William Scoresby, D.D.
Joseph Smith, Esq.
Richard Twining, Esq.
Andrew Ure, M.D.
William Wood, Esq.

On the Foreign List.

Augustin Louis Cauchy.
Le Baron Louis Thénard.

Withdrawn from the Society.

Sir William Burnett, M.D.

List of Fellows elected since the last Anniversary.

Lionel Smith Beale, Esq.
George Boole, Esq.
George Bowdler Buckton, Esq.
Thomas Davidson, Esq.
George Grote, Esq.
Rowland Hill, Esq.
The Rev. Thomas P. Kirkman.
William Marcet, M.D.
John Marshall, Esq.
Andrew Smith, M.D.
Robert Angus Smith, Esq.
Charles Piazzi Smyth, Esq.
Henry Clifton Sorby, Esq.
John Welsh, Esq.
Joseph Whitworth, Esq.

The President then addressed the Society as follows :—

Gentlemen,

In considering what ought to form the subject of the Annual Address, it would naturally occur to any one, having the honour to hold the office of your President, that it might be desirable to mention in detail all the important discoveries and researches which had enriched the annals of science since the preceding anniversary ; and in addition, to describe any events which had occurred, or any specific measures which had been adopted, tending to improve the position of science or its cultivators. With respect however to the former, the range of subjects within the cognizance of this Society is so extensive, that the time ordinarily appropriated to the duty, in which I am now engaged, would be utterly inadequate to the purpose; and it is the less necessary, since it has become the annual custom of the Presidents or Councils of Scientific Associations, cultivating particular branches of science, to take a very extended view of the advance made in their own particular departments ; and in the case of the British Association especially, whose range of subjects is almost as extensive as our own, the Presidents of that Society have been in the habit of giving a very detailed statement of the progress of science during the past year. I may be permitted perhaps to refer for an example to the Address delivered at the last meeting of the British Association, as containing a most able and lucid statement of the kind to which I allude.

Under the other head to which reference has been made, one of the most important events has been the final adoption by your President and Council of the twelve resolutions, to which I took the liberty of referring on a former occasion. I then stated that we had taken

into consideration the important question, whether any measures could be adopted by the Government or Parliament that would improve the position of science in this country? and that, having elicited and duly weighed the opinions of all those most competent to give advice on the subject, we had finally adopted those resolutions and forwarded them to Lord Palmerston. This step was taken in the beginning of this year. The measures recommended by them relate, first, to education, and they are contained in Nos. 1 to 4 inclusive; secondly, there are others which have for their object the encouragement of scientific discovery and research, and these are Nos. 5 to 9 inclusive; and thirdly, Nos. 10 to 12 suggest measures, which, while they are calculated to confer great benefit on cultivators of science, are yet of still greater importance, when viewed in their bearings upon the interests of the nation at large. Although I shall have occasion to allude to some of the other recommendations, it is with respect only to the last-mentioned that I shall trouble you with any detailed observations.

Many here present will doubtless be able to recall to their recollection instances in which great mistakes have been committed for want either of access to competent scientific advice, or from reluctance to refer to it, when access was easy; but those who are familiar with the mode of conducting public affairs in this country, and at the same time duly appreciate the value of scientific research, are best able to estimate the extent of this evil; and to such persons it is distressing sometimes to witness the injudicious expenditure of large sums of money, while the same sums bestowed upon other and more generally beneficial objects, and under proper advice, might have been productive of very valuable results. But this is not all; for, side by side with this wasteful expenditure, we are compelled to witness the most rigid parsimony, where a little money timely bestowed upon the furtherance of some measure recommended by competent authority might have borne fruit a hundredfold.

Your President and Council have ventured to think, that, if the Government could be induced to consult some responsible body of scientific men, either one already existing, or to be specially constituted for that purpose, some of these evils, so far as they affect science at least, might be averted; and happy indeed would it be for those researches and investigations in which we all take so deep an

interest, if a small part only of the money thus saved to the nation should be expended on their promotion; and happy would it be for the nation, if such a check were provided against the improvident waste of the hardly-earned produce of its own industry.

The natural tendency of men is to undervalue what they cannot understand, and thus the abstruse speculations of the geometrician, and the recondite researches of the physicist, the object of which even is unintelligible to the many, are deemed of no value when weighed in the balance of public opinion against the splendid inventions which, after a long series of years, emanate from their labours. Often, when those who have scattered the precious seed are dead and forgotten, and their families are perhaps pining in penury, and calling in vain on an ungrateful country for a petty dole from the public purse, does the glorious harvest ripen, which they have sown. For this cause among many others, the opportunity of taking the opinions of such a Body, or such a Board, would be of the last importance to the best interests of society, for their approval would be a guarantee to the public of the expediency of any measure which they might recommend. It is impossible of course to suggest any innovation from which those who make the proposal could by possibility derive advantage, direct or indirect, without subjecting them to the charge of doing so from interested motives;—and yet a distinguished Member of the Government Grant Committee, from which these resolutions emanated, said early in the course of their discussion, "Let us ask for nothing for ourselves," and in this spirit were the resolutions framed. The chief object aimed at, was the encouragement of young men, competent to the task, who, with insufficient means at their command, were just entering upon a hopeful career of scientific research. You will therefore look in vain for any recommendations to bestow titles, medals, or other honours upon men whose scientific labours have long been before the world. But no amendment in our institutions could ever be carried out, if men were to be deterred by such considerations as I have adverted to, from fearlessly advocating that which in their conscience they believe to be fraught with advantage. Since the resolutions were transmitted to the Government, they have been moved for, laid on the table of both Houses of Parliament, and printed; whatever further steps may be taken to urge their adoption, I do not think that it would be right that this Society,

or its officers, as such, should take any part in them. It is enough that your Council have expressed their opinion; they must leave the result to time.

Since the last Anniversary we have been installed in the occupation of the building in which we are now assembled, and all the arrangements for our reception have been completed, except the painting of the Great Hall, which has been delayed by the dampness of the walls; nor have the requisite seats been yet supplied. I trust that the furniture of the rooms and the arrangement of the library in the main building have given satisfaction. I think credit is due to Mr. Weld our Assistant Secretary, and also to Mr. White, for the care and attention they have displayed in carrying into effect all our instructions.

It would appear from the Address of the noble Earl the President of the Society of Antiquaries, that some of that distinguished body are disposed to think we did not much regret the severance of the connexion which had so long and happily subsisted between the two Societies. Speaking for myself, and I have no doubt for many other members of the Royal Society, I may say that I should greatly rejoice, if by the liberality of Her Majesty's Government that ancient Society again found an abode in our own immediate neighbourhood—not however to the exclusion of any Scientific Society that had just claims to similar accommodation.

Several applications have been made to your President and Council requesting permission to use the Great Hall for various purposes. On the part of the Government it has been applied for, for the purpose of holding Competitive Examinations for admission into the Royal Academy of Woolwich, and likewise for the Examinations of Candidates for Commissions in the Army. These applications on the part of the War Department have always been framed with a courteous regard to our convenience, and they have been acceded to. Your Council were of opinion that the object sought to be obtained was one of great public importance, and that under the circumstances they should not be justified in withholding their consent. At the same time it must be admitted that frequent demands of this description might be productive of inconvenience, especially in reference to the access to the lower library. An application was also made by the Royal Geographical Society for the use of the Hall during

the ensuing session, and this request has also been willingly complied with. That Society meets but once a fortnight, and on a day which does not interfere with our own meetings. Nor can there be any difference of opinion as to the value of their labours in the promotion of science, if prosecuted, as there is every reason to hope they will be, with a special view to that object. Indeed there are few associations that might render more important aid in this behalf, than a Society instituted for the purpose of originating, organizing, and supporting extensive explorations of such portions of the earth's surface as have not hitherto been surveyed at all, or have been imperfectly examined. But in order that the operations of such a Society should be productive of all the benefit of which they are capable, care should be taken that no expedition be allowed to leave our shores without being supplied with proper instruments, and accompanied by those who are competent to make such scientific observations as the nature of the case will admit of; and lastly, provision should be made for the proper reduction and publication of such observations, so that the results may be rendered as extensively useful as possible.

On the occasion of sending out Lady Franklin's last expedition, every exertion was made by us to supply its able commander, Capt. M'Clintock, with a complete set of instruments, properly constructed and carefully compared, and with observers trained in their use. As to the former we were quite successful; but as to the latter, difficulties arose in reference to the employment of a particular officer, whose services the Admiralty were unwilling to dispense with. I am happy to be able to announce to you that I have had the pleasure of perusing a letter from Capt. M'Clintock, dated from the west coast of Greenland, expressing entire confidence in his brave companions and in his ship, and hopeful anticipations as to the future. Need I say more than that it was a letter written in the true spirit of a British Naval Officer, embarked in a momentous enterprise, conscious that many eyes and many hearts were anxiously tracking his steps over the icebound watery waste, and determined to put forth all his energies to achieve success in a noble cause.

That any one should entertain any doubt of the propriety of extending to the utmost our acquaintance with the globe we inhabit, seems extraordinary. That men should be found to undervalue the results of scientific research is not surprising, but that they should

overlook the other manifold blessings which accrue to mankind from successful undertakings of this description, could not be for a moment believed, if we had not evidence that persons do really exist, who look with indifference at such triumphs of human enterprise; men who might have joined in the outcry against Columbus, and have deprecated the expeditions of Dampier, Flinders, Cook, Parry and others. Had a majority of the so-called educated portion of mankind consisted of such men, we should perhaps now be without America and Australia, the human mind would have been miserably stunted in its growth, and civilization impeded in its progress. Such views are not likely to obtain much support at the present time; we may rest assured, for example, that Government will entertain with favour the proposal for exploring the river Zambesi, which has been suggested by the travels of that most persevering benefactor of mankind, the enterprising and distinguished Livingstone.

Though, as I have already said, it would be impossible to specify all the facts of interest which have been lately observed in the arena of physical research, there are some recent discoveries in magnetism which I should be unwilling to pass over in silence.

All who have attended to magnetical phenomena, are aware, that among the results of magnetic observations, and especially that extensive system of research, which was organized a few years ago at the earnest request of the illustrious traveller Humboldt, is to be reckoned the correct knowledge which we now possess of the regular periodical changes of the magnetic elements, depending on the hour of the day, the season of the year, and also on a period, somewhat exceeding ten of our years, which, when first discovered, appeared to have no analogue in any other known periodical phenomenon.

Besides these regular periodical effects, there are changes which occur suddenly at irregular intervals, and simultaneously in the most distant parts of the earth—

> ". . . . . though the main
> Roll its broad surge betwixt, and different stars
> Behold their wakeful motions,"

to use the words of a poet and a Fellow of the Royal Society, who, writing at a very early age, and above a hundred years ago, seems by an extraordinary accident to have described by anticipation one of

the most remarkable magnetic phenomena revealed to us by modern discoveries.

Now these sudden changes are characterized by very large deviations from the normal state. They are termed "Magnetic Storms." It is found, however, that these storms occur more frequently at certain hours of the day than at others, and are therefore themselves subject to periodical laws. These laws have been worked out by our able and persevering Treasurer, who has devoted, and with such distinguished success, so much of his valuable time to this species of research, and he finds that even these storms observe diurnal, annual, and decennial periods.

Several who are now present are aware how many years of laborious toil have been expended in procuring these striking results. In various places of our colonies and dependencies, at St. Helena, at Toronto, at Hobarton, for example, officers and non-commissioned officers of the Artillery, and officers of the Royal Navy were engaged hourly through the day and night in watching the vibrations and angles of small magnetic needles delicately suspended; a class of observations of a very tedious and exhausting description, and requiring great patience, perseverance and accuracy. Still, undeterred by all these difficulties, and often doubtless at great personal sacrifice, these true votaries of science continued their anxious vigils. As the observations were received by General Sabine, they were carefully reduced and coordinated at an establishment organized by him at Woolwich for that purpose.

But while this was proceeding, there was, unknown to our magnetic observers, another persevering enthusiast at work, whose labours were destined to have an important relation to theirs.

We must transport ourselves, in imagination, to the upper chamber of a modest dwelling in an obscure town in Germany, in which an old man has day by day for thirty long weary years been in the habit of observing the sun through two small telescopes, placed in a window overlooking the roofs of the houses. Every day, when the luminary was visible, did this persevering observer, Schwabe by name, survey its face, and note down the appearances of its disk. In this way he obtained a faithful record of the configuration of the spots during the whole of this long period. Science presents few examples of such unwearied concentration of attention on one object. Often,

no doubt, the friends, and perhaps the relatives of the observer, have presumed to ridicule his devotion to what seemed to them a trivial pursuit; but the result of all this labour was the discovery of what had escaped the attention of all astronomers for two hundred years, at least had been before only very faintly suspected, viz. that the spots on the sun's disk pass through the phases of maximum and minimum frequency in a period of about ten years. But, strange to say, it was then discovered, and first announced in our Transactions by General Sabine the discoverer, that the decennial *magnetic* period coincides, both in its duration and in its epochs of maximum and minimum, with the decennial period observed by Schwabe in the solar spots; that is, the period of maximum variation with that of the maximum frequency of spots, and so *vice versâ* the minimum. Hence it is clear that the sun exercises some influence on the earth's magnetism dependent on the existing state of its own luminous atmosphere. Surely this is another remarkable instance that fact is often more strange than fiction. How little could the secluded observer, watching the quiverings of the needle in the remote regions of Australia, or on the shores of the Polar Sea, dream either that his labours would derive additional interest from the unconscious cooperation of a German astronomer, or that he was himself engaged in watching vibrations which were oscillating in harmony with the motions of a gaseous substance ninety-five millions of miles away! This is again another example added to the many other illustrations of the same truth, that scientific researches, if skilfully and perseveringly continued, often lead to most valuable results, which could not have been anticipated *à priori*; and this should surely be as great an incentive to toil as it is a glorious reward of discovery.

But this is not all: it has been found also that our less obtrusive satellite, that the mild influence of the moon affects the magnetic needle; the declination, the inclination, and the magnetic force, all undergo a small variation, dependent on the lunar hour angle. A delicately suspended magnet is truly the most submissive of all slaves, affected by the sun, and the moon, and the earth, and the sport of every electric or magnetic current that approaches it.

"Whate'er the line
Which one possessed, nor pause nor quiet knew
The sure associate, ere, with trembling speed,
He found its path, and fixed unerring there."

Before leaving the subject of magnetism, I would mention another result lately obtained in that department of science, which we owe to one of those voyages to the Arctic Seas, which some are wont to describe as useless to mankind, and a foolish exposure of human life.

Capt. Maguire commanded the Plover, one of the ships employed in the Franklin search in the years 1852–3–4, and he was stationed from the summer of 1852 to that of 1854 at Point Barrow, the most northern cape of the American Continent, between Behring's Strait and the Mackenzie River. During every hour of seventeen months unremittingly did Capt. Maguire and his officers observe and record the variations of the magnetic declination and the concomitant auroral phenomena, in an observatory built of ice and lined with seal-skins; in a most dreary and unenviable locality indeed, but still one of the most favourable for such investigations. The observations, as usual, were reduced under the direction of our Treasurer, and they reveal this important fact:—On comparing them with those made at Toronto, it was found, that although the deflections of the same name at the two stations did not correspond, there existed, on the other hand, a very striking and remarkable correspondence between the easterly disturbances at Point Barrow and the westerly at Toronto, and *vice versâ*. In the case of the regular solar-diurnal variation, the easterly and westerly extremes are reached at both stations at nearly the same hours; but in the case of the abnormal diurnal variation, the progression is reversed, the easterly extreme at the one coinciding very nearly with the westerly at the other. The absolute disturbing force appears to be much greater at Point Barrow than at Toronto, and in correspondence therewith is the frequency of the auroral manifestations. During the six months of darkness in the two successive winters, out of 3625 hourly observations, there were 1077, or 30 per cent., at which the aurora was visible. It appeared, also, that 1 A.M. was the hour at which aurora most frequently appeared, and that between 11 A.M. and 3 P.M. is the period of minimum frequency.

The above interesting results have suggested to the British Association the importance of instituting magnetic observations for another winter or two at some point, so situated between the abovementioned two stations, as to render it a proper locality for ascertaining the laws on which the remarkable and characteristic analogies

and differences in the results derived from each depend. There is no place better fitted for this purpose than the mouth of Mackenzie's River. I cannot but express my earnest desire that Her Majesty's Government will also consent to send out one vessel for *this* purpose, as it will be attended with little or no risk, and a most important scientific object will be attained. Moreover, it will be a great satisfaction to the friends of Capt. M'Clintock and his gallant crew to know that there is a vessel stationed at such a spot, should any unlooked-for misfortune befall an expedition, which, thanks to the devotion of a bereaved widow to her heroic husband, has cost the country nothing but the expenditure of a few hitherto unemployed stores.

The Meteorological Department of the Board of Trade, under the able superintendence of Admiral FitzRoy, bids fair to realize all the hopeful anticipations which were formed of the beneficial effects which such an institution was calculated to produce in improving Navigation and the science of Meteorology. Agents have now been established at the principal ports for the supply of instruments properly verified, books and instructions. More than two hundred ships have been so supplied, and more than one hundred logs have been forwarded, which are in process of coordination and reduction. Valuable materials are thus continually accumulating, and to such a degree, that the only difficulties of the Office arise from the overwhelming extent of the communications which are daily pouring in,—overwhelming, that is to say, when viewed in reference to the Staff available for reducing them.

Meanwhile other Governments have not been slow to avail themselves of the advantages to be derived from Lieut. Maury's Sailing Directions and Charts, and the measures which he recommends. The Officer at the head of the Meteorological Department of the Dutch Marine states, that by their means the voyages to Batavia have been shortened one-eighth. An Officer of the French Navy is also engaged in condensing and translating Maury's works for the use of the French Marine; and a French Department of Oceanic Meteorology is about to be formed. Other nations have followed in the same track, and, on the whole, voyages have been already reduced to from a tenth to a quarter of their former periods; a result to be attributed in part doubtless to the improvement in ship-

building, to which art Science has lately rendered important aid, but principally to the cause in question.

A portion of the success of this Meteorological Department of the Board of Trade is certainly due to the facilities afforded by that admirable institution, the Observatory at Kew, for the verification of the instruments supplied. I have already had occasion to acknowledge the obligations which Science is under to the same establishment for supplying magnetic and meteorological instruments, duly verified, to various scientific expeditions sent out both by this and other countries. Our debt of gratitude to this Observatory and its able Superintendent, Mr. Welsh, is likely to be shortly still further augmented by valuable improvements in the photographic record of various phenomena, and among others, of the spots on the sun's disk; for the observation of which a telescope, by Ross, has been lately erected. Surely it is a cause of great congratulation that, by means of the fund, the income of which was placed at their disposal by the liberality of Dr. Wollaston and others, your Council were able to render essential aid, and at the time of its greatest need, to an Institution whose efficient maintenance has become a European necessity,

In taking a general view of the proceedings of the Institutions, both in this and other countries, whose common object may be said to be the increase of the happiness of our species, though by various and widely different methods, one cannot but be struck, first by the great number of such establishments and the inadequacy of the results obtained, if considered in reference to the means provided; and, secondly, by the fact, that in the same country and at the same time many different bodies are at work in striving to produce the same end; but, instead of combining their forces, they often interfere with each other's operations; instead of helping one another, they are perhaps even disposed sometimes to view with jealousy the success of their rivals. Thus, to take an instance, let us consider how many different bodies of men, official or otherwise, are now employed in attempting, but hitherto with little success, to diffuse more widely among the inhabitants of this favoured island the blessings of education, including a knowledge of the elements of the physical sciences. Let us reflect for a moment what might be the result of so many different currents of intellectual force,

if, instead of crossing and jostling one another in various directions, they were to be forced to flow in one harmonious channel. Surely, when so much is at stake, and the prize to be obtained is human happiness, some attempt at combination might be made, some effort to bring together a portion of the divergent elements. The Scientific Board, above suggested, might do some good; Government or Parliament might do more; but it is to social progress and the general improvement of our species, that we must look for the complete realization of the hopes which every good man is sometimes led to form of a happy future in store for his race; and obliged, as we are, in our course through life, to witness so much that we have great cause to regret, and scarcely any power to mend, this reflection is perhaps one of our brightest and purest consolations.

The Copley Medal has been awarded to M. Chevreul for his important investigations in Organic Chemistry, particularly for his researches upon the nature and composition of the fats and fixed oils, and for his physical investigations on the processes of dyeing, including those on the simultaneous contrast of colours.

Previous to the researches of M. Chevreul upon the fats, it was supposed that saponaceous compounds were combinations of the alkalies, and of oxide of lead with oils and fats; and, with the exception of Scheele's discovery, that glycerine was set free during the process of saponification, no precise chemical knowledge upon this subject existed.

The attention of M. Chevreul appears to have been directed to this matter by his observation that, on diluting a solution of a particular soap largely with water, a crystalline substance was separated in pearly scales; upon examining the nature of this crystalline compound he found it to consist of a combination of alkali with a peculiar fatty body of distinctly acid character. Starting from this point, he was induced to examine the subject of saponification *in extenso*; the result of his inquiries was the discovery of a large number of new compounds, and a masterly elucidation of the chemical history of the fixed oils, fats, soaps, and plasters.

M. Chevreul showed that ordinary fats and fixed oils consisted

mainly of three distinct compounds united in very various proportions. One of these compounds, *oleine*, is liquid at ordinary temperatures, the other two, *margarine* and *stearine*, are solid; and, according as the liquid or solid constituents preponderate, the body assumes the consistence of oil, or of a fat less or more hard. He also showed that each of these three principles contains a distinct fatty acid united with the basis of glycerine, the sweet principle of oils; that, when the fat or oil is acted on by alkalies or other metallic oxides, the basis of the glycerine is displaced, combining with water at the moment of its separation, thus forming true glycerine. The fatty or oily acid in the meantime unites with the alkali or metallic oxide: when the alkalies are used to effect the saponification, soluble soaps are found; when metallic oxides, such as oxide of zinc, or oxide of lead, are employed as the saponifying agents, a plaster, or insoluble metallic soap, is formed.

But the discoveries of M. Chevreul were of far higher value in a scientific point of view than the mere establishment of facts, or the discovery of new bodies of importance in the arts. The methods of research which he introduced, laid the foundation of future inquiries, and may be said to have enabled Organic Chemistry to become what it now is.

At the time when the researches of M. Chevreul were commenced, no trustworthy process of ultimate organic analysis existed. A method capable of furnishing results, which, for accuracy, though not for rapidity and facility of execution, challenges comparison with those at present in use, was by him devised and applied. But a still more important aid to these inquiries was supplied. M. Chevreul was the first to perceive that the numerical results obtained by ultimate analysis of an organic compound were not the only data necessary to fix its true composition, and he first pointed out the importance of an extended study of the changes produced in each compound by the action of reagents.

The merit of recognizing and systematically introducing this leading principle into the methods of research practised in Organic Chemistry is due to M. Chevreul; and the important services which he has thus rendered to the progress of Organic Chemistry are felt more and more in each succeeding year. Like other great men, M. Chevreul was in advance of his age, and a few years elapsed

before his contemporaries justly estimated and began to apply the principles which he had introduced.

But there is another side from which these researches must be viewed. Few investigations in Organic Chemistry have been fraught with more important consequences to the industrial arts than these which we are now considering. New methods of obtaining hard and valuable fats from oils of low price have been introduced, and the method of manufacture adopted for the better descriptions of candles has been entirely altered. A new branch of industry has indeed been created by these researches, since it is to them that we owe the introduction of what are known as stearine or composite candles.

The second important subject with which the name of M. Chevreul is permanently connected, is that of the contrast of Colours. His investigation upon this subject has, like his earlier one upon the fats, been regarded by all subsequent inquirers as the classical work upon the series of phenomena to which it relates. In duly appreciating the importance of these inquiries, it is necessary to consider both their purely scientific value and their direct practical bearing upon the art of the silk-dyer and the calico-printer, to whom they have afforded invaluable assistance, by the establishment of fixed rules to guide them in the selection of suitable and harmoniously contrasted tints, upon which so much of the beauty of their different fabrics depends.

Professor Miller,

Transmit this Medal to M. Chevreul, as the best proof which we can give of the value which we attach to his discoveries; and express to him the gratification with which we have learnt that he is still engaged in the prosecution of scientific labours; and our hope that his life will be prolonged to enable him to continue them, and enjoy the honour he has so deservedly obtained.

One of the Royal Medals has been awarded to Dr. Frankland.

In the year 1846, the question of the constitution of the alcohols and certain allied organic bodies excited the deepest attention among chemists. Dr. Kolbe had succeeded, by a most ingenious electrolytic process, in isolating the hitherto hypothetical radicals, valyl and methyl; but the indirect nature of the reactions, by which these radi-

cals were separated from valerianic and acetic acid, failed to convince a large number of contemporary chemists, who had contended for some years against the existence of such radicals. Early in 1848, Dr. Frankland commenced his attempts to isolate these bodies by direct and unexceptionable reactions. Taking the isolation of the elementary radical, hydrogen, by the action of zinc upon hydriodic acid, as his type, he succeeded, in the laboratory of Professor Bunsen at Marburg, in isolating from their iodides the compound radicals methyl, ethyl, and amyl; thus establishing the complete homology of these substances with hydrogen.

The subsequent prosecution of these researches also led Dr. Frankland to the discovery of a new series of remarkable organic compounds, containing the metals zinc, tin, and mercury, united with the radicals before mentioned. These metals had not been previously known to enter into combinations of that class. The compounds of zinc with methyl and ethyl have, moreover, proved most valuable bodies in the hands of Frankland, Hofmann, and others, for effecting substitutions which had previously been impracticable.

The laborious and masterly researches of Bunsen on cacodyl had rendered this most remarkable substance an object of the highest interest to chemists; nevertheless, its isolated position, and the complicated reactions attending its formation, stood in the way of any satisfactory conclusion as to its rational constitution. The study of the organo-metallic bodies containing zinc, tin, and mercury, and of those discovered by Löwig and others, led Dr. Frankland to a generalization, which grouped cacodyl and the rest of those organo-metallic compounds into a harmonious family, and according to which these substances are formed upon the types of the inorganic compounds of the respective metals with oxygen, sulphur, and other similar bodies.

In the hands of Dr. Frankland, this theory has already borne fruit. Guided by it, he has succeeded in replacing the oxygen of binoxide of nitrogen, by ethyl and methyl, thus obtaining two members of a new series of acids; while the formation, by one of his pupils, of two new organic derivations from sulphurous acid, is another result of the same important generalization.

Dr. Frankland,

Accept this Medal as the appropriate reward of researches, which have excited the greatest interest among all those who cultivate or appreciate the science which you have so materially advanced and adorned.

The other Royal Medal has been awarded to Dr. Lindley, in recognition of the value of his labours in various branches of Scientific Botany; and more especially for his learned and comprehensive works on the Natural Orders of Plants, on Orchideæ, and on theoretical and practical Horticulture.

The eminent position which Dr. Lindley has attained amongst naturalists, is founded upon a knowledge of the Vegetable Kingdom, in the most extended sense of that term, such as few have acquired; the result of many years of diligent research and continued study of the structure, morphology, and development of plants from every part of the globe. To the knowledge thus obtained, he has applied the resources of an original and vigorous intellect, and a quick appreciation of affinities; and he has embodied the results of his labours in a series of works upon the affinities of plants, which are alike remarkable for the clearness of their arrangement, the lucidity of their style, and the influence they have had upon the progress of philosophical Botany. In these labours we recognize with satisfaction the evidence of his regarding systematic Botany in its true light, as the sister science of the Comparative Anatomy of animals; and, like it, depending for the value of its results upon the number and variety, as well as the completeness and accuracy of the naturalist's observations, and upon his powers of combination.

In the systematic investigation of the Orchideous plants, Dr. Lindley has devoted himself during a long series of years to one of the most difficult branches of Descriptive Botany. The species of this remarkable and extensive natural order are of a singularly complicated structure—the clue to their affinities lies in minute organs; and as by far the greater number of species are only to be obtained for study in a dried and mutilated state, their investigation demands an amount of patience and skill in microscopic analysis, such as very few botanists have devoted to similar inquiries.

In the science of Horticulture Dr. Lindley has also taken the highest position; nor is it too much to say, that it is mainly due to his efforts that this branch of knowledge has risen from the condition of an empirical art to that of a developed science. In no department have his labours been more widely appreciated than in this; not only because, up to a very recent period, the ignorance of sound principles and the prevalence of injudicious practice retarded the progress of scientific horticulture, but because these evils could only be remedied by one combining that knowledge of scientific Botany and of gardening operations which he possesses. To these requirements he has added the power of studying the results of experimental research by means of those laws of climate in its relations to horticulture, which had previously been so ably expounded by our late illustrious Fellow, Professor Daniell. In pursuing these inquiries, Dr. Lindley has displayed the same ready facility in applying his varied knowledge to the investigation of difficult problems, which characterizes his works on the affinities of plants.

I may also briefly allude to Dr. Lindley's numerous elementary and other treatises on Botany, and on its applications to Medicine and the Arts; to the long series of illustrated works he has published; to his researches in the Fossil Flora of Great Britain; and to the cordial reception of so many of his writings on the Continent, as is evidenced by the translation of several of them into other European languages. In this award, the Council of the Royal Society further wish to mark their recognition of the great and beneficial influence of Dr. Lindley's labours, both as an author and Professor, in introducing and diffusing a knowledge of the natural system of Botany, and of Vegetable Anatomy and Physiology, into the Schools and Universities of this country.

Dr. Lindley,

Accept this Medal in token of our appreciation of the labours of a whole life devoted to the successful cultivation and the extension of our knowledge of the sciences of Botany, Vegetable Physiology, and Horticulture.

Obituary Notices of deceased Fellows.

Henry James Brooke was born at Exeter on the 25th of May, 1771. His relations were engaged in the manufacture of broad-cloth. After having received an ordinary scholastic education, he studied for the bar, and had very nearly completed the usual period, when the prospect of advantageous connexions with the manufacturing firms in the west of England induced him to engage in the Spanish wool trade in London, for which object he spent nearly two years in Spain.

The precise habit of thought and expression which the active study of the law must necessarily induce, was perhaps mainly instrumental in imparting the tone of extreme precision by which all his subsequent acts and observations were characterized.

Soon after he took up his residence in London, in the year 1802, his attention was turned to the subjects of Mineralogy, Geology, and Botany; and to the two former of these sciences, then in their infancy, the greater portion of his leisure hours was devoted. He was elected a Fellow of the Geological Society in 1815, of the Linnean in 1818, and of the Royal Society in 1819. He served on the Council of the Royal Society in 1842–44.

Mr. Brooke was associated with the late Mr. Henry Hase, cashier of the Bank of England, and others in the establishment of the London Life Assurance Association, the commercial success of which bears ample testimony to the soundness of the principles on which it was established.

On the decline of the Spanish wool trade, which was superseded in a great measure by that with Germany, Mr. Brooke sought a commercial pursuit more congenial to his tastes, and devoted his energies to the establishment of companies to work the mines of South America; but in these undertakings the fairest prospects were blighted by an entire absence of good faith abroad, and failure was the inevitable result. After this period he accepted the office of secretary to the London Life Association, the duties of which he discharged for many years; and on his retirement, the appreciation of his services by the Society was evinced by the grant of a liberal annuity.

During a period of several years, his devotion to his favourite pursuits was much interfered with by the result of an accident:—he

was knocked down by a horse suddenly turning the corner of a road near his residence at Stockwell, and the fall produced a slight concussion of the brain; after which, for a considerable period, his accustomed mental efforts were followed by sleeplessness and other symptoms of undue cerebral excitement. During this period, finding absolute inaction extremely irksome, he sought pursuits which would occupy his hands, with less demand on the brain than those to which he had been devoted. He formed a large collection of shells, but feeling the pursuit to be objectless, if irrespective of the structure and functions of their living tenants, he abandoned it, and presented the collection to the University of Cambridge. Mr. Brooke then became a collector of engravings,—having in early life imbibed a taste for art, and exercised that of water-colour drawing. These he was so successful in cleaning and restoring, that when, having so far recovered as to resume his original pursuits, he disposed of his collection, the aggregate value was greatly augmented, notwithstanding the presentation of some specimens of rare excellence to the national collection in the British Museum.

Having been blessed to the last with an unusually perfect enjoyment of his faculties, his favourite studies were actively pursued until a very short period before his decease, which occurred from natural decay, accelerated by the depression of the system produced by a severe cold, on the 26th of June, 1857.

The 'Familiar Introduction to Crystallography,' the first systematic treatise on this branch of science, was published in 1823. In this, following the steps of Haüy, he referred the existing forms of crystals to an unnecessarily large number of *primary* forms; but the trigonometrical relations of the various existing plane surfaces of crystals were then first clearly traced out.

In the subsequent treatise on Crystallography published in the Encyclopædia Metropolitana, the former system was much simplified and the number of primary forms reduced to six, which, differing essentially from each other, correspond with the six systems generally adopted by continental crystallographers.

The discovery and description of thirteen new mineral species are due to Mr. Brooke's researches: to these may be added two others, the published descriptions of which were just anticipated, in point of time, by those of continental mineralogists. These notices will be

found in the pages of the Philosophical Magazine and Annals, and of the Edinburgh Philosophical Journal.

He was the first to make extensive use of the reflective goniometer in determining the forms of the crystals of artificial salts. The Annals of Philosophy for 1823 contain the determination of the forms of no less than fifty-five different laboratory crystals,—a work of much persevering labour. If only the chemical composition of these salts had been accurately known in England at the time, their measures would have served as a basis whereon to found the theory of Isomorphism.

The treatise on Mineralogy in the Encyclopædia Metropolitana was the first systematic work on the subject with which the name of Mr. Brooke is associated. This was originally intended to have been a very complete treatise, but repeated editorial remonstrances on account of want of space compelled our author to cut it down to little more than a mere catalogue of minerals, with a few of their more important chemical characters. The only complete treatise on Mineralogy with which his name is connected is the recent re-edition or rather reproduction of W. Phillips's treatise, in conjunction with Professor W. H. Miller, who took upon himself by far the greater portion of the labour incidental to publication.

It may be here remarked that Mr. Brooke entertained a strong impression of the desirableness of rendering the study of crystallography more attainable to many, whose minds are not so habituated to the abstractions of analysis as to contemplate a plane merely as the geometrical impersonation of $ax+by+cz=0$: this object he proposed to attain by means of a more direct reference of the existing planes of crystals to simple geometrical, or *primary*, forms than the last-mentioned treatise presents.

Mr. Brooke's latest efforts were directed to the general relations and geometrical similarity of all crystals belonging to the same system. A paper on this subject, read before the Royal Society, which was in the press at the time of his decease, contains a comparison of the forms of all known minerals belonging to the Rhombohedral and Pyramidal systems, and will probably be found to throw some new light on the theory of Isomorphism.

His unrivalled collection of minerals, comprising the choicest specimens that he could, with ample opportunities, collect during half a

century, has been presented to the University of Cambridge, as the best means of rendering it subservient to the advancement of mineralogical science.

M. Augustin Cauchy* had the good fortune to belong to that middle class of society which is neither exposed to the miseries of poverty nor to the temptations of wealth. His father was Archiviste-Secrétaire of the Sénat Conservateur from about 1800 or 1801, and of the Chamber of Peers from 1814 to 1830. Of two brothers, both younger than himself, one became an ornament to the highest court of justice, to which he was promoted, and the other succeeded his father as Secrétaire-Archiviste to the Chamber of Peers. Augustin Cauchy was born on the 21st of August, 1789. His classical education commenced early under his father, and was continued afterwards by able teachers at the École centrale du Panthéon. He left this school in 1804, at the age of fifteen, carrying off the second prize for Latin composition, and the first for Greek and Latin verse. This success procured for him the wreath given to the best classic among the pupils of the École centrale. After having attended for one year only the public mathematical lectures of an excellent Professor, Dinet, Cauchy felt himself qualified to enter the examination of candidates for admission to the École Polytechnique. He was admitted, being second on the list, in 1805, at the age of sixteen years; and at the end of the two years' course, he came out third in 1807. On quitting the school he adopted the career of the Ponts et Chaussées, in which he passed rapidly through the inferior grades, was employed in many works, and became ingénieur en chef in 1825.

On the 6th of May, 1811, at the age of twenty-two years, he presented to the mathematical class of the Institute, a very remarkable memoir on the polyhedron of geometry, and completed the theory of a new kind of regular polyhedrons discovered by M. Poinsot. Legendre, a most austere judge, regarded this memoir as the production of well-exercised powers which promised in due time the highest success. He urged the young author to follow out these researches, and to endeavour to establish a certain theo-

* This notice is extracted principally from the Letter of M. Biot to M. de Falloux.

rem not previously demonstrated. Cauchy obtained it in 1812. Legendre reported on it to the Academy with an enthusiasm very foreign to his character. "We only intended," he said, "to give an idea of this demonstration, and have extracted almost the whole of it. We have thus furnished a new proof of the sagacity with which this young geometer has succeeded in conquering a difficulty which had arrested the progress of the masters of the art, and which it was of importance to solve in order to complete the theory of the solid bodies." These first two memoirs of Cauchy seemed to foretell a peculiar and exclusive aptitude for pure geometry; but it was soon discovered that his genius had a much wider range. In the years 1813 and 1814 he produced two remarkable analytical memoirs, and in 1815 he presented a memoir on the theory of numbers, in which he proved and extended a theorem enunciated by Fermat, a theorem some particular cases of which only had been established by the most able writers in that department of mathematical science, Legendre and Gauss. He published an elegant theorem on the number of values which a function can assume, when the letters which it contains are interchanged. Twenty years later, this theorem enabled the celebrated Abel to prove the impossibility of solving algebraic equations of the fifth or higher degrees. In the same year, the Academy proposed, as the subject of the great mathematical prize, the investigation of the theory of the propagation of waves on the surface of a heavy fluid of indefinite depth. Cauchy gave a complete solution of the problem. His memoir, which obtained the prize in 1816, has for its motto the line of Virgil—

> "Nôsse quot Ionii veniant ad littora fluctus."—Georg. ii.

A peculiarly happy quotation, as the line may be said to contain a striking enunciation of the problem proposed.

This fertility in a young man of seven-and-twenty would have secured for him the first place which became vacant in the Mathematical Section of the Institute. He was admitted into it under circumstances much to be regretted. After the short crisis of a hundred days, a royal ordinance, dated March 21, 1816, re-established the old Academies under their original names,—the Academy of France, of Sciences, of Inscriptions and Belles Lettres, of the

Fine Arts,—and also appointed the members of the restored Academies. In the Academy of Sciences, two celebrated names, those of Carnot and Monge, were replaced by two new names, Breguet and Cauchy. The opinion of men of science was indulgent towards Breguet, but severe towards Cauchy.

Towards the end of 1815 he was appointed Assistant-Professor of Analysis at the École Polytechnique; he became titular Professor in 1816. It was impossible for any man to be more zealous than Cauchy in discharging the duties imposed upon him. Appointed to teach, he turned all his thoughts to the art of teaching. Between 1816 and 1826 he published his Course of Algebraic Analysis, of Differential Calculus, of the application of Infinitesimal Analysis to the Theory of Curves; three excellent works, well arranged, proceeding by vigorous demonstrations and rich in new details, leaving nothing to be desired except perhaps a little condescension in explaining the abstractions of analysis by geometrical considerations. In the same interval he published a memoir on integrals taken between imaginary limits, which has been the foundation of important investigations for many of our young geometers. But even this was not sufficient for his indefatigable ardour; he undertook and commenced publishing, in 1826, a kind of periodical review of his own, entitled 'Exercices Mathématiques,' in which every department of mathematics, the most elementary as well as the highest, was handled with so much generality, fertility and inventive power, that on reading this publication, Abel, one of the most profound analysts of our times, wrote to one of his friends, "Cauchy is, of all others, the geometer who best understands how mathematics ought to be studied." In fact, the discoveries of methods and the sketches of new views, scattered through these 'Exercises,' have been not only to the author, but also to many other geometers, the fertile initiative of brilliant researches. Cauchy continued the nurture and publication of this mathematical treasury up to the time of his death.

The calm flow of his existence was unexpectedly disturbed by the Revolution of 1830. At this epoch he was married and the father of two daughters. He had allied himself with an honourable family, whose social position, tastes and sentiments were in harmony with his own. Besides his Professorship at the École Polytechnique, he

filled a chair in the Faculté des Sciences de Paris, and was Assistant-Professor of Mathematics applied to Physics, at the Collége de France. The new government thought proper to establish its title to power *de facto* by an oath of allegiance imposed upon all public functionaries, even on those who had no duty beyond that of teaching the mathematical and physical sciences. Cauchy took refuge in Switzerland in order to preserve his loyalty to Charles X. unimpeached. The presence of so distinguished a geometer in the country of the Bernoullis and of Euler could not remain long concealed. The king of Sardinia, informed of his voluntary exile, created for him a chair of mathematics at Turin, the duties of which Cauchy discharged with *éclat*, pursuing at the same time his other researches. Thus France lost one of her most illustrious geometers, and one of the most able of her Professors. In 1832 Cauchy was elected a Foreign Member of the Royal Society. In the same year he was invited to Prague by Charles X., to take a part in the education of the Count de Chambord. He sent for his wife and two daughters, and with them followed the princes to Goritz; and during the six years devoted to this honourable employment found leisure to write a multitude of valuable memoirs on various parts of mathematics, which, scattered throughout Germany, are not easy to obtain. He took leave of his pupil in 1837, returned to France, and resumed his place in the Institute, which, contrary to rule, had been left vacant,—protected by the admiration which the genius of its possessor inspired. From this period, his studies being no longer disturbed by the duty of teaching, his mathematical labours being never interrupted except when engaged in works of charity, Cauchy poured forth at the meetings of the Institute the inexhaustible abundance of his mathematical genius. During the last nineteen years of his life, he composed and published in the volumes of the Academy or in the 'Comptes Rendus,' more than 500 memoirs, besides a multitude of reports on memoirs presented by others. Of this immense mass of labour, many parts have a great value of their own; others present the initiative of ideas and of methods, which have been or will at a later time be fertile. All bear upon the highest departments of mathematics, the perfection and extension of pure analysis, the investigation and direct determination of the planetary movements and of their most complicated inequalities, the theory of the undulatory

movement of light, considered in its utmost generality. Unfortunately, this haste in production did not leave him patience to bring his works to maturity. Each new way that presented itself to his mind occupied him exclusively; and in order to follow it he quitted that which he had begun to explore, even without taking time to see to what it would lead. For the sake of proceeding more rapidly, he almost always condensed his new researches in an unusual notation, which rendered them unintelligible to everybody but himself; and often he did not discover that these innovations only disguised under some strange form results already known.

In 1840 a place in the Bureau des Longitudes became vacant by the death of Poisson. The members of this body are renewed by election, subject to the approbation of the Chief of the State. Cauchy was unanimously elected, but declined to take the oath of allegiance to the government of Louis Philippe, consequently his election was not ratified. In 1843 Cauchy was commissioned by the Academy to verify the determination of an inequality of long period in the planetary motions. M. Leverrier announced the discovery, in the motion of the planet Pallas having a period of 795 years. Its maximum effect upon the longitude of Pallas exceeds $15'$, according to the calculations of M. Leverrier. For want of a direct analytical method, he had determined its amount by an extremely bold numerical interpolation which required an immense amount of calculation. In order to avoid the trouble of verifying it, Cauchy invented a direct analytical method, by which all inequalities of this kind can be determined in every case. He obtained the same coefficient as that found by M. Leverrier, and from that time, in problems of this kind, the power of abstract science replaced individual exertion. In 1848 Cauchy resumed the Mathematical Professorship in the Faculté des Sciences de Paris, the only one of his former posts which remained unoccupied. In 1851 Cauchy resigned this Professorship for the second time, but soon afterwards, the Minister of Public Instruction, M. Fortoul, easily obtained permission from the Emperor for Cauchy to resume his Chair unfettered by any condition or political test. He expressed his gratitude for this indulgence by devoting the whole of the income he received from the Faculté des Sciences to charitable purposes in the little Commune of Sceaux, where he resided. Once, when the

Maire, who was the dispenser of his charities, expressed some astonishment on seeing him so prodigal, he exclaimed, "Be not alarmed, it is the Emperor who pays."

Cauchy's determination of the number of real and imaginary roots of any algebraic equation; his rigorous method of calculating approximately the same roots; his new theory of the symmetric functions of the coefficients of equations of any degree whatsoever; his *à-priori* valuation of a quantity less than the least difference between the roots of an equation; his mathematical theory of light, and especially of dispersion; his *à-priori* determination, without any previous photometric observations, without any data besides two angles, of the quantity of light reflected at the surfaces of metals,—have placed him among the number of the truly creative minds, and have made him the illustrious chief of a new mathematical school, much superior in its aims to the school of Laplace his master, or that of his rival, Poisson.

A classical education had developed his natural aptitude for the study of languages. At Turin he lectured in Italian; at the age of fifty-three he learned Hebrew, that he might assist his father in some scriptural researches in which he was engaged.

In the sitting of the Institute on the 4th of May, 1857, M. Cauchy read a second memoir on the employment in astronomy of coefficient regulators, an employment which constitutes an artifice in analysis on which he founded the greatest hopes and which he classed among the happiest of his discoveries. He was present at the sitting of the 11th of May, but was suffering from a bad cold; his family and friends perceived with grief that he appeared much weakened, and that his features were changed. On Tuesday, the 12th of May, he repaired to his pleasant residence at Sceaux. He was unable to leave his room, yet nothing indicated his approaching end. He was continually occupied with the new developments in series, for which he was indebted to his *regulator*, and he completed the programme of his lectures at the Faculté des Sciences. On Thursday, the 21st of May, he conversed for some time with the Archbishop of Paris. His weakness increased on Friday, but he slept well that night; he awoke at three o'clock on Saturday morning, the 25th of May, in a state of great feebleness, and in about half an hour expired, apparently without pain.

The Rev. WILLIAM DANIEL CONYBEARE, Dean of Llandaff, was born in London, 7th June, 1787, and died 12th August, 1857, aged 71. The family name has been for some time honourably connected with the Church of England. His father was Rector of St. Botolph, Bishopsgate.

Following at a short interval, in Oxford, the academical progress of his brother John Josias Conybeare, and his friend William Buckland, he speedily associated himself in their mineralogical and geological studies, contributed actively to the growth of the Oxford Museum, and assisted in the early labours of the Geological Society. He explored personally much of the country round Oxford, and examined the north of Ireland, the vicinity of Bath and Bristol, and the coasts of Dorset, Devon, and South Wales.

Organic remains attracted his attention in the early part of his career. In 1814 he presented to the Geological Society remarks on some singular impressions occurring in flint*; in 1821, he was associated with De la Beche in the discovery of a new fossil animal, forming a link between the Ichthyosaurus and the Crocodile †; and in 1824, completed this investigation on the almost perfect skeleton of the Plesiosaurus ‡.

In these papers Conybeare opened a new and very fertile field of research, and cultivated it with success; manifesting so much knowledge of anatomy and the skeleton of reptiles, as to win from the great author of the 'Ossemens Fossiles' the free adoption of his conclusions, novel and startling as they appeared. Whoever will now read the admirable descriptions by Owen of this extinct reptile, or strive for himself to recompose from the ordinary fragments in museums its strange figure, will revere the early and successful labours of Conybeare, and comprehend in how high a degree they have helped forward the Palæontology of Britain.

From time to time the interest of the restorer of *Plesiosaurus dolichodeirus* was revived by the discovery of other species of the genus, so far as to give them a name, but he never again tasked his powerful mind on a systematic review of the subject:—satisfied, perhaps, with one long and steady gaze on these wonders of the earlier world, he resigned to other travellers the road which he had

* Geol. Trans. 1st Ser. ii. 328.

† Geol. Trans. 1st Ser. v. 559.

‡ Geol. Trans. 2nd Ser. i. 381.

opened to his own field of research. Always zealous in descriptive geology, he gave to the Geological Society in 1816 *, "Notices of the Sections presented by the Cliffs of Antrim and Derry," the fruit of a tour with Buckland, whom he also accompanied to Germany. In 1822 appeared the first volume of the 'Geology of England and Wales,' in which the names of W. Phillips and W. D. Conybeare occur together,—a most valuable work, of which the weightiest parts were contributed by Conybeare. Strange that thirty-five years elapsed in the life of the author, without drawing from his hand the second part of that capital work—not less remarkable the fact, that the deep respect of English geologists prevented any other hand from taking up the pen to complete the work he had begun so well. Among other contributions to his favourite science may be mentioned, Memoirs on the Hydrographical Basin of the Thames †,—on the Structure of the South Wales Coal Basin ‡,—on the Extent of Coal in the Midland Counties §,—and on the Great Landslip of Axminster ||.

English geology, under all its aspects, was the familiar theme of his daily conversation; no one hailed with more delight the discoveries of Murchison and Sedgwick in Siluria and Wales; the work of Wood, and Buddle and Hutton in the Coal-fields of the North; the successful researches of Mantell in the Wealden, and of Webster and Lyell in less ancient deposits. Nor was he negligent of the great contemporary geologists of France and Germany,—Cuvier, De Beaumont, Bronn, Von Buch,—or of the cultivators of this science in other parts of Europe. Perhaps nowhere can be found, previous to 1832, so large and so liberal a review of the progress of Geology in the Old World and in the New, as in the still very valuable Report ¶ on the state and prospects of this science, which he read to the British Association assembled in Oxford. In that and in separate Essays ** he enters fully into the phenomena which bear most directly on theoretical speculations, and presents, among other data for reasoning, a large section of the crust of the earth from the northern extremity of Great Britain to Venice.

In examining some of the districts which he has described, he acted

* Geol. Trans. 1st Ser. ii. 196.
† Proc. Geol. Soc. i. 145.
‡ Phil. Mag. Ser. iii. xi. 110.
§ Phil. Mag. Ser. iii. iv. 161; v. 44.
|| Edin. New Phil. Journ. xxix. 160.
¶ Brit. Assoc. Report, 1832.
** Phil. Mag. 2nd Ser. viii. 215; ix. 19, &c.

in conjunction with Buckland; in other cases his pen was supported by the pencil of De la Beche; but in every record of his scientific life, just self-reliance, close study, logical expression, and completeness of view, strongly mark the mind of W. Conybeare,—a mind well trained in letters and philosophy before it was turned to the difficult problem of the physical history of the earth.

Dean Conybeare was a Corresponding Member of the Institute of France; he was admitted Fellow of the Royal Society in 1819, but he was not a contributor to the Philosophical Transactions.

Dr. Marshall Hall was born at Basford in Nottinghamshire, in the year 1790. His father is stated to have been a man of superior ability, and to have made considerable attainments in chemistry and mechanics, whereby he was enabled to introduce improvements into the cotton-spinning trade in which he was engaged.

Dr. Hall received his early education at Nottingham and Newark, and at the age of twenty entered on the study of medicine at the University of Edinburgh, where three years later, in 1812, he took his degree. For the next two years he held the office of Clinical Clerk (or as it is now termed, 'Resident Physician') in the Royal Infirmary of that city, after which, with a view to further professional improvement, he visited Paris, Berlin, Göttingen, Giessen, and other medical schools of the Continent of Europe. Having commenced practice in Nottingham in 1815, he was appointed Physician to the General Hospital there, and rapidly rose to eminence.

During the ten years that Dr. Hall followed his profession in his native county, his mind was actively engaged in scientific pursuits; and it was at this time that he communicated to the Medical and Chirurgical Society of London his well-known memoir "On the Effects of the Loss of Blood," subsequently published as a separate work, which was directed against the practice, at that time prevalent, of what the author considered excessive depletion in inflammatory and supposed inflammatory disorders.

Having already earned a name in the medical profession, Dr. Hall, in 1826, transferred himself to London. His career as a physician in the metropolis was eminently successful, so that he was enabled at the age of sixty to release himself from strictly professional labour.

Amid the cares and duties of a London physician's life, Dr. Hall

continued to apply himself assiduously to original scientific research, and physiology, especially in its relations to medicine, was his favourite pursuit. In 1831 he published his "Essay on the Circulation of the Blood," which contains observations on the flow of blood in the capillary vessels of Batrachia and Fishes, and on the characters by which these vessels are distinguished from arteries and veins. On this occasion he made known his discovery of a remarkable pulsating sac or "caudal heart" connected with the vessels in the tail of the eel. The results of further physiological inquiries were published in the Philosophical Transactions for 1832, in two papers, one "On the Inverse Ratio which subsists between the Respiration and Irritability in the Animal Kingdom," the other "On Hybernation;" and a few years later he contributed the articles "Hybernation" and "Irritability" to Dr. Todd's Cyclopædia of Anatomy and Physiology.

But his name will hereafter be best known in connexion with the doctrine of the Reflex Function of the Nervous System, which was his most engrossing subject of pursuit for the last twenty-five years of his life. In the Philosophical Transactions for 1833 appeared his "Memoir on the Reflex Function of the Medulla oblongata and Medulla spinalis," the object of which is to show that certain involuntary acts previously designated as "sympathetic motions" are excited by impressions made on the extremities of certain nerves, and conducted by them to the spinal marrow and medulla oblongata, whence as from a centre they are reflected on the motor nerves of the parts moved; that these motions are concerned especially in various functions which are necessary for the preservation of the individual or of the species, and that they are independent alike of sensation and volition; moreover, that the "tone" of the muscular system belongs to the same order of phenomena and depends on the spinal marrow; lastly, that the reflex function may be exalted or depressed by medicinal agents, and in its abnormal or morbid conditions give rise to various well-known spasmodic, convulsive, and other nervous diseases.

Dr. Hall admitted that the phenomena of which he treated had long been known to physiologists, but he believed himself to have been the first to show their independence of sensation, to bring them together under one generalization, to establish with precision the laws of their production, to assign them their just rank in physiology, and to apply the doctrine to the elucidation of disease. But while we do

not question the sincerity of Dr. Hall's conviction of his own originality, there can be no doubt that he was much more largely anticipated by preceding physiologists than he could ever be brought to recognize; and this not only in the observation, but in the generalization and physiological application of the phenomena, and, in short, in all essential parts of the doctrine, such at least as are likely to maintain their ground. This, however, being admitted, it is equally true that from the time this subject was taken up by Dr. Hall, the physiology and pathology of the nervous system may almost be said to have entered into a new phase. With all the ardour of a discoverer persuaded of being the founder of a great and influential doctrine, he thenceforth made the reflex, or, as he subsequently called it, the excito-motory system, his chief study, and laboured incessantly in extending and consolidating its province for the rest of his life; and to him belongs especially the merit of successfully applying the principle to the interpretation of disease. By his numerous writings and unwearied personal exertions, attention was awakened on all sides to the importance of the phenomena; and they soon became a prominent subject of intelligent observation and earnest discussion among physiologists and medical men both in this country and on the Continent, where Professor Müller had independently and almost simultaneously arrived at results in great part similar to those of Dr. Hall, though he was somewhat later in publication. In this way the principle of the reflex function has come to take its due place in physiology and medicine, and important truths, previously seen or at least comprehended in their systematic connexion only by a few, are now established and familar doctrines in science.

Four years after the date of his first memoir, Dr. Hall communicated a second paper to the Royal Society, which formed the subject of the Bakerian Lecture for 1837. In this paper, which was entitled "On the True Spinal Marrow, and the Excito-motory System of Nerves," he gave a new exposition of the constitution of the nervous system, on the assumption that there are special "excitor" and "motor" nerve-fibres subservient to the reflex function, and different from the sensory and volunto-motory fibres, though mixed with them in the same sheaths; and that these special nerve-fibres are in relation to a special part of the spinal marrow and of its encephalic prolongations, which serves as a centre, anatomically blended with, but physiologically distinct from, the part of the cord ministering to

sensation and volition. Opinion was much divided as to the validity of this hypothesis, and the subsequent progress of inquiry cannot be said to have been on the whole favourable to it. In the same memoir Dr. Hall adduces observations and experiments to show that the ordinary (but not voluntary) movements of respiration are excited in a reflex manner through the medulla oblongata, and do not emanate from that part as their *primum mobile,* as he had previously conceived in common with most preceding and contemporary physiologists. This second memoir was not inserted in the Philosophical Transactions, which was ever after a subject of grievance with the author; but in point of fact the original matter contained in it had already been made public by Dr. Hall himself in his 'Lectures on the Nervous System and its Diseases,' published in 1836; and his new experiments and amended views respecting the respiratory movements had also been communicated by him to the Zoological Society and published in their 'Proceedings' in 1834: it subsequently appeared along with a reprint of the first memoir as an independent publication by the author, under the title of 'Memoirs on the Nervous System,' 4to, 1837.

Dr. Hall's further researches and later views in Neuro-physiology are to be found chiefly in his 'New Memoir on the Nervous System,' 1843, and his 'Synopsis of the Diastaltic Nervous System,' which is an outline of the Croonian Lectures delivered by him at the College of Physicians in 1850. His more strictly professional writings are many and valuable; they appeared partly as independent publications, and partly in Journals and in Transactions of Societies; but it would exceed our limits to give even the titles of the numerous and varied productions of his fertile genius, active temperament and ready pen. The more important of them are indicated in the obituary memoirs of Dr. Hall which have appeared in the medical journals; but we cannot thus pass over his last service rendered to the cause of humanity, in the introduction of a simple and easily applied method of restoring suspended respiration, which, if we may trust the positive testimony flowing in on all hands, has already been the means of rescuing many from untimely death.

In 1853 Dr. Hall paid a visit to America, but in the mean time a severe and exhausting complaint under which he had long suffered was gaining upon him, and to escape its aggravation by our less

genial climate he passed the winter of 1854 in Italy; but his powers of life at length succumbed, and he died at Brighton on the 11th of August, 1857.

Dr. Hall was a member of the Institute of France, and of most of the learned societies of Europe and America. The date of his election into the Royal Society is April 5, 1832; he served on the Council in 1850–52.

JOHN AYRTON PARIS, M.D.—The unvaried tenor of a physician's life ordinarily affords few opportunities for remark, even while he is rising in the good opinion of the scientific and learned. This observation must be qualified in relation to the late Dr. Paris. Deeply and thoroughly versed in the practical studies of his profession, he became eminent in general science, and in his own profession his researches tend to throw new lights upon it.

Born in the city of Cambridge in August 1785, and educated there, partly at home, partly under the care of Mr. Barker of Trinity Hall, in his early years, he was matriculated at Caius College on the 17th of December, 1803, and was elected to a Tancred Studentship in Physic on the 3rd of January, 1804. From the commencement of his career at Cambridge he evinced the strong predilection for natural science which afterwards distinguished him, and was a diligent student of chemistry under Professor Farish and of mineralogy under Dr. Clarke. Leaving Cambridge, he proceeded to Edinburgh, and having taken full advantage of the professional teaching of that city, and obtained from Cambridge the degree of Bachelor in Medicine, he proceeded to London. There his talents and acquirements obtained for him at once the high opinion and regard of one similarly accomplished, the late Dr. Maton, which largely promoted his success, and continued for life. Dr. Paris became Physician to the Westminster Hospital in 1809 by a large majority of votes, in his 23rd year.

From London he went in 1813 to Penzance in Cornwall, and there, besides obtaining a high degree of medical reputation, he became eminent in mineralogical and geological researches; he proposed, indeed established with the cooperation of his friends, and largely contributed to, the Royal Geological Society of Cornwall. His contributions comprise papers "On a Recent Formation of Sandstone

occurring in various parts of the North Coast of Cornwall;" "On Accidents which occur in the Mines of Cornwall in consequence of premature Explosion of Gunpowder in Blasting Rocks, and on the Methods to be adopted for Preventing it;" "Observations on the Geological Structure of Cornwall," &c. A valuable paper on the Soils of Cornwall was contributed by him to the Penwith Agricultural Society.

Dr. Paris returned to London in 1817. In the course of an honourable and successful career of practice, he was elected President of the Royal College of Physicians, on the death of Sir Henry Halford, after serving repeatedly in the office of Censor. Finally, in the full possession of his mental powers, and to the last moment devoted to the interests of the College, which he loved, Dr. Paris departed this life, June 24, 1857, under very painful disease, borne by him with great constancy.

Dr. Paris was elected a Fellow of the Royal Society in 1821, and repeatedly served on the Council.

His works were numerous, and obtained a large circulation. The 'Treatise on Medical Jurisprudence,' published in conjunction with Mr. Fonblanque, 1825; the 'Treatise on Diet,' 1827; the 'Life of Sir Humphry Davy,' 1831, a most felicitous instance of perfect biography; his delightful little book, 'Philosophy in Sport made Science in Earnest.' But, greatest of all in its originality and practical usefulness, though earliest in its appearance, was Dr. Paris's 'Pharmacologia'; it came out first as a small volume in 1812. On this last work, if he had published nothing else, his claims, as enlarging the science of medicine, might safely be rested.

The Rev. William Scoresby, son of the well-known whaling captain, was born at Crofton near Whitby in 1789. At the age of ten, having been taken on a farewell visit to his father, who was about to set out on a voyage, he was so delighted with all he saw in the ship, that he contrived a boyish scheme for remaining on board, and thus unpremeditatedly began his acquaintance with the sea. Some of the incidents of this voyage, and among them the clever escape from a hostile cruiser, are related in 'Memorials of the Sea,' a book which he published fifty years afterwards. The voyage made him a confirmed sailor, and from 1803 he accompanied his father in the

Resolution for eight years, and manifested his diligence and turn for observation by keeping a regular journal in addition to his other duties. The fact that he won the responsible post of first mate of the ship while yet in his sixteenth year, and that he was appointed commander in 1811, as soon as he became of legal age, supplies good testimony as to his courage and skill in his adventurous profession.

It was during this period, in 1806, that the Scoresbys sailed to a higher north latitude than, in the absence of trustworthy evidence to the contrary, had ever before been reached. Steering northwards from the western coast of Spitzbergen, they found an open sea, in which they not only captured as many whales as furnished a full cargo, but found on one occasion their position to be 81° 30′ N., about 510 miles from the Pole. The sea was then so clear, that but for the risk of detention from sudden frost, they might have still sailed uninterruptedly to the northward.

In his subsequent voyages, the younger Scoresby observed the disappearance of the vast accumulations of ice that had for years closed the sea on the west of Greenland; and in 1817, pursuing a correspondence of some years' standing with Sir Joseph Banks, he informed that eminent person of the remarkable phenomenon. In the following year the Government, acting on this information, and the recommendation of the Council of the Royal Society, despatched the first of the expeditions which, within the present century, have resolved the important geographical question of a north-west passage.

The winter season between his voyages had always been employed by Scoresby in the acquisition of scientific knowledge: he had studied during two sessions at Edinburgh, where by his assiduity he had gained the friendship of some of the Professors. In 1820, after his seventeenth voyage to the Polar seas, he published, at the suggestion of Prof. Jameson, his well-known book in two volumes, 'An Account of the Arctic Regions, with a History and Description of the Northern Whale Fishery.' Being the first popular work on that subject, it was eagerly read, and while it brought fame to the author, prepared the way for further developments of whaling enterprise, and for arctic research generally.

Meanwhile Capt. Scoresby was making observations on magnetism, a part of science to which, at a later period of his life, he especially

applied, and carrying out measures for preserving the health of his crews, in which, though he maintained strict discipline, he gained their esteem. He owed much of his success to a rigid observance of Sunday, making it a complete day of rest. And it is said that in "his later voyages he adopted the temperance principle on board his vessel, finding that hot coffee was a very much stronger preservative than spirits against the intense cold of arctic regions."

On the death of his second wife in 1822, Capt. Scoresby relinquished the whale fishery, and thenceforth devoted himself to scientific pursuits and religious duties on shore. He had been for some time a Fellow of the Royal Society of Edinburgh, when in 1824 he was elected a Fellow of the Royal Society of London, and subsequently he was chosen a member of the section Geography and Navigation, of the Academy of Sciences of the Institute of France. Yielding however to the religious convictions which had always characterized him, he entered himself as a student of Queen's College, Cambridge, where he took his degree of B.D. in 1834, followed afterwards by that of D.D. and entrance into Holy Orders. He officiated for awhile at the Mariners' Church, Liverpool, then at Exeter, and at Bradford in Yorkshire, but eventually resigned the living and retired to Torquay.

Here he applied himself anew to the study of magnetic phenomena, and published in a collected form, with new facts and observations, the various papers which had appeared from his hand in the Edinburgh Philosophical Journal, the Transactions of the Royal Society of Edinburgh, and the Philosophical Transactions. The important questions of the effect of the iron of ships upon the compass—the effect of concussion and of change of latitude on the permanent magnetism of iron ships—the capacity and retentiveness of steel in different states for the magnetic condition—the nature and phenomena of magnetic induction, and other allied questions, are discussed in the publication referred to, which appeared at intervals from 1839 to 1852, in three volumes, under the title of 'Magnetical Investigations.'

The Reports of the British Association also contain papers by Dr. Scoresby on these subjects. At the meeting of that body at Glasgow in 1855, he communicated additional evidence in favour of his views and suggestions, particularly on the question of elevating the com-

pass to a considerable height above the deck in iron ships. And a few months later, with a view to further observation and experience, he undertook a voyage to Australia in the *Royal Charter*, making careful and laborious investigations on the question which he had so much at heart, both going and returning. His arrival at Melbourne was made the occasion of conferring on him the degree of M.A. of the University of that city.

Dr. Scoresby came back from Australia in a weakened state of health, caused by the fatigues of the voyage; nor did his return home promote recovery. He grew gradually weaker, and died at Torquay on the 21st of March, 1857, at the age of sixty-seven, leaving his third wife a widow.

Dr. Scoresby was the author of several works not mentioned above. Besides a volume of Sermons and books of a religious character, he published in 1822, 'Journal of a Voyage to the Northern Whale Fishery; including Researches and Discoveries on the Eastern Coast of West Greenland.' A translation of this book into German appeared three years later at Hamburg. In 1851 he brought out a volume, entitled 'Memorials of the Sea,' being records of his father's adventurous life, and intended to follow it by a similar volume concerning his own. This will probably now appear as a posthumous work. He published also the 'Zoistic Magazine,' with a view to elicit the scientific principles of mesmeric phenomena; and during the first excitement respecting the lost arctic explorers, he wrote 'The Franklin Expedition,' a small book embodying his views as to the course and fate of the party, and the means to be taken for their rescue.

M. Thénard was born on the 4th of May, 1777, at Nogent sur Seine, in Champagne. His father, a farmer in humble circumstances, was a man of strong sense. He soon discovered the abilities of his son, and, at a great sacrifice to himself, procured for him a good elementary education at Sens. There, young Thénard acquired a taste for classical literature, which never forsook him. At the age of sixteen he went to Paris, in order to study pharmacy, with the intention of returning to practise it in Loutière. By good fortune he commenced his chemical studies in the laboratory of Vauquelin, who soon discovered the talents of his pupil. Thénard felt a new life spring up

within him in the midst of the intellectual activity of Paris, and the intention of returning to Champagne was abandoned. Thénard's ability as a teacher, and his power of elucidating science with a kind of dramatic effect, suggested, it is said, by witnessing the performances of Talma, induced Vauquelin to obtain for him the appointment of Répétiteur at the E'cole Polytechnique, where he first became acquainted with Gay-Lussac. This happened about the year 1798. Soon afterwards, Vauquelin deputed him to deliver some chemical lectures for him at the E'cole Polytechnique, celebrated then, as now, for the ability of its lecturers. In this task he was eminently successful, notwithstanding the disadvantage of a strong Champagne dialect, which he overcame with difficulty. About this time, being twenty years old, his labours, for which the E'cole Polytechnique opened a field, attracted the attention, and earned the applause of Laplace and Berthollet. Thénard's first publication, a memoir on the combinations of antimony with oxygen and sulphur, appeared in 1800. Guyton de Morveau, one of the Commissioners appointed to report upon it to the Institute, declared they recognized in M. Thénard, at that time only twenty-three years of age, a chemist practised in the most delicate manipulation, and possessed of all the means of promoting the science, and that he ought to be encouraged in a career upon which he had entered with such success. In 1801 M. Thénard obtained sebacic acid by submitting to distillation various fatty substances. He discovered a new method of making cerusse, which was carried into practice by Roard; also a blue pigment, known by his name, obtained by igniting hydrate of alumina with arseniate or phosphate of the oxide of cobalt, and a very simple and practical method of purifying oils and rendering them more fit for the purposes of illumination. This process has been used on an enormous scale for more than the third of a century. He was the first chemist who devised a process for estimating with accuracy the quantity of carbonic acid present in atmospheric air, by agitating the air with a solution of baryta. In 1802 Thénard was appointed to fill the Chair of Chemistry at the Collége de France, vacant by the retirement of Vauquelin, who recommended Thénard as his successor. About the same period he became one of the ordinary Professors of the E'cole Polytechnique, and may be considered to have fairly established himself on a level with the most eminent men of his time. From the

year 1807 he was associated with Gay-Lussac in the production of the 'Recherches Physico-chimiques,' which have founded their reputation in Europe, and have contributed so largely to the progress of chemical science. The account of their labours appeared in two volumes in 1811. The following are some of the more important subjects discussed in this work. The discovery of a purely chemical process of preparing in considerable quantities the metals of the alkalies by decomposing potash and soda by contact with iron at a high temperature, immediately after Davy had obtained them in minute quantities by the action of a voltaic current; the discovery of boron, of fluoboracic acid, and of hydrofluoric acid; researches on muriatic acid and oxygenated muriatic acid, since known as hydrochloric acid and chlorine; and lastly, a discovery which ranks among those that have had the greatest influence on the progress of chemistry, two methods of analysing organic compounds, with an application of one of these methods to the analysis of fifteen different organic substances. So highly was this work esteemed, that in 1810 M. Thénard was unanimously elected Member of the Institute in the place of Fourcroy.

About this time a third Professorship was bestowed upon him, that of Chemistry at the Faculté des Sciences de Paris. The discharge of his new duties was followed by the same brilliant success that had attended his lectures at the E'cole Polytechnique and the Collége de France. The three Professorships which he held at the same time, and the duties of which he discharged without apparent effort, seemed hardly sufficient to satisfy the extraordinary activity of his mind. The attractive richness of his teaching, combined with his beautiful discoveries, spread his reputation over the whole scientific world. It became requisite to build larger lecture-rooms for him, and during twenty years he lectured to a class of more than a thousand hearers. In 1812 appeared his 'Traité E'lémentaire de Chimie Théorique et Pratique.' It went through six editions, and made the fortune of the publisher. This treatise, equally remarkable for lucidity of exposition and completeness of matter, was translated into many different languages, promoted a knowledge of chemistry, and rendered the name of Thénard popular in every country into which science has penetrated.

The most remarkable of his discoveries was that of the peroxide of

hydrogen in 1818. This was followed by the discovery of the peroxides of calcium and strontium. M. Pelouze, in his address to the Academy on the occasion of the death of Thénard, from which, and that of M. Ch. Giraud, the greater part of this notice is taken, observes that no person has so largely contributed to spread a taste for chemistry by his books and lectures, and especially through the medium of his numerous pupils, as Thénard. In 1827 he was elected a Member of the Chamber of Deputies, in which he joined the liberal and moderate party.

After the Revolution of 1830, he became a Member of the Council of Public Instruction, and soon afterwards, in company with Gay-Lussac, was called to the Chamber of Peers. In the capacity of Administrator of the Collége de France, and of the Faculté des Sciences, as Member, and afterwards Vice-President, for a great number of years, of the Conseil supérieur de l'Instruction publique, he contributed, more than any person since Cuvier, to the development and progress of the principal scientific institutions of France.

He was three times President of the Jury of the Exposition; he took an active part in the administration of railroads, and was President of the Société d'Encouragement after the death of Chaptal in 1832.

To the end of his life he took an active share in the labours of the Academy. He was elected a Foreign Member of the Royal Society in 1824.

He married the daughter of M. Humblot Conté. She was the granddaughter of Conté, Member of the Institute of Egypt. Her death, after a union of forty years, was soon followed by that of one of his sons.

During the last years of his laborious life, he published some interesting researches on the waters of Mont Dore, and, conjointly with his son, M. Paul Thénard, commenced researches on decompositions by contact, the first part of which has been read before the Academy. A few months before his death, he undertook the formation of a charitable institution, called "La Société de Secours des Amis des Sciences." M. Thénard came from his estate of La Ferte-sur-Crosne, near Châlon-sur-Saone, to his house in the Place Saint-Sulpice in Paris, in order to undergo a slight surgical operation, the removal of an encysted tumour. The operation was successfully performed by

M. Velpeau. The wound healed rapidly, and there seemed no doubt of a complete cure, when he was attacked by a catarrhal affection, which, during the last fifteen years of his life, had often obliged him to keep his bed. On Thursday, the 18th of June, 1857, the mildness of the air tempted him to drive in the Bois de Boulogne. On returning to his house about six in the evening, fever had increased, and the organs of respiration were more and more oppressed, without, however, causing any serious alarm. On the night of Friday he grew worse. His son, M. Paul Thénard, who had been sent for, arrived on Sunday morning, while M. Thénard was still conscious, and able to converse. About 2 P.M. his speech failed. He died between 5 and 6 P.M., on Sunday, the 21st of June.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s*. Annually.	Paying £4 Annually.	Total.
December 1, 1856..	9	50	376	10	275	720
Since elected......	......	......	+9	......	+6	+15
Re-admitted	......	......	......	......	......	....
Since compounded..	......	......	+1	......	−1	....
Withdrawn	......	......	......	......	−1	−1
Since deceased	......	−2	−12	−2	−3	−19
November 30, 1857	9	48	374	8	276	715

Statement of the Receipts and Payments of the Royal Society between November 30, 1856, and December 1, 1857.

	£	s.	d.
Balance in the hands of the Treasurer	136	14	3
Subscriptions and Compositions	1760	16	0
Rents	181	3	2
Dividends on Stock	1128	3	2
Sale of £1500 Consols	1329	5	0
Sale of Transactions, Proceedings, &c.	205	0	3
Miscellaneous Receipts	53	7	6
Balance due to Banker	47	9	0
	£4841	18	4

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s.* per annnm.

Estate at Acton, Middlesex (28 A. 0 R. 21 P.), £60 0*s.* 0*d.* per annum.

Fee farm rent in Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per cent. Annuities.

£23,569 17*s.* Consolidated Bank Annuities.

£513 9*s.* 8*d.* New $2\frac{1}{2}$ per cent. Stock.

EDWARD SABINE,

Treasurer.

	£	s.	d.
Professor Faraday, Bakerian Lecture	3	14	8
Mr. Paget, Croonian Lecture	2	16	6
Salaries and Wages	753	14	6
Fire Insurance	45	1	6
Printing Transactions	500	8	6
Ditto Proceedings and Miscellaneous	214	2	10
Engraving	316	15	3
Paper for Transactions and Proceedings	289	1	3
Binding Transactions	74	13	3
Books Purchased and Binding	359	19	2
Burlington House Removal Expenses	1299	19	10
Stationery	12	16	6
Shipping Expenses	17	10	10
Fire and Lighting	138	0	0
House Expenses	54	16	2
Table Furniture for Soirées	52	7	6
Taxes	49	3	4
Law Expenses (1853 to 1857)	45	3	0
Copley Fund	10	12	6
Donation Fund	451	13	0
Wintringham Fund	33	15	0
Postage, Miscellaneous and Petty Charges	88	8	7
Cash in the hands of the Treasurer	27	5	8
	£4841	18	4

December 10, 1857.

The LORD WROTTESLEY, President, in the Chair.

Mr. Robert Angus Smith was admitted into the Society.

The President announced that he had appointed the following gentlemen Vice-Presidents, viz. Mr. Gassiot, Mr. Grove, Dr. Hooker, Mr. Horner, and Mr. Owen.

The following communications were read:—

I. "On the Chemical Action of Water on Soluble Salts." By Dr. J. H. Gladstone, F.R.S. Received November 19, 1857.

Before extending my researches on chemical affinity among substances in solution, it seemed desirable to ascertain, if possible, what specific chemical action water exerts on a salt. This inquiry is beset with unusual difficulties, and unfortunately my experiments have not led to any conclusive result. Yet some of the observations made during the course of the inquiry have a value independent of theory, and a brief notice of them may not perhaps be deemed unworthy of a place in the Proceedings of the Royal Society.

It is well known that many anhydrous salts will absorb water, and still remain solid bodies, either amorphous or crystallized. In such a case the water combined is always in simple atomic relation with the salt itself; great heat is often evolved, and a change of colour frequently ensues. These "hydrated" salts (as they are usually considered) are generally soluble in water; and it is the condition of such a body when dissolved that opens a wide field for speculation. The water may act merely as a solvent; or it may unite without decomposition with the dissolved salt, becoming an integral part of the compound in solution; or reciprocal decomposition may ensue, each electro-positive element combining with each electro-negative one in certain proportions; or the ultimate result may be due to two or more of these modes of action in conjunction.

When a "hydrated" salt is dissolved in a minimum of water,

nothing is usually observed beyond the new physical properties resulting from the change in its state of aggregation and the absorption of heat. No change of colour, as far as I can find, ever ensues, though a change in the amount of fluorescence may occur. When an anhydrous salt, which will not combine with water to form a solid compound, dissolves, a change of colour does sometimes ensue. Sometimes, however, an evident decomposition takes place, the hydrogen and oxygen of the water combining each with one of the elements of the other binary compound, and the products of this action remaining uncombined. Chloride of bismuth and citrate of ammonia are instances. But in the vast majority of instances, the salt MR and the water HO do not suffer reciprocal decomposition, unless indeed, as has been contended, the resulting MO, HR remain combined together in solution.

If a reciprocal decomposition of this character actually occurs, it may be anticipated by analogy, that by increasing the amount of HO, more MR will be decomposed. Now, if additional water be added to saturated aqueous solutions of pentachloride of antimony, ferric sulphate, ammoniacal nitrate of copper, or nitrate of bismuth, decomposition results, and a precipitate forms proportional within certain limits to the amount of water added; but not one of these is a salt of the simplest constitution. Sometimes, however, a change is rendered apparent in simple salts by a change of colour without the formation of a precipitate.

This was closely examined. It might be expected, *à priori*, that a certain amount of salt would have the same absorbent effect on a given quantity of light, whether it were dissolved in much or little water, and that as the absorbent power of water is practically *nil*, it would appear to the eye of precisely the same depth and character of colour in the two cases. And this actually holds good in the majority of instances; but to prove it a special contrivance was necessary, in order to make the same quantity of light impinge upon the solution before and after dilution. This was effected by means of colourless cylindrical glasses of uniform diameter and the same size, closed at one end with a flat plate of glass, so that when placed upright they could hold liquids: they stood in a case so contrived that all the light which passed through the strong or diluted solution, as looked through from above, had to enter by the flat plate

at the bottom. Every experiment was performed by a comparative method, two glasses being placed side by side, one containing the solution to be diluted, the other a similar quantity of the same solution which served as a standard.

In this manner it was determined that the following salts absorbed the same light whether dissolved in much or in little water:—

Ferrous Sulphate.
Ferric Nitrate.
Ferric Meconate.
Ferric Comenate.
Ferric Comenamate.
Ferric Gallate.
Nitrate of Nickel.
Nitrate of Cobalt.
Sulphate of Cobalt.
Chloride of Chromium.
Acetate of Chromium.
Chromate of Chromium.
Nitrate of Uranium.
Chloride of Uranium.
Sulphate of Ceric Oxide.
Terchloride of Gold.
Terbromide of Gold.
Protochloride of Platinum (in hydrochloric acid).
Bichloride of Platinum.
Bichloride of Palladium.
Chromate of Potash.
Ferrocyanide of Potassium.
Ferridcyanide of Potassium.
Nitroprusside of Sodium.
Sulphindigotic acid.
Sulphindigotate of Ammonia.
Carbazotate of Copper.
Pentasulphide of Potassium.

The following salts were affected in regard to their absorption of light, by adding water to their saturated solutions:—

Salt.	Saturated solution.	Dilute solution.
Ferric Acetate.	Red.	Darker.
Ferric Tartrate.	Red.	Slightly paler.
Ferric Chloride.	Orange-red.	Orange-yellow.
Ferric Citrate.	Red.	Orange and paler.
Ferric Sulphocyanide.	Intense red.	Orange.
Chloride of Nickel.	Yellowish green.	Bluish green.
Iodide of Nickel.	Deep green.	Paler blue-green.
Chloride of Cobalt.	Red.	Paler and less pure.
Iodide of Cobalt.	Deep green.	Pale red.
Acetate of Cobalt.	Red.	Paler and more orange.
Sulphocyanide of Cobalt.	Intense purple.	Pale red.
Chloride of Copper.	Green.	Blue.
Bromide of Copper.	Green.	Blue.
Acetate of Copper.	Greenish blue.	Paler and purer blue.
Permanganate of Potash.	Purple.	Paler and redder.
Chromic acid.	Red.	Orange.

That these changes of colour are due to the action of the water, and not to any merely physical cause, is proved by the fact that alcohol does not occasion them. Quantitative experiments were instituted with acetate of copper and sulphocyanide of iron, to determine whether the effect of successive additions of water is in a decreasing ratio. It was found to be so on the whole, but the

results showed certain irregularities that do not usually occur in cases of reciprocal decomposition, where the mass of one of the compounds is successively increased.

A prismatic examination of the rays absorbed by these salts in different states of solution revealed two very suggestive facts. The one is, that in every case (except ferric acetate) the salt in dilute solution not only transmits every ray that was transmitted by it in saturated solution, but also some rays which it then absorbed. The other is, that strong solutions of the chlorides, bromides, and iodides of copper, cobalt, nickel, and iron—analogous metals—exhibit not only the absorption due to the respective bases, but another absorption which can be identified with that produced by the halogens themselves when simply dissolved in water; while, when these solutions are diluted, they cease to produce this second absorption, and give precisely the same prismatic image as any compound of the same base with a colourless acid. The amount of water required to effect this change depends on the temperature. That the phenomena indicate some difference of arrangement among the elements of the dissolved salt and the water, cannot, I think, be doubted, but they fail to show in any distinct manner what that difference is.

The action of water on double salts is a still more complicated problem; but the question as to whether water separates the two components did not prove so difficult of decision. While on the one hand the physical properties of many double salts, as for instance the potassio-chloride or iodide of platinum, prove that they are not decomposed by water, the experiments of Graham, on the other hand, show that some salts, as for instance alum, suffer at least a partial decomposition in diffusion.

The iodide of mercury and potassium, and the sulphocyanide of silver and potassium, dissolve in a small quantity of water, but the addition of more causes the separation of the insoluble component. The double sulphates of copper, nickel, or chromium with potash, the sulphate of copper and ammonia, the chloride of platinum and potassium, the iodides of platinum or gold with potassium, and the hydrochlorate of chloride of gold, do not change in colour on the dilution of their aqueous solutions; but this does not prove that no separation has taken place, for the colour of these double salts in solution is precisely that of an equivalent amount of that component

to which the colour is due. But bichlorate of potash and bicomenamate of iron likewise exhibit no change of colour on dilution, though such must ensue, if they be converted into neutral salt and free acid. On the other hand, the red potassio-oxalate of chromium varies in intensity of colour on the addition of water, and the different double chlorides of copper undergo the same change as the simple salt. If hydrochlorate of terchloride of gold be added to the terbromide of that metal, a reduction in colour ensues, and an analogous result is obtained when the double sulphate of copper and potash acts on the acetate of copper—facts which point to a decomposition of the double salt in solution. Indeed it is evident that some double salts are resolved more or less into their components by water, while others are not so affected.

The general tendency of my observations has led me to the opinion, that water does not act upon a salt dissolved in it in a manner analogous to that of the hydracids, but I hesitate to draw any conclusion as to the rational constitution of a dissolved salt.

II. "On the Molecular Properties of Antimony." By George Gore, Esq. Communicated by Dr. Tyndall, F.R.S. Received December 10, 1857.

(Abstract.)

Antimony may be readily deposited by the electro-process from either of the following liquids :—5 parts of tartar-emetic and 5 parts of tartaric acid dissolved in a mixture of 2 parts of hydrochloric acid and 30 parts of water ; or 3 or 4 parts of tartar-emetic dissolved in 1 part of the ordinary chloride of antimony.

The metallic deposits obtained from these two liquids differ greatly in appearance, in structure, and in physical properties : that obtained from the first liquid has a silver-grey colour and frosted surface, is hard in texture, and has a beautiful radiating crystalline structure ; whilst that obtained from the second liquid has the colour and appearance of highly polished steel, and has a bright metallic amorphous fracture. The specific gravity of the former is 6·55, whilst that of the latter is 5·78, both being somewhat variable in this respect. The electro-chemical equivalent of the crystalline variety,

after deducting a small portion of gas contained in it, is about 40·2; and of the amorphous kind, after deducting a much larger per-centage of gas and of chloride of antimony, which it always contains, the same; but the equivalents actually obtained, including those substances, were 40·7 for the crystalline and 43·3 for the amorphous variety. Amorphous antimony was found to be electro-positive to the crystalline kind, both in acids and alkalies; it was also thermo-electro-positive to that substance; and both reduced silver by immersion in a solution of nitrate of silver.

Both these substances when deposited are in unequal states of cohesive tension at their two surfaces, frequently in so great a degree as to rent the metal in all directions. But the most remarkable circumstance, and of which a brief account was published in the Philosophical Magazine, January 1855, is, that amorphous antimony is liable, by percussion or heat, to undergo a rapid and intense molecular change throughout its mass, consisting apparently of a violent commotion amongst its particles, similar, but in a much higher degree, to the changes already observed by other experimentalists in sulphur, selenium, iodide of mercury, &c., and attended by evolution of an extraordinary amount of heat, sufficient, when the substance is massive, to raise its temperature from 60° to upwards of 450° Fahr., melting in several instances bars of tin and other metals.

During the action the chloride of antimony and a portion of the gas are expelled by the heat, and the substance loses its remarkable property. After the action the antimony is found to have undergone no oxidation, but to have considerably altered in its physical characters; it has lost its steel-bright colour and become comparatively grey, and has acquired a dull grey granular fracture; its specific gravity has also increased, and it has evidently passed a considerable stage towards the condition of the other variety. The grey metal undergoes no such change.

By careful trituration of thin pieces of the amorphous metal under cold water, it has been obtained in the state of a fine powder possessing the same molecular property. The chloride of antimony adheres to the metal with considerable force, and is only partly removed by digesting the powder in dilute hydrochloric acid for a week; and the gas contained in both varieties is only expelled by pressing them.

III. "Researches on the Structure and Homology of the Reproductive Organs of the Annelids." By THOMAS WILLIAMS, M.D., F.L.S., Physician to the Swansea Infirmary. Communicated by THOMAS BELL, Esq., F.R.S., Pres. L.S. Received October 21, 1857.

The present communication is a revision of a paper by the author, which was read on the 12th of February, 1857, under which date an Abstract is given.

December 17, 1857.

Major-General SABINE, Treasurer and V.P., in the Chair.

The following communications were read:—

I. "Observations on the Poison of the *Upas Antiar*." By Professor ALBERT KÖLLIKER, of Würzburg. Communicated by Sir B. C. BRODIE, Bart. Received December 1, 1857.

During my stay in England, in the autumn of 1857, I was so fortunate as to acquire the rare poison of the famous *Antiaris toxicaria* (Lesch.), with which no experiments have been tried since the time of Magendie, Brodie, Horsfield, and Schnell and Emmert (1809–1815). I owe my specimens of the Antiar poison to my friend Prof. Christison, of Edinburgh, who had it from Borneo, and to Dr. Horsfield, of London, who collected it himself during his stay at Java in the beginning of this century; and as both specimens were fully active—as some preliminary experiments made in company with my friends Dr. Sharpey and Dr. Allen Thomson showed—I thought it well worth while to devote some time to the study of the poison, and to try to elucidate its manner of action on the animal organism. The following are the principal results which I obtained in my experiments with frogs, and I hope that they will not be deemed unworthy of notice by those who take an interest in the physiological action of poisons in general.

The Antiar, like most other poisons, acts from the intestinal canal, and from wounds; but it must be remarked, that it is much more energetic and rapid when introduced into a wound. The symptoms which are observed in frogs, in the latter case, are the following:—First of all, the *voluntary movements* become less energetic, and at length cease totally, 30 to 40 minutes after the introduction of the poison (after 21^m min. and 1^h 21^m max.). Then follows a time in which reflex movements may be caused by stimulating the skin; but this faculty also is lost very soon, viz. at from 50 to 60 minutes (at 33^m min. and 85^m max.); and the animals die without the slightest trace of convulsions or tetanic spasm. If now the frogs are opened, we find that, without any exception, *the heart has ceased to beat*. The auricles are dilated, the ventricle corrugated, rather small, and generally red, as if blood had been extravasated into its muscular parietes; but very soon the exposure of the heart to the air causes the ventricle to shrink a little more, and to become pale and stiff, as if in the state of *rigor mortis*. All interior organs, especially the lungs, liver, stomach, intestine, and kidneys, are gorged with blood, and in a state of great, especially venous, hyperæmia. The blood is fluid and rather dark, but soon coagulates when exposed to the air, and assumes a brighter colour. The lymphatic hearts cease to beat as soon as the reflex movements are lost. At the same time the nerves are yet found excitable, but their power is very low, and generally vanishes in the second hour after the application of the poison. The same must be said of the muscles, which contract very feebly when directly stimulated by galvanism, and in most cases lose their power totally in the second or third hour, and generally a little after their nerves. The *rigor mortis* begins early, sometimes in the sixth hour, and is generally well established at the eighteenth hour.

Amongst all these symptoms, to which we may add some signs of vomiting occurring now and then, there was none which attracted my attention more than the cessation of the movements of the heart, considering the great energy which this organ possesses in frogs; and I tried, therefore, before all, to elucidate the action of the Antiar upon the heart. For this purpose I instituted a new series of experiments, in which I exposed the heart by the section of the sternum, before the poison was introduced into a wound of

the back; and in this way I easily got the result, that the heart ceases to beat as soon *as from the fifth to the tenth minute* after the introduction of the Antiar; and so, that first the ventricle stops, and half a minute or one minute later, also the auricles. Now, as the frogs at this time are not at all deprived of their faculty to move, we may have the rather astonishing view of an animal, with artificially-paralysed heart, which moves and leaps as freely as if nothing had happened.

The experiments just mentioned prove, *that the first action of the* Upas Antiar *is to paralyse the heart*; and I am therefore quite in accordance with Sir Benjamin Brodie, who, by his experiments on mammalia, came to the same result in 1812; whilst I cannot otherwise than disagree with Schnell (Diss. de *Upas Antiar*, Tubingæ, 1815), who assumes that this poison acts in the first place on the spinal marrow. Now this point fixed, the further question arises, whether the other symptoms mentioned, viz. the paralysis of the voluntary and reflex movements, and the loss of the irritability of the muscles and nerves, are only the results of the paralysis of the heart, or must be attributed to a specific action of the Antiar. For the elucidation of this question, I found it necessary to study the consequences of the suppression of the heart's action on the organism of frogs, which I did in the same way as it had been done by others, especially by Kunde (Müller's Archiv, 1847); viz. by cutting out the heart, or by putting a ligature around the base of it, so as to stop the circulation totally. The results of these experiments were in both cases the same, that is to say, the voluntary movements ceased in from 30 to 60 minutes, and the reflex movements after one or two hours. Hence it follows that these two symptoms of the poisoning with Antiar are simply dependent on the paralysis of the heart caused by it. With reference to the irritability of the muscles and nerves, on the contrary, it is easy to show that the ligature or excision of the heart has not the same influence as the Antiar; inasmuch as in the first case the muscles and nerves are found irritable six or seven hours, and more, after the experiment has been made. Therefore it may be said that the Antiar has a direct action on these organs.

These points once demonstrated, there remained one more question to elucidate, namely, whether the Antiar acts only upon the muscles, or also upon the nerves. If we consider that the Antiar un-

doubtedly paralyses the muscles, we may easily see that the loss of the excitability of the nerves possibly depends merely upon the impairment of the muscular contractility, and is therefore not real, but only apparent. With a view to determine the real state of things, I tried a third series of experiments—poisoning frogs in such a manner that the muscles of one limb were kept free from the influence of the poison. This was done in two ways: first, by putting a ligature round the crural artery and vein of one leg; and secondly, by cutting through a leg entirely, after the ligature of its vessels, with the exception only of the ischiadic nerve. In poisoning frogs treated in one of these ways, through a wound of the back, I found that, with the exception of the heart, the Antiar acts in the first instance upon the muscles. This is shown by the fact, that in the second hour, at the time when the muscles of the poisoned parts have lost their irritability, the nerves of the sacral plexus in the abdomen still possess their full influence upon the muscles of the leg which has been kept free from the action of the poison. One might be inclined from this to conclude, that the nerves are not at all acted upon by the Antiar; but this inference would be erroneous. In fact, the experiments just mentioned, if followed a little longer, show that in the third or fourth hour the sacral plexus also becomes inactive, at a time when the muscles of the non-poisoned leg are fully contractile. The Antiar, therefore, paralyses also the nervous trunks, but later than the muscles.

From all these experiments, it seems to follow that the Antiar is a poison which acts principally upon the muscular system (the heart and the voluntary muscles), a conclusion, in favour of which I may further add, that the muscles and the heart of frogs poisoned by Urari (Woorara, Curare) lose their irritability totally, and in a short time, if Antiar is introduced into a wound some time after the Urari. If we consider that, as I have shown (see Proceedings of the Royal Society, 1856, p. 201), the Urari only acts upon the terminations of the nerves in the muscles, and does not affect the irritability of the heart and muscles at all, we may conclude, that a poison, which, as the Antiar, is capable of paralysing the muscles after the Urari, has really a direct action upon the muscular fibre.

The results of my investigation into the effects of the Antiar upon frogs, are therefore the following:—

1. The Antiar is a paralysing poison.

2. It acts in the first instance and with great rapidity (in 5 to 10 minutes) upon the heart, and stops its action.

3. The consequences of this paralysis of the heart are the cessation of the voluntary and reflex movements in the first and second hour after the introduction of the poison.

4. The Antiar paralyses in the second place the voluntary muscles.

5. In the third place it causes the loss of excitability of the great nervous trunks.

6. The heart and muscles of frogs poisoned with Urari may be paralysed by Antiar.

7. From all this it may be deduced, that the Antiar principally acts upon the muscular fibre and causes paralysis of it.

So much for this time. My experiments with the Antiar upon warm-blooded animals have only begun, and I am not yet able to draw any conclusion from them. As soon as this will be possible, I shall take the liberty to submit them to the Royal Society, together with the results of my experiments with the *Upas tieuté*, which poison I had also the good fortune to obtain through the kindness of Sir Benjamin Brodie and Dr. Horsfield. With regard to the Antiar I may further add, that experiments made independently, and at the same time, by my friend Dr. Sharpey with this poison, have conducted to the same results as my own.

II. "On some Physical Properties of Ice." By John Tyndall, Ph.D., F.R.S. Received December 17, 1857.

(Abstract.)

In this paper the following points are considered:—

1. The effects of radiant heat upon ice.
2. The effects of conducted heat upon ice.
3. The air- and water-cavities of ice.
4. The effects of pressure upon ice.

For the experiments on radiant heat, slabs of Wenham Lake and Norway ice were made use of. Through these a solar beam, con-

densed by a double convex lens, was transmitted. At the moment the beam crossed the transparent solid, the track of the beam became instantly starred by little lustrous spots, like shining air-bubbles. Round each of these a figure, shaped like a flower of six petals, was formed. The petals were manifestly liquid water. When the beam was permitted to traverse different portions of the ice in succession, the sudden appearance of the stars, and the formation and growth of the flowers around them, could be distinctly observed through an ordinary pocket lens.

To test whether the brilliant spots at the centres of the flowers contained air or not, portions of ice containing them were gradually melted in warm water. The moment a liquid connexion was established between the cavities and the atmosphere, the bubbles collapsed, and no trace of air rose to the surface of the water. The formation of each liquid flower is therefore accompanied by the formation of a vacuum at its centre.

The perfect symmetry of these flowers at once enables us to infer that ice is a uniaxal crystal, the line perpendicular to the planes in which the flowers are produced being the optic axis.

For a long time during the investigation it was found that the flowers were formed in planes parallel to those of freezing; but some apparent exceptions to this rule were afterwards noticed, which are described in the paper.

In some masses of ice, apparently homogeneous, the flowers were formed on the track of the beam, in planes which were in some cases a quarter of an inch apart. This proves that the interior portions of a mass of ice may be melted by radiant heat which has traversed other portions of the mass without melting them.

In a second section of the paper the author describes the gradual liquefaction of masses of ice by the formation of drops of water within them; and he infers from his observations that the melting-point of ice oscillates within small limits on each side of the ordinary standard. Through weakness of crystalline texture, or some other cause, some portions of a mass of ice melt at a temperature slightly under 32° Fahr., while others of stronger texture require a temperature slightly over 32° to liquefy them. The consequence is, that such a mass, raised to the temperature 32°, will have some of its parts liquid and some solid.

In a third section the air- and water-cavities observed in ice are examined. These the author observed in lake ice, and they are manifestly the same as those described by M. Agassiz, the Messrs. Schlagintweit, and Mr. Huxley, as occurring in the ice of glaciers. The hypothesis of M. Agassiz and the Messrs. Schlagintweit is, that the air-bubble absorbs the heat which the ice, as a diathermanous body, has permitted to pass, the solid surrounding the bubble being liquefied by the heat thus absorbed. Mr. Huxley makes the supposition most in accordance with the facts known at the time of his observations, namely, that the water in the cavity has never been frozen. It is shown by the author that the water-cavities examined by him have been produced by the melting of the ice.

But the hypothesis of M. Agassiz and the Messrs. Schlagintweit, which appears to have received general acceptance, leads to the following consequences:—Taking the specific heat of water and of air into account, the author shows that a bubble of air, in order to raise its own volume of water 1° in temperature, must lose 3080°.

Taking the latent heat of water into account, the author shows that, to melt its own volume of ice, an air-bubble must part with $3080 \times 142 \cdot 6$, or 439,208° of temperature. Now M. Agassiz states, that when a piece of ice containing bubbles is exposed to the sun, the water formed soon exceeds the air in volume. Hence, if his hypothesis be correct, the quantity of heat absorbed by the air in the brief time of an observation, would, if it had not been communicated to the ice, be sufficient to raise the bubble to a temperature 160 times that of fused cast iron. The author further infers, from the experiments of Delaroche and Melloni, that the quantity of heat absorbed by a bubble of air at the earth's surface, after the heat has traversed our atmosphere and been sifted by it, is absolutely inappreciable. This conclusion becomes stronger when the absorption by the ice in the case before us is added to the absorption by the atmosphere.

Regarding heat as a mode of motion, the author shows that the liberty of liquidity is attained by the molecules at the surface of a mass of ice before the molecules at the centre of the mass can attain this liberty. Within the mass each molecule is controlled in its motion by the surrounding molecules. But if a cavity exist at the interior, the molecules surrounding that cavity are in a condition

similar to those at the surface; and they are liberated by an amount of motion which has been transmitted through the ice without prejudice to its solidity. The conception is helped when we call to mind the transmission of motion through a series of elastic balls, by which the last ball of the series is detached, while the others do not suffer visible separation.

The author proves, by actual experiment, that the interior portions of a mass of ice may be liquefied by an amount of heat which has been *conducted* through the exterior portions without melting them.

The converse of this takes place when two pieces of ice at 32° Fahr., with moist surfaces, are brought into contact. Superficial portions are by this act virtually transferred to the centre; and as equilibrium soon sets in between the motion of the tenuous film of moisture between the pieces of ice and the solid on each side of it, the consequence is shown to be that the film freezes, and cements the two pieces of ice together. The fourth section of the paper is devoted to these considerations.

In the fifth section a series of observations bearing upon the conductivity of ice for heat is recorded.

In the sixth section the influence of pressure upon ice is examined. A cylinder of the substance was placed between two slabs of boxwood, and subjected to a gradually-increasing pressure. Looked at perpendicular to the axis, cloudy lines were observed drawing themselves across the cylinder. Looked at obliquely, these lines were found to be the sections of dim surfaces which traversed the cylinder, and gave it the appearance of a crystal of gypsum whose planes of cleavage had been forced out of optical contact by some external force.

The surfaces are not of plates of air, for they are formed when the compressed ice is kept under water. They also commence sometimes in the centre of the mass, and spread gradually on all sides till they finally embrace the entire transverse section of the cylinder. A concave mirror was so disposed that the diffuse light of day was thrown upon the cylinder while under pressure. The hazy surfaces produced by the compression of the mass were observed to be in a state of intense commotion, which followed closely upon the edge of

the surface as it advanced through the solid. It is finally shown that these surfaces are due to the liquefaction of the ice in planes perpendicular to the pressure.

The surfaces were always formed with great facility parallel to those planes in which the liquid flowers already described are produced by radiant heat, while it is exceedingly difficult to obtain them perpendicular to these planes. Thus, whether we apply heat or pressure, the experiments show that ice melts with peculiar facility in certain directions.

The Society then adjourned over the Christmas holidays, to January 7, 1858.

January 7, 1858.

J. P. GASSIOT, Esq., Vice-President, in the Chair.

The following communications were read :—

I. "Remarks upon the Magnetic Observations transmitted from York Fort in Hudson's Bay, in August 1857," by Lieut. BLAKISTON, of the Royal Artillery. By Major-General SABINE, R.A., Treas. and V.P.R.S. Received December 16, 1857.

In the spring of 1857, Her Majesty's Government, designing to send an expedition to examine and survey the yet unsettled country north of the boundary-line between the British territory and that of the United States, and comprised between Canada on the east and the Rocky Mountains on the west, notified their intention to the Royal Society, and invited suggestions regarding any objects of physical research, for which the Royal Society might deem this to be a fitting occasion.

Amongst the subjects to which attention was called in the reply, the expediency of confirming and extending the Magnetic Survey of British North America, which, at the instigation of the Royal Society, was made in the years 1843 and 1844, and of which the results are contained in the 'Philosophical Transactions' for 1846, Art. XVII., was not forgotten ; and Lieut. Blakiston, of the Royal Artillery, personally known to Mr. Palliser, the conductor of the proposed Expedition, having been appointed to the special charge of the Magnetic Observations, and to assist generally in Geographical Determinations, the Royal Society undertook to provide the instruments suitable for the purpose, and with the sanction of the Committee of the Kew Observatory of the British Association, placed their preparation under the superintendence of Mr. Welsh, Director of that Observatory, where also Lieut. Blakiston received instructions for their use, and acquired practical experience in their manipulation. About the middle of June, Lieut. Blakiston sailed in the Hudson's Bay Com-

pany's ship the 'Prince of Wales' for York Fort, where he arrived on the 16th of August, and after completing the Magnetic Observations which he had been charged to make at that station, proceeded on the 30th of the same month, by the canoe route, to join Mr. Palliser, who had quitted England some days before him, and had taken the route by the United States to Canada and the Red River Settlement, and thence to Carlton House, where the whole party would be assembled in the fall.

The care which Lieut. Blakiston bestowed upon his determinations at York Fort appears to have been commensurate with the theoretical importance which, before he quitted England, he was aware would attach to the results. In submitting these to the Society, I must solicit a continuance of the patience and indulgence so kindly given to me on a recent occasion; for the subject of Terrestrial Magnetism is far less generally understood than I believe it deserves to be; and there is often an apparent complexity in the details, especially to those who are not familiar with the subject, which requires time to be occupied in their elucidation. I shall commence with showing the confirmation which Lieut. Blakiston's results give to the approximate accuracy of the value assigned in the 'Philosophical Transactions' for 1846, for the absolute magnetic force at its principal point of maximum in the northern hemisphere.

Those who are conversant, either from personal recollection or as a matter of history, with the opinions regarding the phenomena of terrestrial magnetism entertained in the first quarter of the present century, will scarcely need to be reminded how generally the belief then prevailed, that the magnetic dip and the intensity of the magnetic force at different points of the earth's surface might be represented with at least a sufficient approximation by mathematical formulæ, obtained by supposing the magnetism of the earth to be concentrated into two magnetic poles, very near to each other and to the earth's centre; the supposition being also equivalent to that of an infinite number of small magnets parallel to each other, distributed equally throughout the earth's surface. According to this supposition, the greatest intensity of the magnetic force in each of the two hemispheres should be found at the points where the dip should be 90°, and the intensity should vary in the proportion of 2 : 1 between places where the dip should be respectively 90° and 0°.

In the Arctic Expeditions of 1818, 1819, and 1820, I had an opportunity of measuring the intensity of the magnetic force at several stations in the immediate vicinity of the dip of 90°; and in the years 1821 and 1822, of comparing these measures with others made at several points of the coasts of Europe, Africa, and America, and at islands in the Atlantic Ocean (which I visited for the purpose of making observations with the pendulum), in dips which, including the Arctic stations, varied from 0° to 88° 47′. The result of this comparison was to place beyond a question the irreconcilability of the phenomena with the supposition of a coincidence between the points of 90° of dip and of the maximum of force. For example, the magnetic force was found to be considerably greater at New York, where the dip was not more than 73°, than at the stations in the Polar Sea where it was nearly 90°; and by graphical delineations, according to well-known methods, in which all the observations were taken into the account, it was shown that whilst the dip of 90° could not be in a more southerly latitude than 70°, the greatest intensity of the force would be found somewhere about the 53rd parallel in the vicinity of Hudson's Bay, not less than 1000 geographical miles distant from the point of 90° of dip with which it had been supposed to coincide.

The hypothesis, so generally put forward in the elementary treatises on Magnetism of that period, was therefore shown to be no longer tenable. It was in fact specially one of that class of speculations designated by Bacon as "anticipations of nature," of which it is so commonly the fate to be swept away, as knowledge advances by that more slow and gradual, but more philosophical and certain "interpretation of nature," which results from a strictly inductive process.

Steadily pursuing this last-named process, the Royal Society—after provision had been made by the establishment of Colonial Magnetic Observatories for a systematic examination of the phenomena of the variations of comparatively small amount, which are produced at the surface of our planet by the influence of other bodies of our system; and by the Antarctic Expedition of Sir James Ross, for the magnetic survey of such portions of the higher latitudes of the southern hemisphere as are accessible to navigation,—recommended to Her Majesty's Government, that in the northern hemisphere a magnetic survey

should be made of those parts of the British possessions which were adjacent to the position which observation had indicated as that of the principal maximum of the magnetic force in that hemisphere. This recommendation was carried out in 1843 and 1844, and the particulars of the survey, together with the conclusions derived from it, form No. VII. of the magnetic contributions in the 'Philosophical Transactions' for 1846, Art. XVII. The geographical position of the maximum of magnetic force derived from the combination of the 78 stations of that survey was 52° 19′ N. and 91° 59′ W. of Greenwich, and the absolute value of the force at its point of maximum was found to be 14·21 in British units (*i. e.* of mass, a grain; of time, a second; and of space, a foot). As both the geographical position of the point of maximum, and the absolute value of the force prevailing there, are subject to a secular variation, of which the nature, the period, and the epochs are desiderata of the highest theoretical importance,—and as the determinations which are now made may therefore probably be referred to as data by remote posterity,—their confirmation, by the observations of a second observer visiting the same localities within a few years of the same date, furnished with different instruments, and pursuing in some respects different methods, was viewed as a circumstance much to be desired by the Committee of the Royal Society appointed, at the request of Her Majesty's Government, to suggest scientific desiderata, to be accomplished by Mr. Palliser's North American Expedition.

York Fort had been one of the stations visited by Lieut. (now Lieut.-Col.) Lefroy, in the Survey of 1843–44. It is situated nearly due north of the point of maximum deduced from that survey, and less than 300 miles distant from it. The intensity of the force at York Fort in July 1843, derived from the combined observations of the inclination and of the horizontal force observed by Gauss's well-known absolute method, was 14·07; and by Mr. Fox's statical apparatus, taking Toronto as a base, 14·03. We have now to compare with these Lieut. Blakiston's results in August 1857, viz. 14·024 by the combination of the inclination and the absolute horizontal force, and 14·017 by a recent improvement of Dr. Lloyd's statical method, which renders the result independent of changes which may take place in the magnetic moment of the needle employed in the determination. The first of these two last-named results has been computed by Mr.

Welsh, of the Kew Observatory, from the observations received from Lieut. Blakiston, who was too much pressed for time by his approaching departure from York Fort to compute them himself. The second is the mean of five determinations on three different days, which were computed by himself on the spot; they are severally as follows:—

August	20th, noon......	14·03
,,	20th, 3 P.M.....	14·01
,,	22nd, 5 P.M.....	14·024
,,	24th, noon......	14·00
,,	24th, 3 P.M.....	14·02
	Mean..........	14·017

We have therefore by the mean of the two methods in 1843, 14·05, and by the mean of the two methods in 1857, 14·02, differing only about $\frac{1}{470}$th from each other. As far, therefore, as agreement *at a single station* may be regarded as confirming the conclusions of the survey of 1843–44, Lieut. Blakiston's results furnish that confirmation; and judging from the result at the first station at which the comparison has been made, we may anticipate, from the opportunities which he is likely to have of repeating observations at other stations of the former survey, as well as of adding stations previously unvisited, that the ultimate conclusion in respect to the absolute value of the magnetic force at its point of maximum at this particular magnetic epoch, will be as perfect as could be desired. With respect to its present geographical position, we may also hope that Lieut. Blakiston may have an opportunity, before his employment is terminated, of removing any doubts that may exist as to the precision of the *longitude* assigned to it by the survey of 1843–44. It cannot have escaped notice that the 78 stations of that survey, which by their combination assigned the latitude and longitude of the point of maximum, did not perfectly fulfil one important condition regarding their distribution, viz. that of symmetrical arrangement on all sides of the point in question. There was a considerable preponderance of stations situated on the west of the meridian of the point itself, and a deficiency on the eastern side, which might have been remedied, had circumstances permitted, by a line of stations as originally contemplated on the canoe route from Canada to Moose

Fort at the south-western end of Hudson's Bay, and possibly by some additional stations between Moose Fort and York Fort. The experience which Lieut. Blakiston has had in canoe-travelling will have prepared him to profit by the opportunities it may afford for observation, and the route referred to is one of the ordinary canoe routes of the Hudson's Bay Company: with this addition, the determinations of geographical position and of the value of the magnetic force at its point of maximum, may be expected to be amongst the most perfect, as they will undoubtedly be amongst the most important data, in this great branch of Physical Geography.

I proceed to notice Lieut. Blakiston's observations upon the magnetic declination at York Fort, which, taken in conjunction with those of the survey in 1843–44, tend to substantiate conclusions of no less theoretical importance than those with which we have been occupied regarding the magnetic force. It is well known to those who are conversant with the phenomena of the *secular change* of the declination, that during the whole of the last century, and for some time after the commencement of the present century, the secular change which took place in the position of the isogonic lines in the northern parts of the North American continent, consisted in the progressive translation of the lines from west to east. The line of no declination, for example, to which, when Halley collected and coordinated the most trustworthy observations previous to the publication of his Magnetical Map in 1702, he assigned a position "about the meridian of the middle of California" (Phil. Trans., No. 148), appears in Hansteen's 'Mappa hydrographica sistens Declinationes magneticas Anni 1787' (Erdmagnetismus, Atlas), at the latter epoch, as crossing Lake Superior, and proceeding from thence in a direction west of north, so as to pass altogether to the west of any part of Hudson's Bay; whilst from well-assured observations of a still later date we know that soon after the beginning of the present century, places situated on the western shores of Hudson's Bay had *east* declination, showing that the line of no declination had passed over and was now to the east of them. Consistently with this general movement of the isogonic lines from west to east, the declination at York Fort, which, according to the observations of Capt. Middleton (Phil. Trans. 1726, No. 393, and 1731, No. 418) was at least 19° West in 1725, had diminished to about 5° West in 1787 (Hansteen, *l. c.*). In Septem-

ber 1819 it was found by Sir John Franklin to be 6° *East* (Journey to the Shores of the Polar Sea, 1819–22, p. 26), and by Lieut.-Colonel Lefroy in 1843, 9° 25′ East. Thus we perceive that in little more than a century (from 1725 to 1843) the declination at York Fort had changed progressively, by the operation of secular change, not less than 28°, always in the direction of westerly decreasing or easterly increasing; (which is in effect the same as a movement of translation of the isogonic lines from west to east).

In 1841 the Toronto Observatory commenced its observations, and although (from defective instrumental organization) the conclusions in regard to the secular change of the declination were not *at first* as precise as could be desired, they were sufficiently so to justify a strong persuasion that some very notable change had recently taken place in the order of the phenomena, and to lead to the commencement, in January 1845, of a special series of monthly determinations in a detached building, appropriated chiefly to a close investigation into the direction and amount of the secular change. The result is stated in the 3rd volume of the Toronto Observations, p. cxxvi, and is as follows:—"The secular change of the declination from 1845 to 1851 inclusive was an annual increase of 1′·95 of west declination. From July 1851 to April 1854 (two years and nine months) an annual increase of 2·54: and assuming the circumstances of a new series commenced in 1855 with the same instrument placed in a new building to be strictly comparable with those of the old series, the increase from April 1854 to October 1855 is at the mean annual rate of 3′·54." The progressively increasing amount of the rate of secular change is a circumstance which, for obvious reasons, may be expected to follow for a time after the reversal of the direction of the change.

Attention being thus alive, particular care was taken that the azimuth compass with which Lieut. Blakiston was supplied should be free from instrumental error, and the practice was recommended to him of repeating observations at different hours and on different days. The following is a transcript of the report received from him from York Fort, showing how thoroughly these directions were kept in view:—

"Declination at York Fort, 1857.

	h m	° ′
17th August,	5 30 P.M.	7 01 E.
"	5 43 P.M.	7 21 E.
"	6 14 P.M.	7 43 E.
20th August,	5 16 P.M.	7 41 E.
"	5 53 P.M.	7 24 E.
26th August,	5 54 A.M.	8 01 E.
"	6 40 A.M.	7 57 E.
"	7 20 A.M.	7 50 E.
	Mean.	7 37 E.

"Ten to twelve observations in each set, the compass being lifted and shaken between each observation."

	° ′
The observations of Franklin in September 1819 gave	6 00 E.
Those of Lefroy in July 1843 gave	9 25 E.
Those of Blakiston in August 1857 gave	7 37 E.

It appears therefore that the secular variation which between 1819 and 1843 caused an *increase* of east declination, caused on the contrary between 1843 and 1857 a *decrease* of east declination. This is a reversal in the same sense as that which has been seen to have taken place at Toronto. It seems probable from an inspection of the intervals, and of the differences of declination-value, in the three determinations above noticed, that the epoch of reversal must have coincided very nearly with that of the survey of 1843–44; and consequently that Lieut.-Col. Lefroy's result may show approximately the maximum which the easterly declination attained at York Fort before the change took place. If we might assume 1843 to be the precise epoch, it is deserving of remark that it is the same year in which the observations of the inclination at Toronto show that the annual secular variation of that element changed from a decreasing to an increasing rate. The dip observed by Lieut. Blakiston at York Fort was 83° 53′ in 1857, and by Lieut.-Col. Lefroy 83° 47′·2 in 1843, showing, as at Toronto, a slight increase to have taken place in that element in the interval.

I am indebted to Dr. Norton Shaw, Secretary of the Royal Geographical Society, for a copy of declinations observed by Mr. Palliser in his passage between Fort William and the Red River Settlement.

It happens that four of the stations in this route, at which Mr. Palliser observed the declination in the summer of 1857, had been stations of Lieut.-Col. Lefroy in 1843–44. They are as follows :—

	Lat.	Long.	Declination. 1843–44.	1857.
	° ′	° ′	° ′	° ′
Savannah Portage......	48 53 N.	90 05 W.	7 46 E.	6 53 E.
Fort Francis...........	48 36	93 30	9 36	9 31
Lake of the Woods	49 27	94 44	12 16	10 17
Lake Winipeg	50 28	96 35	15 30	14 25
		Means......	11 17 E.	10 14 E.

At all the stations the easterly declination is less in 1857 than in 1843–44; and on the average of the four stations it would appear to have decreased about 1° in the fourteen years.

It would be unjust to the memory of the profound and sagacious philosopher, by whom, more than 150 years ago, the facts both of the magnetic declination in different parts of the globe and of its changes were first collected and framed into an hypothesis (Halley in Phil. Trans. 1692, No. 193), if we were to fail to recognize that this reversal in the direction of the motion of the isogonic lines, in the vicinity of the principal magnetic pole in the northern hemisphere (using the term 'pole' in the physical sense in which Halley employed it), is conformable to the hypothesis which he propounded at that early date,—" to explain," according to his own words, " the change in the variation (declination) of the magnetic needle." By the supposition of a double system of the terrestrial magnetic forces, occasioning two poles or principal points of attraction in each hemisphere producing resultant phenomena in all parts of the surface of the globe according to their relative strength and proximity, Halley showed that all the apparently complex phenomena of the magnetic direction might be systematically represented; and by the further supposition that one of the two systems (the stronger one) was fixed, and the other (the weaker one) possessed a gradual and slow motion, that a reasonable explanation could be given of the phenomena of the secular change in different parts of the globe, as far as they were known in his time. At the period when this hypothesis was originated, viz. in 1692, the two poles in the northern hemisphere were considered to be situated as follows: that of the stronger and fixed

system in North America, about the meridian of the middle of California, and that of the weaker and moving system, about the meridian of the British Islands, having a progressive motion towards the east. Now as the resultant phenomena in the north of America, though influenced principally by the nearer and stronger system, would still exhibit in a slighter degree the influence of the weaker and moving system, the isogonic lines in that part of the globe should have, according to the hypothesis, a movement of translation from west to east conformably to the motion of the weaker system, until the difference in longitude between the poles of the respective systems should amount to 180°, an event which would constitute an epoch in the secular magnetic variations, characterized (amongst other circumstances) by the reversal of the motion of the isogonic lines in America, which would thenceforward take place from east to west, as the distance between the poles should diminish on the Siberian side of what Halley termed the American Pole. Now it is well known that the expedition of MM. Hansteen, Erman, and Due, across the continents of Europe and Asia in 1828 and 1829, had, for its principal object, the determination of the magnetic phenomena around the point of maximum attractive force of the weaker or moving system; and that the position those gentlemen assigned to it in longitude at the time of this expedition was about 115° East of Greenwich, to which meridian it had progressively moved in the interval which had elapsed since Halley assigned its position near the meridian of our Islands. Fully recognizing that in the present, as in the earlier state of magnetical science we can only regard such assignments as approximate, we have still full reason to believe that about the time of the memorable expedition of MM. Hansteen, Erman, and Due, *i. e.* a few years earlier or a few years later than 1828–29, the epoch must have occurred when the points of greatest attraction of the two systems in the northern hemisphere must have passed through their greatest longitudinal distance from each other, and when, according to Halley's hypothesis, the direction of the movement of translation of the isogonic lines in the northern parts of America should be reversed, which we find to have now taken place.

I have ventured to think that these few remarks, recalling to recollection an hypothesis which was not framed without a most laborious coordination and sagacious grouping of the phenomena

which it professed to represent, and which has its place in the earlier volumes of our Transactions, would not be unacceptable to the Members of the Royal Society, of which Society Halley has ever been regarded as one of the brightest ornaments.

II. "On the Isolation of the Radical, Mercuric Methyl." By George Bowdler Buckton, Esq., F.R.S. Received December 4, 1857.

(Abstract.)

Dr. Frankland, in his valuable memoir communicated to the Royal Society, has pointed out that hydrargyro-methylium, zinc-ethylium, and analogous bodies may be regarded as formed upon the type of the metallic oxides, the oxygen of which he considered was represented by methyl, ethyl, &c. The hypothetical radical hydrargyro-methylium, $C_2\,H_3\left\{\begin{matrix}Hg\\Hg\end{matrix}\right.$, according to this view would correspond to numerous oxides, $O\left\{\begin{matrix}Hg.\\Hg.\end{matrix}\right.$

Dünhaupt and Strecker have studied and described the salts of hydrargyro-methylium and hydrargyræthylium, but chemists do not appear, hitherto, to have succeeded in reducing these bodies to the mercuric type, or in preparing the metalloids themselves.

The author has undertaken experiments with a view to the completion of this portion of their history, a brief summary of which he now offers.

Iodide of hydrargyro-methylium was prepared through the agency of sunlight, in the usual manner; and after the removal of every trace of iodide of methyl, it was intimately mixed in a mortar with finely powdered cyanide of potassium. Small charges were then introduced into flasks and distilled over the gas flame. Gaseous and solid products are formed, together with a heavy liquid, which passes into the receiver. After washing with water, and rectification over chloride of calcium, this liquid has the following properties:—

It is colourless, highly refractive to light, and almost wholly insoluble in water. When pure, it has a faint and somewhat sweetish odour. It is very combustible, and burns with a luminous flame and abundant evolution of mercurial vapour. It is very soluble in

alcohol and in ether, from the former of which it precipitates on addition of water. Its boiling-point lies between 93° and 96° C, and its specific gravity is 3·069. It thus appears to have a weight greater than any known non-metallic liquid at ordinary temperature.

By analysis it gave numbers according with the formula

$$C_2H_3Hg.$$

The formation of this body is readily intelligible from the following equation, if we neglect secondary decompositions,—

$$C_2H_3Hg_2, I+KCy=C_2H_3Hg+KI+Cy+Hg.$$

The cyanogen does not, however, appear as liberated gas, but remains behind in the form of paracyanogen.

From the constitution of this substance, the name mercuric methyl is proposed. Should this appellation be accepted, Dr. Frankland's radical would be styled mercurous methyl.

To control the analysis, and further corroborate the formula, the specific gravity of the vapour was taken after Dumas's method. It was found to be 14·86. The weight represented by the formula C_2H_3Hg, divided by the experimental density, gives the quotient 7·73. Supposing the constituents of mercuric methyl condensed into one volume of vapour, the number 7·23 should have been obtained.

The theoretical density of mercuric methyl is $\frac{115}{7{\cdot}23}=15{\cdot}9$. It is, however, more probable that the elements of this compound are condensed into two volumes, whence the formula should be doubled to $(C_2H_3)_2Hg_2$.

Mercuric methyl may also be obtained, but less readily, by employing hydrate of potassa or lime, instead of an alkaline cyanide.

In this reaction much gas is liberated.

$$2(C_2H_3Hg_2,I)+2KOHO=C_2H_3Hg+C_2H_4+3HgO+HO+2KI.$$

Mercuric methyl exhibits no tendency to unite with the electro-negative elements, such as chlorine, oxygen, &c. All attempts to produce such combinations lead to the destruction of the substance. With iodine or bromine the liquid hisses as if hot metal were plunged into water. Methyl gas is liberated, and the iodide or bromide of mercurous methyl is produced:—

$$\left.\begin{matrix} C_2H_3Hg \\ C_2H_3Hg \end{matrix}\right\} + I = \underbrace{C_2H_3Hg_2\ I}_{\text{Iodide of mercurous methyl.}} + C_2H_3.$$

On the other hand, the action of concentrated sulphuric or hydrochloric acid furnishes hydride of methyl or marsh gas, with deposition of crystals of the corresponding chloride or sulphate.

$$\left.\begin{matrix} C_2H_3Hg \\ C_2H_3Hg \end{matrix}\right\} + HCl = C_2H_3Hg_2Cl + C_2H_3, H.$$

The salts of mercurous methyl, and the radical mercuric methyl, are both decomposed by the action of a dilute acid and clean zinc, into metallic mercury and gases.

Mercuric methyl furnishes with bichloride of tin a crystalline compound, which decomposes, on addition of water, into chloride of mercurous methyl and a soluble tin salt. The same chloride also is produced by the action of terchloride of phosphorus.

Mercuric methyl is a ready solvent of caoutchouc, resins, and phosphorus. It, however, has but little solvent action on sulphur.

Some interest attaches to the circumstance that iodide of mercurous methyl is easily produced by heating mercuric iodide with mercuric methyl.

Mercuric ethyl.

The author has also prepared the radical of mercuric ethyl. From its proneness, however, to decomposition at the high temperature at which the reaction is effected, he has not been able to obtain more than sufficient to make a qualitative examination of the new body. It boils at a temperature above that of water; and burns with a more lurid flame than is exhibited by mercuric methyl.

III. "On Certain Formulæ for Differentiation." By Arthur Cayley, Esq., F.R.S. Received November 26, 1857.

(Abstract.)

In seeking for a formula in the theory of multiple definite integrals, I was several years ago led to investigate the successive differential coefficients of $(\sqrt{x+\lambda}-\sqrt{x+\mu})^{2i}$, and the results which I then obtained are given in my paper, "On certain formulæ for differentiations, with applications to the evaluation of definite inte-

grals*." I subsequently sought for the successive differential coefficients of the more general expression $\{(x+\lambda)(x+\mu)\}^{\frac{1}{2}k}(\sqrt{x+\lambda} - \sqrt{x+\mu})^{2i}$, but the investigation was not finished. My attention was recalled to the subject by two remarkable identities obtained in Prof. Donkin's memoir, "On the equation of Laplace's Functions, &c.†," by a comparison of his results with those of Prof. Boole, which identities I perceived to belong to the class of formulæ above referred to: the first of the two identities is in fact readily deduced from a formula in my paper; the demonstration of the second is much more difficult, and I have only succeeded in making it depend on the establishment of the equality of the coefficients of two expressions of the same form. I have since resumed the unfinished investigation above referred to. The several results which I have obtained are given in the present memoir. I remark that, putting for shortness $P=2x+\lambda+\mu$, $Q=\sqrt{(x+\lambda)(x+\mu)}$, $R=(\sqrt{x+\lambda}-\sqrt{x+\mu})^2$, the subject to which the results all belong is the differentiation of the expression $P^\alpha Q^\beta R^\gamma$; the before-mentioned expression $\{(x+\lambda)(x+\mu)\}^{\frac{1}{2}k}(\sqrt{x+\lambda}-\sqrt{x+\mu})^{2i}$ is of this form, and the question in relation to it is to obtain the development of $\partial_x^r P^\alpha Q^\beta R^\gamma$, where $\alpha=0$. The question arising from the second of Prof. Donkin's identities is to obtain the development of $(P^{-1} Q_4 \partial_x)^\gamma P^\alpha Q^\beta R^\gamma$, where $\alpha=\gamma-\beta$. As the demonstration of these identities is one of the objects of the present memoir, I have given in the first section their reduction to the form in which they are considered. The second section treats of the development of the expression $\partial_x^r P^\alpha Q^\beta R^\gamma$ where $\alpha=0$; the third section of that of the expression $\{P^{-1} Q^4 \partial_x\}^r P^\alpha Q^\beta R^\gamma$ where $\alpha=\gamma-\beta$; the fourth section contains the application of the formulæ to the demonstration of the two identities and some other applications of the formulæ.

* Cambridge and Dublin Mathematical Journal, t. ii. pp. 122, 128 (1847).

† Philosophical Transactions, 1856, pp. 43–57.

January 14, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "On the Electric-Conducting Power of the Metals." By AUGUSTUS MATTHIESSEN, Ph.D. Communicated by C. WHEATSTONE, Esq. Received November 20, 1857.

(Abstract.)

The following values for the conducting power of the metals were determined in the Physical Laboratory at Heidelberg, under the direction of Professor Kirchhoff, by the same method as is described in the 'Philosophical Magazine,' Feb. 1857.

	Conducting Power	at Temp. in Celsius's degrees.
Silver	100	0°
Copper, No. 3.	77·43	18·8
Copper, No. 2.	72·06	22·6
Gold	55·19	21·8
Sodium	37·43	21·7
Aluminium	33·76	19·6
Copper, No. 1.	30·63	24·2
Zinc	27·39	17·6
Magnesium	25·47	17·0
Calcium	22·14	16·8
Cadmium	22·10	18·8
Potassium	20·85	20·4
Lithium	19·00	20·0
Iron	14·44	20·4
Palladium	12·64	17·2
Tin	11·45	21·0
Platinum	10·53	20·7
Lead	7·77	17·3
Argentine	7·67	18·7
Strontium	6·71	20·0
Antimony	4·29	18·7

	Conducting Power at	Temp. in Celsius's degrees.
Mercury	1·63	22·8
Bismuth	1·19	13·8
Alloy of Bismuth 32 parts / Antimony 1 part	0·884	24·0
Alloy of Bismuth 12 parts / Tin 1 part	0·519	22·0
Alloy of Antimony 2 parts, Zinc 1 part	0·413	25·0
Graphite, No. 1	0·0693	22·0
Graphite, No. 2	0·0436	22·0
Gas-coke	0·0386	25·0
Graphite, No. 3	0·00395	22·0
Bunsen's Battery-Coke	0·00246	26·2
Tellurium	0·000777	19·6
Red Phosphorus	0·00000123	24·0

All the metals were the same as those used for my thermo-electric experiments, with the exception of cadmium, which was purified by my friend Mr. B. Jegel.

The alloys of bismuth-antimony, bismuth-tin, antimony and zinc were determined in order to ascertain whether, as they give, with other metals, such strong thermo-electric currents, they might be more advantageously employed for thermo-electric batteries than those constructed of bismuth and antimony.

Coppers No. 1, 2, 3 were wires of commerce. No. 1 contained small quantities of lead, tin, zinc, and nickel. The low conducting power of No. 1 is owing, as Professor Bunsen thinks, to a small quantity of suboxide being dissolved up in it.

Graphite No. 1 is the so-called pure Ceylon; No. 3 purified German, and No. 2 a mixture of both. The specimens were purified by Brodie's patent and pressed by Mr. Cartmell, to whom I am indebted for the above.

The conducting power for gas-coke, graphite, and Bunsen's battery-coke increases by heat from 0° to 140° C.; it increases for each degree 0·00245, *i. e.* at 0° C. the conducting power =100, and

between the common temperature and a light red heat about 12 per cent. The following metals were chemically pure :—Silver, gold, zinc, cadmium, tin, lead, antimony, quicksilver, bismuth, tellurium. Those pressed were sodium, zinc, magnesium, calcium, cadmium, potassium, tin, lead, strontium, antimony, bismuth, tellurium, and the alloys of bismuth-antimony and bismuth-tin. The way in which these wires were made is described in the 'Philosophical Magazine' for February 1857.

II. "On the Thermo-electric Series." By Augustus Matthiessen, Ph.D. Communicated by Charles Wheatstone, Esq. Received November 20, 1857.

[Abstract.]

Being enabled by the method described in the 'Philosophical Magazine' (Feb. 1857) to obtain wires of the metals of the alkalies and alkaline earths, I have determined their places, together with those of most of the other metals, in the thermo-electric series.

If A, B, C are different metals, and (AB), (BC), (CA) the electromotive powers of thermo-elements formed out of each two of these metals, whose alternate soldering points are at two different temperatures, so is $(AB) + (BC) + (CA) = 0$, and therefore

$$(AB)=a-b,$$
$$(BC)=b-c,$$
$$(CA)=c-a,$$

where the values a, b, c not only depend on the two temperatures, but also on the nature of each of the metals A, B, C. As the differences of the same constitute the electromotive powers, the value for either of these metals may be put $= 0$.

If the temperatures of the soldering points of a thermo-element only vary slightly, the electromotive powers may be said to be in ratio with the difference of the two temperatures, and under the same conditions the values a, b, and c are also in ratio with the difference of the temperatures, and their relations to each other therefore independent of the same.

If now the value of the second metal relative to the above value of the first be taken equal to 1, the values of the others, in relation to these, become constants, and only depend on the nature of each metal; these values I will call the Thermo-electric Constants. The results obtained are given in the following Table, where the thermo-electric constant of chemically pure silver is taken = 0, and that of a certain commercial sort of copper = 1.

Bismuth (commercial, pressed wire)	+35·81
Bismuth (pure, pressed wire)	+32·91
Alloy of 32 parts of bismuth and 1 part of antimony (cast)	+29·06
Bismuth (pure, cast)	+24·96
Bismuth (crystal, axial)	+24·59
Bismuth (crystal, equatorial)	+17·17
Cobalt No. 1 (a pressed specimen prepared by Professor Duflos, and out of the Collection of the Heidelberg Chemical Laboratory)	+8·977
Potassium (the same as used for the determination of its electric conducting powers for different temperatures)	+5·492
Argentine (wire of commerce, hard)	+5·240
Nickel (commercial, free from cobalt, but containing iron, &c.)	+5·020
Cobalt No. 2 (from the Collection of the Heidelberg Chemical Laboratory)	+3·748
Palladium (wire, hard, from Desmoutis, Chapuis and Co. of Paris)	+3·560
Sodium (the same as used for the determination of its electric conducting powers for different temperatures)	+3·094
Quicksilver (pure, fused in a glass tube)	+2·524
Aluminium (from Rousseau frères of Paris, wire-drawn, analysed by Dr. G. C. Caldwell, and found to contain Si 2·34, Fe 5·89, and Al 91·77)	+1·283
Magnesium (wire, pressed)	+1·175
Lead (pure, pressed wire)	+1·029
Tin (pure, pressed wire)	+1·000
Copper No. 1 (wire of commerce annealed, containing appreciable quantities of zinc, tin, lead and nickel)	+1·000
Copper No. 2 (wire of commerce annealed)	+0·922
Platinum (wire from Desmoutis, Chapuis and Co. of Paris)	+0·723

Gold (wire, hard drawn, purified by Dr. C. Meyboom) ..	+ 0·613
Iridium (from the Collection of the Heidelberg Chemical Laboratory)	+ 0·163
Antimony (wire, pressed specimens, purified by Dr. W. P. Dexter and Dr. G. C. Caldwell)	+ 0·036
Silver (pure, drawn, hard)	+ 0·000
Gas-coke (from the Heidelberg Gas-Manufactory, the hard mass remaining in the retorts)	− 0·057
Zinc (pure, pressed)	− 0·208
Copper (galvanoplastically precipitated)	− 0·244
Cadmium (a strip of foil from Prof. Böttger)	− 0·332
Antimony (commercial, pressed wire)	− 1·897
Strontium (pressed wire)	− 2·028
Lithium (pressed wire)	− 3·768
Arsenic (a piece, pure)	− 3·828
Calcium (pressed wire)	− 4·260
Iron (pianoforte wire No. 4)	− 5·218
Antimony (axial)	− 6·965
Antimony (equatorial)	− 9·435
Red phosphorus (from Prof. Schrötter, from the Collection of the Heidelberg Chemical Laboratory)	− 9·600
Antimony (purified as above)	− 9·871
An alloy of 12 parts of bismuth and 1 part of tin	−13·670
An alloy of 2 parts of antimony and 1 part of zinc	−22·700
Tellurium (from M. Alexander Loewe, purified by M. Holtzmann)	−179·80
Selenium (from the Collection of the Heidelberg Chemical Laboratory)	−290·00

The method by which these determinations were made is the following:—Two thermo-elements, whose warm and cold soldering points had the same temperatures, were compared with each other; these formed a circuit with the coil of a multiplicator, which surrounded a magnet rod (of about a pound weight) to which was fastened a piece of looking-glass, thereby allowing the deflections of the magnet to be observed at a distance by means of a telescope and scale, in the same manner as observations are made with the magnetometer. Two commutators were also brought into the circuit; the one changed the direction of the current in the wire of the multipli-

cator, the other allowed the currents of the thermo-elements to pass either so as to strengthen, or so as to oppose each other.

The foregoing experiments were carried out in the Physical Cabinet at Heidelberg, under the direction of Professor Kirchhoff, to whose advice and assistance I am much indebted.

III. "A Memoir on the Theory of Matrices." By Arthur Cayley, Esq., F.R.S. Received December 10, 1857.

[Abstract.]

The term matrix might be used in a more general sense, but in the present memoir I consider only square and rectangular matrices, and the term matrix used without qualification is to be understood as meaning a square matrix; in this restricted sense, a set of quantities arranged in the form of a square, *e. g.*

$$\begin{pmatrix} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{pmatrix}$$

is said to be a matrix. The notation of such a matrix arises naturally from an abbreviated notation for a set of linear equations, viz. the equations

$$\begin{aligned} X&=ax+by+cz \\ Y&=a'x+b'y+c'z \\ Z&=a''x+b''y+c''z \end{aligned}$$

may be more simply represented by

$$(X, Y, Z)=\begin{pmatrix} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{pmatrix}(x, y, z)$$

and the consideration of such a system of equations leads to most of the fundamental notions in the theory of matrices. It will be seen that matrices (attending only to those of the same degree) comport themselves as single quantities; they may be added, multiplied, or compounded together, &c.: the law of the addition of matrices is precisely similar to that for the addition of ordinary algebraical quantities; as regards their multiplication (or composition), there is the peculiarity that matrices are not in general convertible; it is nevertheless possible to form the powers (positive or negative, integral or

fractional) of a matrix, and thence to arrive at the notion of a rational and integral function, or generally of any algebraical function of a matrix. I obtain the remarkable theorem that any matrix whatever satisfies an algebraical equation of its own order, the coefficient of the highest power being unity, and those of the other powers functions of the terms of the matrix, the last coefficient being in fact the determinant. The rule for the formation of this equation may be stated in the following condensed form, which will be intelligible after a perusal of the memoir, viz. the determinant, formed out of the matrix diminished by the matrix considered as a single quantity involving the matrix unity, will be equal to zero. The theorem shows that every rational and integral function (or indeed every rational function) of a matrix may be considered as a rational and integral function, the degree of which is at most equal to that of the matrix, less unity; it even shows that in a sense, the same is true with respect to any algebraical function whatever of a matrix. One of the applications of the theorem is the finding of the general expression of the matrices which are convertible with a given matrix. The theory of rectangular matrices appears much less important than that of square matrices, and I have not entered into it further than by showing how some of the notions applicable to these may be extended to rectangular matrices.

IV. "A Memoir on the Automorphic Linear Transformation of a Bipartite Quadric Function." By Arthur Cayley, Esq., F.R.S. Received December 10, 1857.

[Abstract.]

The question of the automorphic linear transformation of the function $x^2+y^2+z^2$, that is the transformation by linear substitutions, of this function into a function $x_i^2+y_i^2+z_i^2$ of the same form, is in effect solved by some formulæ of Euler's for the transformation of coordinates, and it was by these formulæ that I was led to the solution in the case of the sum of n squares, given in my paper "Sur quelques propriétés des déterminants gauches," Crelle, t. xxxii. pp. 119–123 (1846). A solution grounded upon an *à-priori*

investigation and for the case of any quadric function of n variables, was first obtained by M. Hermite in the memoir "Remarques sur une Mémoire de M. Cayley relatif aux déterminants gauches," Cambridge and Dublin Mathematical Journal, t. ix. pp. 63–67 (1854). This solution is in my Memoir "Sur la transformation d'une fonction quadratique en elle-même par des substitutions linéaires," Crelle, t. l. pp. 288–299 (1855), presented under a somewhat different form involving the notation of matrices. I have since found that there is a like transformation of a bipartite quadric function, that is a lineo-linear function of two distinct sets, each of the same number of variables, and the development of the transformation is the subject of the present memoir.

V. "On some of the Products of the Destructive Distillation of Boghead Coal."—Part II. By C. Greville Williams, Esq., Lecturer on Chemistry in the Normal College, Swansea. Communicated by Professor Stokes, Sec. R.S. Received December 17, 1857.

[Abstract.]

In this paper the author describes the method adopted by him for the separation of the three classes of hydrocarbons forming the more volatile portion of the distillate. On treatment with bromine in presence of water, the naphtha is entirely converted into a heavy oil, containing the $C^n H^n$ series chemically, and propyle and benzole mechanically combined. The two latter may be removed by mere distillation on the water-bath. They are easily separable by fuming nitric acid, the benzole being dissolved while the propyle is untouched. The nitro-benzole obtained in this manner, on treatment by Béchamp's process, yields aniline mixed with a little toluidine, but no bases belonging to any other class.

The bromine compound (in consequence of its preparation in presence of water) could not be obtained free from oxygen. When kept for some time it separates into three layers, the upper being water faintly acidulated with hydrobromic acid, the middle bromine compound, and the lower, hydrobromic acid of 37 per cent., and the

density 1·320. The bromine compounds, treated successively with alcoholic potash and sodium, undergo a curious decomposition, the original hydrocarbons, from which they were derived, being regenerated. The brominated oil from the naphtha, boiling between 71° and 77°, affords hexylene boiling at 71°, and the oil from the next homologue distilling between 82° and 88°, yields heptylene boiling at 99°. The annexed Table illustrates some of their physical properties.

Physical Properties of Hexylene and Heptylene from Boghead Naphtha.

	Formula.	Boiling-point.	Density at 18°.	Density of Volume.	
				Expt.	Theory.
Hexylene	$C^{12} H^{12}$	71°	....	3·020	2·904
Heptylene	$C^{14} H^{14}$	99°	0·718	3·320	3·386

VI. "On the Electrical Nature of the Power possessed by the Actiniæ of our Shores." By Robert M'Donnell, M.D., M.R.I.A., Lecturer on Anatomy and Physiology in the Carmichael School of Medicine, Dublin. Communicated by William Bowman, F.R.S., Surgeon to King's College Hospital and the Royal London Ophthalmic Hospital. Received November 30, 1857.

After referring to the well-known phenomena manifested by electrical fishes, and to alleged instances of numbing effects, but of doubtful electrical nature, produced on the naked hand by the contact of certain marine Invertebrata, the author describes his own observations and experiments with the Actinia as follows :—

Suppose that into a vessel containing some actiniæ well expanded, and apparently on the look-out for food, some of the tadpoles of the common frog be introduced, these little creatures do not, like many freshwater fishes of about the same dimensions, immediately die; on the contrary, the salt water seems to stimulate their activity, they become very lively and swim about with vivacity. One of them may not unfrequently be observed to make its way among the tentacles of

an actinia and get off again quite uninjured; it may even for a time nestle among the tentacles with as much impunity as if it were only in contact with a piece of sea-weed; but should the tadpole have the misfortune to fall in with a more voracious actinia, the reception it meets with is very different. Sometimes, when by an incautious lash of its tail it touches even a single tentacle, it may at once be laid hold of, and in the violent efforts which it forthwith makes to break loose, often merely brings itself within the reach of other tentacles, by which it is seized and overpowered. Occasionally, however, after having been thus seized, the tadpole by its superior activity succeeds in effecting its escape, and when it does so, it seems for a time singularly excited; it twists and writhes and wriggles through the water, so as to leave no doubt that some very remarkable influence has been exerted upon it.

These observations are no doubt familiar to all who have studied the habits of these animals; for although the tadpole seems more susceptible of the peculiar stimulus which the actinia can communicate than most of those creatures which are ordinarily cast in its way, yet the same occurrences take place with the small crustaceans, &c. which are abundant in sea-water. Indeed no very close attention is necessary to perceive, that while on some occasions these little animals may creep to and fro over the surface and among the tentacles of the actinia, at other times they are seized and killed with the greatest promptitude.

It remained to be determined what is the exact nature of the power which the actinia has been thus found to have under its control. If it seized its victim by a simple mechanical effort, why should the tadpole be so agitated for some time after having escaped from its grasp? No peculiarly viscid secretion could be detected on the tentacles, nor could any decided reaction be discerned on their surface differing from the feebly alkaline condition of the sea-water in which they were placed; moreover, the power of the actinia seemed often to be exerted with too much promptness to be compatible with the notion of the formation of a poisonous or stinging fluid over its surface.

On the hypothesis that it is an electrical power with which the actiniæ are endowed, it is obvious that the existence of animal electricity in them ought to be experimentally demonstrable by its

physiological effects, inasmuch as these phenomena are the most striking which animal electricity is capable of producing in common with other electricities derived from different sources.

The following experiments, in which the frog's limb was used as a galvanometer (the limb of this animal being, as is well known, an instrument of extreme delicacy for this purpose), seem satisfactorily to establish the fact that the common actiniæ of our shores are gifted with electrical power.

1st. Having prepared the lower limb of a lively frog after the mode described by Matteucci, by stripping off the skin, dissecting out the sciatic nerve from among the muscles of the thigh, and then cutting off the thigh a little above the knee, so as to leave the nerve uninjured and as long as possible, the limb was laid on a small piece of glass, so that the nerve hung down over its edge. The pendent nerve was lowered into the water and gently brought in contact with the tentacles of an expanded actinia. From the first or the second, or even several, possibly no effect may result, but arriving at last at one more vigorous than his neighbours, smart muscular contractions follow as he grasps the nerve in his tentacles, and the toes are thrown into active movement.

2nd. The next experiment, although of precisely the same nature as that first detailed, renders the effect produced on the muscles of the frog's limb more striking. A large and lively frog is killed, the skin is stripped off, and the viscera being removed, the body is cut off about the middle; a knife being slipped behind the lumbar plexus of nerves, the pelvic bones and contiguous soft parts are cut away, so that the lumbar vertebræ remain connected with the lower extremities merely by the nervous cords passing to each limb. Thus prepared, the limbs are laid on a thin piece of board, so that the vertebræ hang over its edge dangling by the undivided nerves. The piece of board is placed floating on the surface of the water in which are the actiniæ, and is slowly pushed over within reach of an active one. Immediately that the actinia seizes the morsel thus offered to it, contractions are observed to commence in the thigh, extend to the calf, and soon the toes are in movement.

3rd. In order to set aside the supposition that these muscular contractions might be the result of chemical or mechanical irritation applied to the extremities of the nerves, it became necessary to devise

a modification of the foregoing experiments; for although irritants, such as turpentine, croton oil, ammonia, friction with a nettle leaf, &c., were applied to the nerves without producing any effect like that obtained from the actiniæ, it seemed still possible that the contractions might be due to some other agent than electricity.

The following experiment seems to remove all doubt. A piece of copper wire, a few inches long, was coated with sealing-wax, except about half an inch at each end; the ends were rubbed clean with sand-paper, one of them was thrust into the lower part of the spinal canal of a frog prepared as in the last experiment, while the other, which was to be offered to an actinia, was passed into a portion of the frog's intestine put on like a glove; for the actinia does not seize vigorously metallic substances. The limbs of the frog with the nerves and vertebræ attached, are laid on a piece of board, while the copper wire, which is curved, arches over the edge of it; so that the end covered with frog's intestine can be readily brought within the reach of the actinia. Having waited for a few minutes until the muscular contractions excited by thrusting the wire into the spinal canal have ceased (and they are in general very transient), the board is placed floating on the water, and the frog's intestine offered to an actinia; muscular contractions ensue, perhaps not so promptly, certainly not so vigorously as in the former experiments, but nevertheless easily to be recognized and unmistakeable. They commence in the thighs, and, as in the former case, extend to the calves, and then the toes move actively. This last experiment has been modified in a variety of ways, but the same result has been constantly obtained. Perhaps the best modification of it is to use a piece of copper wire having one end coiled so as to form a disk which is covered with chamois-leather, while the other is sharp-pointed to enter the spinal canal of the frog. The whole, except the surface of the disk, which is to be given to the actinia, and the point for the spinal canal, is covered with sealing-wax, and the frog's limbs extended upon a thin piece of board. With this arrangement precisely the same effects were produced as already described.

It is a remarkable fact, and deserves special notice, that in all these experiments the muscular contractions, when once strongly excited, whether by direct contact or through the medium of wire, do *not* at once subside. When the limbs are withdrawn from the

influence of the actinia in the first experiments, or removed from the wire in the last, strong muscular contractions continue to take place for from three to five minutes.

All the varieties of actinia which have hitherto been made the subject of experiment, have given similar evidence of electrical power, but by no means in an equal degree. The large varieties are found, in proportion to their size, much feebler than those of less dimensions, and any attempt to succeed in the experiment with the copper wire has failed with them.

A somewhat similar observation has been made by Dr. John Davy regarding the torpedo, for he tells us (Philosophical Transactions, 1834, p. 548) that he has seen strong vivacious fish which made great muscular exertions in the water, almost or entirely destitute of electrical action.

It is obvious that in creatures of such moderate dimensions as actiniæ, of so peculiar a form and of such feeble power, much difficulty is to be expected in demonstrating the other experimental effects which animal electricity is capable of producing in common with other electricities, viz. magnetic deflection,—magnetising of needles,—spark,—heating power, and chemical action; and it must be admitted that all experiments hitherto undertaken on this subject have been attended with negative results. I hope, and indeed expect, when further opportunities are afforded of examining these creatures in health and vigour in their native pools, to obtain more satisfactory results on these points, when I shall look forward to the pleasure of making a further communication on the subject.

January 21, 1858.

Dr. J. D. HOOKER, V.P., in the Chair.

The following communication was read :—

"On the Physical Structure of the Old Red Sandstone of the County of Waterford, considered with relation to Cleavage, Joint Surfaces, and Faults." By the Rev. SAMUEL HAUGHTON, Fellow of Trinity College, Dublin, and Professor of Geology. Communicated by Dr. TYNDALL, F.R.S. Received January 19, 1858.

After describing the general features of the district and giving his reasons for selecting it, the author proceeds to give a detailed account of the faults, joint surfaces, and cleavage planes, 345 in number, observed by him during the course of his survey.

The faults are nineteen in number and reducible to two pairs of rectangular systems. The bearings of these systems are E. 7° 30′ N. and E. 34° 22′ N. The other faults, which form nearly right angles with the preceding and may be called Conjugate Faults, have the following bearings, N. 3° 45′ W. and N. 33° 24′ W.

The author considers that the existence of two systems of conjugate faults indicates two distinct systems of upheaving force in the district; a supposition which is strongly confirmed by the fact that the average strike of the beds is E. 10° 46′ N., a direction intermediate between those of the systems of faults. He then demonstrates from 345 observed planes, that the systems of joint and cleavage planes are also conjugate systems, reducible to four, of which two are identical with the two conjugate systems of faults already established. The average observed angles between the conjugate axes of these four systems of planes are 89° 11′, 91° 52′, 91° 20′, and 90° 30′ respectively; and the bearings of their cleavage planes are—

Cleavage.	*Faults.*
7° 46′ North of East.	7° 30′ North of East.
33° 31′ North of East.	34° 22′ North of East.
30° 30′ South of East.	
10° South of East.	

The cleavage planes are distinguished from the joint planes by a peculiar flaggy or platy structure developed in the rock-mass, parallel to their direction. This structure the author thinks to be the result of pressure; and that it indicates that the cleavage planes are perpendicular to the lines of maximum force; he considers the cleavage planes to have been developed while the rock was yet soft. The joint planes, on the contrary, which are conjugate to the cleavage planes, are considered as perpendicular to the lines of minimum force of compression; they were formed by the shrinking of the rock mass, were subsequent to the cleavage planes, and formed when the rock was hard.

Having established the geometrical relations of the structural planes of the conglomerate, the author then deduces from them the mechanical forces which have been at work in bringing the district to its present condition and form. He believes that the method he has adopted in reference to the conglomerate of the county of Waterford is applicable to the physical structure of other districts; and that his results, if confirmed by corresponding results in other districts, of which he is confident, will prove to be a substantial addition to the arguments in favour of the mechanical theory of slaty cleavage.

The paper was accompanied by four diagrams illustrative of the cleavage and joint planes of Portally Head, Swiny Head, Shanooan Head, and of the reversed fault at Portnashrughann.

January 29, 1858.

RICHARD OWEN, Esq., V.P., in the Chair.

The following communcations were read :—

I. "Mémoire sur les Limites de la Pression dans les Machines travaillant à la détente du Maximum d'effet; et sur l'influence des Espaces libres dans les Machines à un seul Cylindre." Par M. MAHISTRE, Professeur à la Faculté des Sciences de Lille. Communicated by Professor STOKES, Sec. R.S. Received October 12, 1857.

§ I. *Limites de la Pression.*

1. Le travail transmis en une minute au piston d'une machine à un seul cylindre, est donné par la formule

$$T_m=\frac{V}{l}\left(\frac{n}{q}+P\right)\left[al'+\{a(l'+c)+\beta+\theta\}\log\frac{a(l+c)+\beta+\theta}{a(l'+c)+\beta+\theta}\right]$$
$$-\frac{V}{l}al\left(\frac{n}{q}+\varpi\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

De même, la course d'admission qui fait sortir la vapeur sous la pression ϖ du condenseur, ou de l'atmosphère, a pour valeur

$$l'=\frac{n+q\varpi}{n+qP}l\left(1+\frac{c}{l}+\frac{\beta+\theta}{al}\right)-\frac{\beta+\theta}{a}-c. \quad . \quad . \quad . \quad (2)$$

(Voir notre mémoire sur le Travail de la Vapeur, dans les Comptes Rendus de l'Académie des Sciences de Paris. Séance du 15 juin.)

Nous avons démontré récemment (Comptes Rendus du 21 septembre) que pour une telle admission *la vaporisation mécanique d'une machine était la même que si, dépourvue d'espace libre, la machine travaillait à pleine vapeur, sous la pression qui s'exerce derrière le piston. Il résulte de cet énoncé que la vaporisation, indépendante de la pression d'admission, reste constante, tant que la vitesse et la pression ϖ restent elles-mêmes constantes.* Cela posé, je me propose d'abord de rechercher ce que devient T_m quand on fait varier P, la vitesse de rotation $\frac{V}{l}$, et la pression ϖ restant les mêmes.

Si l'on résout l'équation (2) par rapport à $\frac{n}{q}+\mathrm{P}$, on trouve d'abord

$$\frac{n}{q}+\mathrm{P}=\left(\frac{n}{q}+\varpi\right)\frac{a(l+c)+\beta+\theta}{a(l'+c)+\beta+\theta}\,;$$

à l'aide de cette valeur, celle de T_m devient

$$\mathrm{T}_m=\frac{\mathrm{V}}{l}\left(\frac{n}{q}+\varpi\right)\left[a(l+c)+\beta+\theta\right]\left(\frac{al'}{a(l'+c)+\beta+\theta}+\log\frac{a(l+c)+\beta+\theta}{a(l'+c)+\beta+\theta}\right)$$
$$-\frac{\mathrm{V}}{l}al\left(\frac{n}{q}+\varpi\right);\quad\ldots\ldots\ldots\ldots\quad(3)$$

or il est évident que cette valeur de T_m sera un maximum, lorsque la quantité

$$y=\frac{al'}{a(l'+c)+\beta+\theta}+\log\frac{a(l+c)+\beta+\theta}{a(l'+c)+\beta+\theta},$$

sera elle-même un maximum, ce qui arrive pour $l'=0$. La limite de $\frac{n}{q}+\mathrm{P}$ devient ainsi

$$\frac{n}{q}+\mathrm{P}=\left(\frac{n}{q}+\varpi\right)\frac{a(l+c)+\beta+\theta}{ac+\beta+\theta}.\quad\ldots\ldots\quad(4)$$

Si dans cette équation on néglige $\beta+\theta$, en supposant que cette somme soit une petite quantité par rapport à ac, on aura, à très peu pres,

$$\mathrm{P}=\varpi+\left(\frac{n}{q}+\varpi\right)\frac{l}{c}.\quad\ldots\ldots\quad(5)$$

Ordinairement les constructeurs donnent à $\frac{l}{c}$ des valeurs comprises entre 15 et 20. D'un autre côté, la pression dans le condenseur est, le plus souvent, de $\frac{4}{19}$ d'atmosphère ; on peut donc supposer $\varpi=2176$ kil. Prenant en même temps $\frac{l}{c}=20$, et observant que $\frac{n}{q}=799$, on trouve

$$\mathrm{P}=61676 \text{ kilog. ou 6 atmosphères, environ.}$$

Par conséquent, *les machines à un seul cylindre, à condensation, timbrées à* 6 *atmosphères au plus, et marchant à la détente du maximum d'effet*, pourront généralement développer tout le travail que leur vaporisation constante est capable de produire. En aucun*

* Il ne s'agit pas ici de la course d'admission du maximum d'effet *analytique*, mais uniquement de celle qui fait sortir la vapeur sous la pression qui s'exerce derrière le piston, et qui diffère très peu de la première.

cas, les machines sans condensation ne pourront utiliser tout le travail relatif à leur vaporisation; puisqu'il faudrait pour cela pouvoir porter la pression de beaucoup au de la du timbre de la chaudière. C'est ainsi que pour des valeurs très petites de $\frac{\beta+\theta}{ac}$, la pression-limite peut dépasser 22 atmosphères.

À l'égard des machines du système de Wolf, on tire d'abord de la formule (12) du mémoire cité,

$$\frac{n}{q}+\mathrm{P}=\left(\frac{n}{q}+\varpi\right)\frac{a_{\prime}l_{\prime}+ac+\theta}{al+ac+\theta}\,\frac{a(l+c)+\beta+\theta}{a(l'+c)+\beta+\theta}; \quad . \quad . \quad (6)$$

Substituant cette valeur dans la formule (10) du dit mémoire, puis exprimant la condition que T_m soit un maximum, on trouve

$$l'=\frac{\beta+\theta}{a}\log\frac{ac+a_{\prime}(l_{\prime}+c_{\prime})+\mu}{a_{\prime}c_{\prime}+a(l+c)+\mu}. \quad . \quad . \quad . \quad . \quad (7)$$

Comme cette valeur de l' est très petite, si on fait dans l'équation (6) $l'=0$, on aura, à très peu près,

$$\frac{n}{q}+\mathrm{P}=\left(\frac{n}{q}+\varpi\right)\frac{a_{\prime}l_{\prime}+ac+\theta}{al+ac+\theta}\,\frac{al+ac+\beta+\theta}{ac+\beta+\theta};$$

et plus simplement, mais avec une approximation moindre,

$$\mathrm{P}=\left(\frac{n}{q}+\varpi\right)\frac{a_{\prime}l_{\prime}}{al}\left(\frac{l}{c}+1\right)-\frac{n}{q}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Ordinairement $\frac{a_{\prime}l_{\prime}}{al}$ est compris entre 4 et 5; prenant $\frac{a_{\prime}l_{\prime}}{al}=4$, et, comme précédemment,

$$\frac{l}{c}=20,\ \frac{n}{q}=799,\ \varpi=2176 \text{ kil.};$$

on trouve

$$\mathrm{P}=249{,}101 \text{ kilog., ou } 24 \text{ atm. environ.}$$

Si la machine ne condensait pas, la limite de P serait évidemment plus grande. De la il résulte que *une machine de Wolf, marchant à la détente du maximum d'effet, ne pourra jamais utiliser tout le travail que sa vaporisation constante est capable de produire.*

Mais *dans deux machines de même système, l'une à condensation, l'autre sans condensation, et travaillant à la détente du maximum d'effet, une même quantité d'eau vaporisée produira le même travail*

*aux limites de la pression, si les volumes engendrés par les pistons sont respectivement égaux, ainsi que les espaces libres homologues**.

Considérons pour fixer les idées deux machines à un seul cylindre. Si l'on pose, pour abréger, $\frac{V}{l}=N$, l'équation (3) sera de la forme

$$T_m=N\left(\frac{n}{q}+\varpi\right)M.$$

Relativement à la machine sans condensation, on aura pareillement

$$T'_m=N'\left(\frac{n}{q}+\varpi'\right)M'.$$

Divisant ces deux égalités membre à membre, et observant qu'aux limites de la pression $M=M'$, il vient

$$\frac{T_m}{T'_m}=\frac{N}{N'}\,\frac{n+q\varpi}{n+q\varpi'}.$$

Soit S la vaporisation commune ; d'après le théorème cité au commencement de ce mémoire

$$S=alN\,(n+q\varpi),$$
$$S=ALN'\,(n+q\varpi')\,;$$

de la on tire

$$\frac{N}{N'}\,\frac{n+q\varpi}{n+q\varpi'}=1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

puisque par hypothèse les volumes al, AL engendrés par les pistons, sont égaux. Par suite

$$T_m=T'_m;$$

ce qu'il fallait démontrer. La démonstration serait la même pour deux machines du système de Wolf.

On voit par ce qui précède, que *la machine sans condensation n'est désavantageuse, que parceque la pression ne peut y être portée jusqu'a ses dernières limites.*

Si l'on veut que dans les deux machines, et pour des pressions moindres que les pressions limites, la même quantité d'eau vaporisée produise le même travail, il suffira d'exprimer que les *volumes d'admissions* al', AL' sont égaux, ce qui exige qu'on ait

$$\frac{n+q\varpi}{n+qP}=\frac{n+q\varpi'}{n+qP'}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

* Relativement à cette dernière partie de l'énoncé, il suffit que la somme des espaces libres soit la même dans les deux machines, quand celles-ci sont à un seul cylindre.

les lettres accentuées se rapportant, comme précédemment, à la machine sans condensation. De la on tire

$$P = \frac{n+q\varpi}{n+q\varpi'}\left(\frac{n}{q}+P'\right)-\frac{n}{q}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

En même temps l'équation (9) donne, pour le rapport des vitesses des rotations

$$\frac{N}{N'} = \frac{n+q\varpi'}{n+q\varpi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Si l'on prend

$$\varpi = 2176 \text{ kilog.}, \quad \varpi' = 10335 \text{ kilog.}, \quad \frac{n}{q} = 799,$$

ces relations deviennent, en négligéant le 2me terme de la valeur de P,

$$P = (0{\cdot}2672)\, P', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$\frac{N}{N'} = 3{\cdot}74. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Ce qui fait voir que *les deux machines ne pourront produire le même travail qu'entre des limites très étroites.*

C'est ainsi, par exemple, que depuis 3·7 atm. jusqu'à 10 atm., la machine sans condensation pourra marcher à la même force, pour la même vaporisation, que la machine à condensation travaillant depuis 1 atm. jusqu'à 2·6 atmosphères.

2. Nous terminerons la 1re partie de ce mémoire par le théorème suivant :

Dans deux machines de même système, toutes deux à condensation, ou toutes deux sans condensation, et travaillant à la détente du maximum d'effet, une même quantité d'eau vaporisée produira le même travail, si dans les deux machines la pression d'admission est la même, et si les capacités homologues du système distributeur sont, respectivement, dans le même rapport avec les volumes engendrés par deux pistons de même nom.

Considérons, pour fixer les idées, deux machines à un seul cylindre ; je suppose que le rapport

$$\frac{ac+\beta+\theta}{al}$$

soit le même dans les deux machines ; je suppose aussi que la vaporisation constante soit égale de part et d'autre, et je dis qu'il en sera de même du travail. En effet, de l'équation

$$S = alN\,(n+q\varpi) = aV\,(n+q\varpi),$$

on tire

$$aV = \text{constante}.$$

La formule (2) donne pareillement

$$\frac{al'}{al} = \text{constante},$$

pourvu que P soit le même de part et d'autre. Donc aussi

$$T_m = \text{constante},$$

car la valeur de T_m peut s'écrire sans la forme

$$T_m = aV\left(\frac{n}{q}+\varpi\right)\left(1+\frac{ac+\beta+\theta}{al}\right)\left(\frac{\frac{al'}{al}}{\frac{al'}{al}+\frac{ac+\beta+\theta}{al}}+\log\frac{1+\frac{ac+\beta+\theta}{al}}{\frac{al'}{al}+\frac{ac+\beta+\theta}{al}}\right)$$
$$-aV\left(\frac{n}{q}+\varpi\right).$$

La démonstration serait la même pour deux machines de Wolf.

On peut remarquer que le théorème précédent aura lieu qu'elle que soit la détente, pourvu que les volumes d'admissions restent égaux. Seulement, la vaporisation commune variera avec la pression, et dans le même sens.

Il résulte de ce qui précède que *dans deux machines de même système, l'une à condensation, l'autre sans condensation, travaillant à la détente du maximum d'effet, et dont les capacités homologues du système distributeur sont dans les rapports indiqués ci-dessus, une même quantité d'eau vaporisée produira le même travail aux limites de la pression. Ce travail pourra aussi être rendu égal pour de certaines pressions moindres que les pressions limites.*

§ II. *De l'influence des espaces libres dans les machines à* un seul *cylindre.*

3. Considérons une machine destinée à marcher avec une course d'admission l', une vitesse de rotation $\frac{V}{l}$, et une pression d'admission P. *Je me propose de rechercher qu'elle est l'influence des espaces libres sur le travail de la machine.* Si l'on pose pour abréger

$$ac+\beta+\theta=x,$$

la valeur (1) de T_m devient

$$T_m = \frac{V}{l}\left(\frac{n}{q}+P\right)\left[al'+(al'+x)\log\frac{al+x}{al'+x}\right]-\frac{V}{l}al\left(\frac{n}{q}+\varpi\right). \quad (9)$$

Nous ferons remarquer tout d'abord que T_m est indépendant des espaces libres pour $l'=l$, car dans ce cas l'on a simplement

$$T_m=\frac{V}{l}al(P-\varpi). \quad \ldots \ldots \quad (10)$$

Maintenant si l'on veut rendre T_m maximum par rapport à x, il suffira évidemment de rendre maximum le terme

$$y=(al'+x)\log\frac{al+x}{al'+x};$$

et pour cela, il faudra déterminer x par la relation

$$\frac{a(l-l')}{al+x}=\log\frac{al+x}{al'+x}. \quad \ldots \ldots \quad (11)$$

Si dans cette équation on néglige les termes de l'ordre de x^2, on trouve

$$x=a\,\frac{l\log\frac{l}{l'}-(l-l')}{\frac{l-l'}{l'}-\log\frac{l}{l'}}. \quad \ldots \ldots \quad (12)$$

4. Supposons maintenant qu'on fasse travailler la machine à la détente du maximum d'effet. Dans ce cas l' sera une fonction de x déterminée par la relation

$$\frac{al'+x}{al+x}=\frac{n+q\varpi}{n+qP}; \quad \ldots \ldots \quad (13)$$

et l'équation du travail deviendra

$$T_m=\frac{V}{l}\left(\frac{n}{q}+P\right)\left[(al+x)\frac{n+q\varpi}{n+qP}\left(1+\log\frac{n+qP}{n+q\varpi}\right)-x\right]-\frac{V}{l}al\left(\frac{n}{q}+\varpi\right).$$

Or on s'assurera sans peine que cette fonction prend sa valeur maxima pour $x=0$. Dans ce cas, les limites de l' et de T_m deviennent

$$l'=\frac{n+q\varpi}{n+qP}l, \quad \ldots \ldots \quad (14)$$

$$T_m=\frac{V}{l}al\left(\frac{n}{q}+\varpi\right)\log\frac{n+qP}{n+q\varpi}. \quad \ldots \ldots \quad (15)$$

Il doit être entendu que les logarithmes qui entrent dans les diverses formules sont des logarithmes Népériens.

On voit par ce qui précède, que *les espaces libres doivent être déterminés pour la détente à laquelle la machine doit marcher habituellement. Dans le cas de la détente du maximum d'effet, ils doivent être rendus aussi petits que les nécessités de la construction le permettent.* Une fois la somme des espaces libres déterminée, on réglera le développement des conduits de manière à donner à ceux-ci

la plus grande section possible, à fin de ne pas créer d'obstacle inutile au mouvement de la vapeur.

Les espaces libres n'entrant pas d'une manière symmétrique dans la formule du travail d'une machine de Wolf, la théorie qui précède n'est pas applicable à cette machine. Toutes fois on pourra déterminer

$$x = ac + \beta + \theta,$$

de manière à rendre maxima la somme des deux premiers termes de la valeur de T_m.

5. Pour donner une application numérique de ces formules, nous prendrons pour exemple la machine horizontale, et sans condensation, de la Gare de Fives.

Dimensions des principaux organes de la machine.

Course du piston . . . . $l = 0{\cdot}45$ m.
Rayon du cylindre . . . . $r = 0{\cdot}115$ m. d'où
$a = 0{\cdot}04555$ m. q.
Liberté du cylindre . . . . $c = 0{\cdot}015$ m.
Volume de conduit qui fait communiquer la boite à vapeur au cylindre . . . . $\theta = 0{\cdot}0012$ m. c.
Nombre de tours de la manivelle par minute. . $\frac{V}{l} = 300$.

Comme dans cette machine le tiroir fait lui même détente, le volume β de la boite à vapeur ne doit pas entrer dans les formules; alors on a simplement

$$x = ac + \theta = 0{\cdot}0018225 \text{ m. c.}$$

Cela posé, si l'on prend

$$l' = 0{\cdot}08 \text{ m.},$$

la relation (12) donne

$$x = 0{\cdot}0060634 \text{ m. c.}$$

Maintenant si l'on calcule la force de la machine en prenant

$$P = 6 \text{ atm.} = 62010 \text{ kilog.}$$

et faisant usage, successivement, des valeurs ci-dessus de x, on trouve

avec les espaces libres effectifs	$T_m = 29{\cdot}85$ ch.	Diff. $= 8{\cdot}25$ ch. $= 28$ per cent.
avec les espaces libres calculés	$T_m = 38{\cdot}10$ ch.	

Dans le cas de la détente du maximum d'effet, et pour la même pression de 6 atm., les résultats sont les suivants,—

avec les espaces libres effectifs	$T_m=22{\cdot}64$ ch.	Diff.$=3{\cdot}71$ ch.$=16$ per cent.
avec les espaces libres nuls	$T_m=26{\cdot}35$ ch.	

II. "On the Action of Nitrous Acid on Aniline." By A. Matthiessen, Ph.D. Communicated by Professor Stokes, Sec. R.S. Received January 12, 1858.

On repeating the experiments of Hunt* and Hofmann†, on the action of nitrous acid on aniline, I found that the reaction does not take place exactly as these chemists state; Hunt gives the reaction as

$$\left.\begin{matrix} C_{12}\,H_5 \\ H \\ H \end{matrix}\right\} N+NO_3+HO=C_{12}\,H_6\,O_2+N_2+2HO.$$

Hofmann says that phenylic alcohol is not formed, but nitrophenassic acid, when binoxide of nitrogen is led into a diluted solution of the nitrate:

$$\left.\begin{matrix} C_{12}\,H_5 \\ H \\ H \end{matrix}\right\} N+NO_3+NO_5=C_{12}\left\{\begin{matrix} H_5 \\ NO_4 \end{matrix}\right\} O_2+N_2+2HO.$$

This reaction, although correct in the end result, omits the intermediate stage, which is—

$$\left.\begin{matrix} C_{12}\,H_5 \\ H \\ H \end{matrix}\right\} N+H_2\,O_2+NO_3=C_{12}\,H_6\,O_2+NH_3+NO_3,$$

and then $NH_3+NO_3=N_2+3HO$.

On account of the free nitric acid, the phenylic alcohol is always converted into nitrophenassic acid. The ammonia was determined as platinum salt, and two experiments gave 43·9 and 44·1 per cent. of platinum; the theoretical quantity required is 44·2 per cent.

It appears, therefore, when nitrous acid acts on aniline, that in the first part of the reaction it causes only a substitution, and afterwards, the ammonia being attacked by it, gives off nitrogen and water.

* Sill. Am. Journ. (2) viii. 372. † Chem. Soc. Quart. Journ. iii. 231.

Ammonia was obtained either by treating aniline with nitrous acid, or by the action of nitrate of potash on the chloride, or by leading the binoxide of nitrogen into a solution of the nitrate; the latter was the way generally employed. After about twelve hours' action of NO_2 on a solution of the nitrate in a water-bath, the solution was filtered from the nitrophenassic acid, and distilled with potash, the distillate treated with ether to dissolve out the aniline, redistilled in hydrochloric acid, evaporated, and the ammonia determined as platinum salt. These results have led me to try the action of nitrous acid on other organic bases, and I have already obtained from ethylaniline a base which to all appearance is ethylamin. The chloride gives off, when heated with potash, an alkaline inflammable gas, and the platinum salt resembles that of ethylamin; but the platinum determination made with it does not agree very well with that salt. I am now repeating the reaction on a larger scale, so that I shall shortly be able to see whether it is really ethylamin or not.

The foregoing experiments were carried out in the Royal College of Chemistry under the direction of Professor Hofmann.

III. "On the Existence of Amorphous Starch in a new Tuberaceous Fungus." By Frederick Currey, Esq., M.A. Communicated by Joseph Dalton Hooker, M.D., F.R.S. Received December 17, 1857.

Amorphous starch (including under that term all starch not in the form of the ordinary starch-granule) is rare in the vegetable world. Until the present year Schleiden was the only botanist by whom it had been noticed, and his observations have been doubted by Sanio, Caspary, and Schenk. He (Schleiden) states (Grundzüge, i. 181) that he has seen amorphous starch in the form of a thin pasty layer in the cells of the albumen of *Cardamomum minus*, in *Sarsaparilla*, and in the rhizome of *Carex arenaria*. Sanio* has just published the result of some experiments made by him upon the cells of the epidermis of *Gagea lutea*. Upon applying a solution of iodine to these cells, he observed a fine flocculent blue precipitate in their interior. The blue colour was confined to the fluid contents of the

* Bot. Zeitung, 19th June, 1857.

cells, the primordial utricle and the nucleus becoming yellow under the iodine.

Another observer, Dr. Schenk*, has lately noticed the occurrence of starch in a state of solution in the epidermal cells of the stem, leaves, and other parts of *Ornithogalum nutans* and *Ornithogalum lanceolatum*. These cells were found to contain (besides nuclei) a thick homogeneous fluid. Tincture of iodine coloured the fluid first wine-red, then violet, and finally indigo-blue; and the fluid at the same time lost its homogeneous nature, and became finely granular and flocculent.

The above mentioned are all cases of phænogamic plants. The Fungi have hitherto been considered wholly devoid of starch, unless, perhaps, the case mentioned by Schacht† may be an exception. He states that he observed the mycelium of a small mould-fungus become clear blue under the action of iodine. He could not, however, ascertain whether the colour was in the membrane or in the contents, and if the former, it is as likely that the colour (being clear blue) arose from the presence of cellulose in a young condition, as from starch‡.

Mohl, in his treatise on the vegetable cell, speaks of starch as probably existing in all plants *except the Fungi*. A special interest, therefore, attaches to any plant of the latter tribe, in which starch can be shown to exist, and such a plant has lately come under my observation. The fungus in question, which is interesting not only

* Bot. Zeitung, July 17th, 1857.

† Die Pflanzenzelle, p. 39.

‡ In the 'Annales des Sciences Naturelles,' 4th Series, vol. iii. p. 148, Nylander mentions a blue colour being produced by iodine in the summits of the asci of certain *Sphæriæ*, which he attributes to the presence of lichenin. Gerhardt, however, in his 'Lehrbuch der Organischen Chemie,' states that a pure solution of lichenin is coloured yellowish by iodine.

It is not easy to understand why the writers who speak of amorphous starch, take no notice of the lichens. Irrespective of the fact that the membrane of the asci of lichens is coloured blue by iodine, it is well known that the asci and paraphyses are often surrounded by a viscid substance which is coloured by iodine in the same manner as starch, and which cannot well be anything else than starch in an amorphous state. Schacht, indeed, calls the viscid substance "aufgequollene Stärke;" but this expression would be more applicable to the condition of starch when subjected to the action of hot water or sulphuric acid, and seems hardly consistent with his previous definition of the substance as a shapeless, paste-like mass. (See Die Pflanzenzelle, pp. 148, 149.)

for its chemical composition, but as constituting a new genus in the family of the Tuberacei, occurred in the spring of the present year, growing gregariously upon fragments of wood on the sands by the sea-shore at Sketty, near Swansea. To the naked eye each individual specimen presents the appearance of a small, round, somewhat flattened body, of a dull yellow colour, and with an unevenness of surface caused by numberless convolutions of the coat of the fungus, which require the aid of a lens in order to be clearly seen. The diameter of the largest specimen does not much exceed the 1-8th of an inch. Externally there is a strong resemblance to small specimens of *Dacrymyces deliquescens*, or perhaps a nearer still to the truffle described by Tulasne, in the "Fungi hypogæi," under the name of *Hydnobolites cerebriformis*. This resemblance, however, is only superficial, as will be seen by the following description of the plant when examined microscopically. The coat of the fungus consists of a convoluted membrane of considerable thickness, formed of several layers of cells, the outer of which are large and rounded, the inner long and flat. In most of the specimens the contents of the coat consist mainly of an innumerable multitude of naked spores; but in almost all, a careful examination will detect, here and there, isolated sacs or asci containing sporidia; and a few of the plants which were in a younger state than the rest exhibited asci in abundance, showing satisfactorily that the fungus must be classed with the Ascomycetes,—not with the Gasteromycetes.

There is no doubt that the asci are absorbed at an early period, and the sporidia then form a dense mass.

It is exceedingly difficult, from the crowded state of the contents, to trace out the manner in which the asci originate; but I have satisfied myself that they spring at intervals from threads proceeding from the inner surface of the thick external membrane. Fig. 4 represents one of these threads with the asci springing from it, magnified 315 diameters. The asci themselves are broadly clavate, with a very short stem, and are frequently, if not usually, drawn out at the apex into a sort of point, as shown in figs. 2, 3 and 4.

The sporidia are extremely curious. They are globular and colourless, and furnished with long delicate sharp rays projecting from the surface in every direction. Each sporidium is furnished with an internal nucleus, or probably oil-drop (sometimes broken up into

several), which varies somewhat in size, and is sometimes in the centre of the globe, sometimes placed eccentrically. Their form will be seen by reference to fig. 5.

The average diameter of the sporidia is about $\frac{1}{2000}$th of an inch. Upon placing a thin section of one of the plants in water under the microscope, and adding a drop of solution of iodine, the sporidia in the course of a few seconds assume a more or less dark purple colour, precisely similar to that produced in starch by the same reagent; and not only are the sporidia themselves thus affected, but the fluid surrounding them is tinged of an intense purple colour for some distance round the mass of sporidia. This latter colouring is doubtless produced by the effect of the iodine upon a viscid matter which surrounds the sporidia, and which may either originate in the disintegration of the asci, or may be an independent secretion. There can be little doubt, I think, that this viscid matter is starch in a state of solution. It might be taken for dextrine, but that it differs from that substance in assuming a purple colour under iodine.

The sporidia, although coloured by iodine in the same manner as starch-granules*, do not exhibit any cross when viewed by polarized light. The small size of the Fungi precludes the possibility of procuring a sufficient quantity of the viscid matter to test its effect upon the plane of polarization.

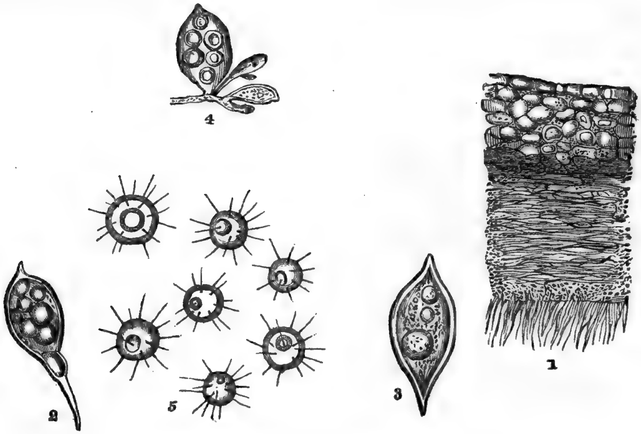
I find the sporidia unaffected by boiling water or even by long soaking in sulphuric acid, in which respect they differ from starch-granules. The purple colour, however (as is the case with starch), disappears under the action of heat or of alcohol.

I have named the plant *Amylocarpus encephaloides*, for reasons sufficiently obvious from the above description. Its systematic position is certainly with the Tuberacei, but it has no near allies. The only plant resembling it in structure is *Endogone*, but it is doubtful whether the vesicles of *Endogone* be spores or asci. If the latter, the affinity with *Endogone* would be close.

In conclusion I may mention, that in a very late number of the 'Annales des Sciences Naturelles' (4 série, vol. vi. p. 318), which has reached me since my first observations on the above fungus, M. Tulasne remarks, that in several species of *Erysiphe* the tips of

* The blue colour does not extend to the nucleus of the sporidia, which is yellow under the iodine.

the radicular appendages are tinged blue by solution of iodine, and that he has observed the same effect produced upon the matter contained in the summits of the asci, and upon the mucous envelope of the sporidia of several species of *Sphæria*. It would seem, therefore, that the absence of starch can no longer be considered as characteristic of the Fungi, and that the existence of that substance in an amorphous state may be considered as satisfactorily proved.



DESCRIPTION OF THE FIGURES.

Fig. 1. Vertical section of the coat of the Fungus, showing the successive layers of cells, the innermost of which give off threads into the interior of the plant, × 315 diameters.

Figs. 2 and 3. Asci with sporidia, × 415. In fig. 3 the sporidia are only partially matured.

Fig. 4. The extremity of a thread showing the mode of origin of the asci, × 315.

Fig. 5. Free sporidia, × 415.

IV. "On the Singular Solutions of Differential Equations." By the Rev. Robert Carmichael, Fellow of Trinity College, Dublin. Communicated by Arthur Cayley, Esq. Received December 28, 1857.

(Abstract.)

The objects contemplated in this paper are the following:—

1. The reduction to a symmetrical form of the well-known theorem by Clairaut for the integration of differential equations in a

single independent variable, and the simultaneous determination of the singular solutions, if such exist; the generalisation of the transformed types, and the application of the result to the integration of a large variety of partial differential equations in any number of independent variables, and the simultaneous determination of their singular solutions, where such exist.

2. The examination of the general theory commonly attributed to Laplace.

3. The indication of certain desiderata.

February 4, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "On the daily Fall of the Barometer at Toronto." By THOMAS HOPKINS, Esq. Communicated by WILLIAM FAIRBAIRN, Esq. Received December 19, 1857.

(Abstract.)

In this paper the writer exhibited tables of the movements of meteorological instruments registered at Toronto in 1846, in the months of January and July, as specimens of the changes which take place in the atmosphere in winter and summer. The principal object was to find the cause of the fall of the barometer in the middle of the day. The author endeavours to show that the vapour, which in the early part of the day was produced by solar heat at the surface, by its expansive power, bore that heat to the upper regions of the air, where it was condensed by the cold of the gases in that situation, when the heat of elasticity was set at liberty to warm and expand the gases, and that it was this expansion which reduced atmospheric pressure in the locality and caused a fall of the barometer.

II. "Researches on the Poison-apparatus in the *Actiniadæ*." By Philip Henry Gosse, Esq., F.R.S. Received January 18, 1858.

(Abstract.)

The organs which have been termed "thread-cells," "thread-capsules," "urticating organs," "lasso-cells," &c., I propose to call *cnidæ*. They are found in various tissues of the body, but are specially localized in two sets of organs, which I call *craspeda* and *acontia*. The *craspeda* are gelatinous cords connected throughout their length with the free edges of the muscular septa. The *acontia* are somewhat similar cords, but free throughout, except at their base, where they are inserted into the septa. The cord-like appearance of these latter organs is, however, illusory, as each is a narrow ribbon with involute margins. Both the *craspeda* and the *acontia* are composed of a clear plasma, in which many *cnidæ* are crowded.

The *craspeda* appear to be universally possessed by this tribe of animals, but the *acontia* are limited to a few genera, principally *Sagartia* and *Adamsia*. They are ejected from the body of the animal, and are again withdrawn.

For the emission of these organs special orifices exist, which I term *cinclides*. These are minute perforations of the muscular coats and the integument, bearing a resemblance in appearance to the spiracles of insects. Being placed in the interseptal spaces, they have a perpendicular arrangement, but are not regularly disposed in any other respect. They can be opened widely, or perfectly closed at the will of the animal; and are well seen, under a low power of the microscope, when a *Sagartia bellis* or *dianthus* is much distended in a parallel-sided glass vessel, with a strong light behind it. The width of these orifices varies from $\frac{1}{300}$th to $\frac{1}{50}$th of an inch. No ciliary current passes through them.

Under irritation the *Sagartia* forcibly and repeatedly contracts its body, forcing out the water which had distended its aquiferous canals and the general cavity of the body. Much of the fluid finds vent at these foramina, carrying with it the free floating part of some or other of the numerous *acontia*, each through that *cinclis* which happens to lie nearest to it. The frequency with which the *acontia*

escape in a *loop* or *bight*, shows that the issue is the result of a merely mechanical action, viz. that of the escaping water.

The *cnidæ* occur under four distinct forms. 1. Chambered cnidæ (*Cnidæ cameratæ*). This is the most widely distributed, and the most elaborately armed. In *Cyathina Smithii* they occur of comparatively large size, and are therefore well suited for observation. They are transparent, colourless vesicles, of a long, oval figure, $\frac{1}{260}$th of an inch in length, and $\frac{1}{2000}$th in diameter. A fusiform chamber passes through the centre of the anterior moiety, merging at one extremity into the walls of the *cnida*, and at the other diminishing to a slender chord, which is irregularly coiled within the general cavity.

Under stimulus the *cnidæ* suddenly expel their contents with great force. In general the eye can scarcely follow the excessive rapidity with which the chamber and its twining thread are shot forth. When fully expelled, the thread, which I distinguish by the term *ecthoræum*, is often thirty times as long as the *cnida*; but in *Sagartia* generally, it frequently is not more than once and a half the length of the *cnida*.

In the *ecthoræum* from *chambered cnidæ* the basal portion is distinctly swollen; thence, becoming attenuated, it runs on as an excessively slender wire of equal diameter. Around this basal part wind one or more spiral thickened bands, varying, in different species, as to their number, the number of volutions made by each, and the angle which the spiral forms with the axis. The direction is from east to north. The spiral armature I call the screw, or *strebla*. There is no other form of armature than this.

These thickened spiral bands afford insertion to a series of fine *setæ*, which I call *pterygia*. These are from eight to twelve in a single volution, and they project in a diagonal direction from the *ecthoræum*, but often become reverted. In some cases, perhaps in all, the *strebla* and the *pterygia* are continued beyond the swollen portion of the *ecthoræum*, even to the end of the attenuated part.

2. Tangled cnidæ (*Cnidæ glomiferæ*). This sort differs from the preceding chiefly in the uniform slenderness of the *ecthoræum*, which lies coiled up more or less regularly in the *cnida*, without any chamber. *Corynactis viridis* affords excellent examples for observation.

3. Spiral cnidæ (*C. cochleatæ*). The walls of the tentacles, in a few species, contain very elongated fusiform *cnidæ*, which seem

composed of a slender thread coiled up in a very close and regular spiral, bearing a resemblance to the shell of a *Cerithium*. The *ecthoræum* is discharged reluctantly, and the wall of the *cnida* is very subtile.

4. Globate cnidæ (*Cnidæ globatæ*)? These are globose vesicles found in the *acontium* of *S. parasitica,* which have some characters in common with the *cnidæ,* but of whose real nature I am doubtful.

In the indubitable *cnidæ* the emission of the *ecthoræum* is a process of eversion. This is proved by many circumstances, such as the order in which the portions are evolved, the basal portion first; as well as by direct observation, the terminal part of the *ecthoræum* being occasionally detected in running out through the centre of the portion already evolved.

The *cnidæ* are filled with a fluid, which holds organic corpuscles in suspension, and these are seen driven rapidly through the *ecthoræum* in the process of eversion. I conclude that in this fluid resides the expansile force, which, on the excitement of a suitable stimulus, distends and projects the tubular portion of the wall that has hitherto been inverted.

All of the four kinds of *cnidæ* enumerated have been at various times seen surrounded by a membranous investiture, which I distinguish as the *peribola.* This coat must be ruptured before the *cnida* can emit the *ecthoræum.*

Several experiments show that the *ecthoræum* has the power of penetrating the tissues of other creatures, and even of the Vertebrata. In some of these experiments shavings of human cuticle, presented for an instant to the tentacles of *B. crassicornis,* and to the *acontium* of *S. parasitica,* were found on examination to be pierced through with numerous *cnidæ.*

Experiments with blue vegetable juices were instituted, with a view to test the acid or alkaline properties of the poisonous fluid supposed to be ejected on the discharge of the *ecthoræum*; but with no definite result. The existence of such a poisonous fluid is inferred, however, with a degree of probability amounting to moral certainty, and that of such concentrated power as, under certain circumstances, to destroy life with great rapidity, even in vertebrate animals.

Admitting the existence of a venomous fluid, it is difficult to

determine where it is lodged, and how it is injected. I incline to the hypothesis, that the cavity of the *ecthoræum* in its primal inverted condition, while it yet remains coiled up in the *cnida*, is occupied with the poisonous fluid, and that it is poured out gradually, within the tissues of the victim, as the evolving tip of the wire penetrates farther and farther into the wound.

The paper is illustrated by figures of the organs described.

February 11, 1858.

Major-General SABINE, Treasurer and V.P., in the Chair.

The following communication was read :—

"An Account of some recent Researches near Cairo, undertaken with the view of throwing light upon the Geological History of the Alluvial Land of Egypt."—Part II. By LEONARD HORNER, Esq., V.P.R.S. Received January 25, 1858.

(Abstract.)

In the first part of this Memoir, read on the 8th of February, 1855, and published in Part I. of the Transactions of that year, the author states the main object of the inquiry to have been, to endeavour, by probing the alluvial land in appropriate places, to discover the probable time that has elapsed since the lowest layer of Nile sediment was deposited, and thus to connect geological and historical time. This object, in the opinion of the author, can only be attained by means of shafts and borings of the soil in the immediate neighbourhood of monuments of a known age. The places he selected for these excavations were the vicinity of the Obelisk of Heliopolis, and the site of ancient Memphis. The general introductory matter, and the analyses of the various soils penetrated, together with a description of the researches at Heliopolis, are given in the first part of the memoir ; but the author deferred his general conclusions, and all inferences as to the secular increase of the alluvial deposits, until he should have an opportunity of laying before the Society an account of the more extensive researches in the district of Memphis. That

account, together with the author's general conclusions, form the subject of this second part.

The practical part of the whole inquiry has been conducted under the immediate direction of Hekekyan Bey, an Armenian engineer officer in the service of the Viceroy of Egypt; and a brief biographical account of him is given, showing his eminent scientific qualifications for such researches. The author had the advantage of obtaining the zealous cooperation of our Consul-General in Egypt, the Honourable Charles Augustus Murray, and his successor, the Honourable Frederick Bruce, on whose representations the late Viceroy Abbas Pacha, and the present, not only gave a ready assent to the undertaking, but, with a rare and most exemplary liberality, ordered that the whole expense should be defrayed by the Egyptian Government.

As at Heliopolis the Obelisk is all that remains above ground of that city, so, at Memphis, there is one solitary monument of its former greatness, a fallen colossal statue of the great king Ramesses II., the Sesostris of the Greeks. All testimony appears to concur in assigning the foundation of Memphis to Menes, the first king of the first dynasty, who, according to Lepsius, began his reign 3892 years B.C. The same authority assigns the dates of 1394 to 1328 B.C. for the reign of Ramesses II. The site of Memphis presented therefore a peculiarly fit situation for prosecuting the inquiry, by sinking pits to the greatest practicable depth near this colossal statue, and around it.

The surface of the ground, for some distance around the statue, being uneven, it became necessary, in order to ascertain the variable depth of water during an inundation, at the mouths of the pits, intended to be sunk in various parts of the area, that the level of the highest rise of the water over the ground at a given time should be determined. This was done for the inundation of 1851, and it proved to be somewhat above the 24th cubit mark of the Rhoda Nilometer, a height of water which covers the entire surface of the valley, leaving above it artificial elevations. The inequalities of the ground are such, that in any section, under the 24th cubit level, the surface varies from where it coincides with that level to nearly 20 feet in the deepest part; so that, while in one part of the district there might be a depth of nearly 20 feet of turbid water, in another it might be

less than an inch; and consequently, the same period of time would be represented by very different degrees of thickness of the sediment.

Two pits were sunk close to the fallen colossal statue, sections of both of which are given. In the deepest, the shaft was continued to the depth of 24 feet 5 inches, when further progress was stopped by filtration water. This interruption to excavations occurred in every other pit that was sunk. From the bottom of the shaft, a boring tool was applied, and cores of soil were brought up from successive depths, the lowest being 41 feet $4\frac{1}{2}$ inches from the surface of the ground. The sections given of the two pits in this locality show, that the soil consists of varieties of loam and sand in irregularly alternating layers; and the Nile sediment from the lowest part of the boring was found, by a careful analysis, to be nearly identical in composition with that deposited by the inundation of the present day. At a depth of 5 feet 8 inches from the surface of the ground they came upon the upper surface of the platform on which the colossus had stood, consisting of two courses of cyclopean masonry, together 5 feet 6 inches thick, resting on an artificial bed of sand, the sand resting on Nile sediment. Throughout the excavation various objects of art and some bones of domestic animals were met with, and the boring instrument brought up from the lowest depth a fragment of pottery.

The author next proceeds to describe, with references to detailed sections, seventeen pits and borings sunk in the area of Memphis, and also a series of seven pits opened in ground below the inundation level of 1851, in a line across the valley from the foot of the Libyan Hills on the west of the Nile, to the skirt of the Arabian Hills on the east of the river, embraced within an area of about five miles from west to east, and a mile from north to south.

In 1854 another series of pits and borings were sunk in the parallel of Heliopolis, above eight miles above the apex of the Delta, in ground below the inundation level of 1853, which was very nearly the same as that of 1851, the line including fifty-one pits in a distance of about sixteen miles, eight miles on the right, and eight miles on the left bank of the river; two of them near the river were carried to a depth of 50 feet, and one to a depth of 60 feet from the surface of the ground. This last reached to within $7\frac{1}{2}$ inches of the mean level of the Mediterranean.

The author then reviews the chief facts made known by the ninety-five probings of the alluvial land above described, and gives the following results:—

1. That the alluvium consists of two principal kinds, viz. an argillaceous earth or loam more or less mixed with fine sand, and of quartzose sand, which is probably brought from the adjacent deserts by violent winds;

2. That the Nile sediment found at the lowest depth reached is very similar in composition to that of the present day;

3. That in no instance did the boring instrument strike upon the solid rock, which may be presumed to form the basin between the Libyan and Arabian Hills, containing the alluvium accumulated through unknown ages;

4. That, except minute organisms discoverable only by a powerful microscope, few organic remains were found, and those met with were recent land shells and bones of domestic animals;

5. That there has not been found a trace of an extinct organic body;

6. That at the same level great varieties in the alluvium have been found in adjoining pits, even when the distances between them were very moderate;

7. That there is an absence of all lamination in the sediment. The author points out the causes that account for this,—chiefly the rapid drying of the soil, so soon as the inundation water has subsided, the operations of agriculture, and the violent winds that sweep over the valley forming vast clouds of dust;

8. That in many places the disintegrations of sun-burnt bricks have contributed largely to the soil;

9. That in nearly every part of the ground penetrated, artificial substances have been found, such as fragments and particles of burnt brick and pottery, and at the lowest depth reached.

The author then enters, at some length, into the circumstances which modify the deposition of the sediment in different parts of the valley, showing how the coarser and heavier matter held in suspension in the inundation water must be deposited in greatest amount in the higher parts of the river's course, in its bed, and near its banks; that this must be further caused by the slight fall, which between Assouan and Cairo is less than $6\frac{1}{2}$ inches in a mile, the Nile

in its whole course from the first cataract to the sea not being used as water power; that the vast heat must cause an evaporation that lets fall the solid matter more abundantly in the southern latitudes; that the river from 42 miles below the first cataract is nowhere allowed to overflow the land, but is confined by embankments, so that the waters of irrigation are spread by canals, by which and by the irregularities of the ground eddies are formed. From all these causes affecting the distribution of the sediment over the land, the depth of the annual deposit by the inundation is very different in different parts of the valley, and consequently the same lapse of time may be represented by very different depths of the soil.

The author next treats of the rate of secular increase of the alluvial land. Before entering upon the results at which he arrives by these recent researches, he refers to the operations of the French engineers at the end of the last century, who state the mean of the rise of the land between Assouan and Cairo to be 5 inches in a century. From that conclusion, and especially from the application of it, the author dissents, and states his reasons at considerable length in the Appendix to his Memoir. He considers that in every situation where a calculation is to be made of the rate of secular increase, we must have a fixed point in time to start from; that is, the known age of a monument, the foundation of which rests upon Nile sediment, and upon the sides of which the latter has accumulated by subsequent inundations. If there have been no local causes to disturb the probability that the sediment above and below the foundation has accumulated at the same rate, we divide the amount above the foundation by the number of centuries known to have elapsed from the erection of the monument to the present time, and then apply the same chronometric scale to the greatest ascertained depth of sediment below the foundation. Estimated by this rule, the researches at Heliopolis gave the result of a rate of increase of 3·18 inches in a century. But a degree of uncertainty arises at this place, because of the city appearing to have been built upon a portion of land somewhat raised above the level of the rest of the skirt of the desert, and advancing into the low ground then inundated by the Nile; whereby it became doubtful whether a bed of sand penetrated was sedimentary or a part of the desert land.

In the excavations near the colossus of Ramesses II. at Memphis,

there were 9 feet 4 inches of Nile sediment between 8 inches below the present surface of the ground and the lowest part of the platform on which the statue had stood, after making a due allowance for the foundation of the platform having been below the then surface. It is assumed that the platform was laid in the middle of the reign of that king, that is, in the year 1361 B.C., which, added to A.D. 1854, when the observation was made, give 3215 years during which the above depth of sediment was accumulated; and supposing that no disturbing cause had interfered with the normal rate of deposition in this locality, and of which there is no evidence, we have thus a mean rate of increase within a small fraction of $3\frac{1}{2}$ inches in a century. Below the platform, there were 32 feet of the total depth penetrated, but the lowest two feet consisted of sand, below which it is possible there may be no true Nile sediment in this locality, thus leaving 30 feet of the latter. If that amount has been deposited at the same rate of $3\frac{1}{2}$ inches in a century, it gives for the lowest part deposited an age of 10,285 years before the middle of the reign of Ramesses II., 11,646 years B.C., and 13,500 years before A.D. 1854.

The author then observes, that these recent researches, taken in conjunction with those of a similar kind by the French engineers at the close of the last century, high in Upper Egypt, afford strong presumptive evidence that the whole of the land of Egypt between the bounding hills, from the first cataract to the sea, extending nearly 700 miles—that land which is associated in our minds with all that is most ancient in history or tradition—belongs entirely to the recent geological period. No trace of an extinct organism has been turned up to take the formation of the alluvial land of Egypt beyond that modern epoch from which we are used to carry back our geological reckonings.

The author concludes with some remarks on the evidence which these researches seem to afford of a very early existence of man in Egypt. In a large majority of the excavations and borings the sediment was found to contain at various depths, and frequently at the lowest, small fragments of burnt brick and of pottery. In the lowest part of the boring of the sediment at the colossal statue in Memphis, at a depth of 39 feet from the surface of the ground, consisting throughout of true Nile sediment, the instrument brought

up a fragment of pottery. [This fragment was exhibited when the paper was read.] Having been found at a depth of 39 feet, it would seem to be a true record of the existence of man 13,371 years before A.D. 1854, reckoning by the before-mentioned rate of increase of $3\frac{1}{2}$ inches in a century; 11,517 years before the Christian era; and 7625 years before the beginning assigned by Lepsius to the reign of Menes, the founder of Memphis; of man, moreover, in a state of civilization, so far, at least, as to be able to fashion clay into vessels, and to know how to harden them by the action of a strong heat.

February 18, 1858.

LEONARD HORNER, Esq., Vice-President, in the Chair.

In accordance with notice given at last Meeting, the Lord Talbot de Malahide was balloted for and duly elected a Fellow of the Society.

The following communication was read:—

"On the Functions of the Tympanum." By JAMES JAGO, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary. Communicated by Prof. STOKES, Sec. R. S. Received January 23, 1858.

(Abstract.)

As in my present effort to obtain further light upon some of the still obscure points in the physiology of the ear I have been *primarily* guided by observations made upon my own ears, I should premise that both are very efficient for hearing; but that they differ from each other in the important particular that the faucial orifice of the right Eustachian tube closes much less tightly than that of the left, insomuch that there are times when the former becomes quite patent, with no disposition to collapse. Again, having lately been troubled for above five weeks with a *tympanic* deafness, I carefully registered a series of auditory phenomena resulting therefrom, and

found them exceedingly noteworthy. Lastly, I have made certain experiments upon the external auditory canals of the sound ears.

I compare, then, with one another, the phenomena yielded by a normal ear, an ear with an open Eustachian tube, an ear with the drum impaired in a particular manner, and an ear whose external meatus is in a known altered condition; calling in facts from other sources in aid; and, finally, endeavour to determine the uses to be assigned to the several structures of the drum in order to embrace all the phenomena*.

I assure myself that my Eustachian tubes are ordinarily shut, by the difficulty (greater for my left one) of forcing the breath into the drums when I stop my mouth and nose, and the hinderment to its escape till I swallow or eructate, showing that those acts open the tubes. If we mark the sinking-in of the lachrymal sac when we swallow with the mouth and nose stopped, we may see that the naso-guttural cavity *enlarges* as the glottis is closed in that act, *producing a partial vacuum* in the drums, and therefore from the greater barometric pressure a feeling of tightness upon the membrana tympani, whilst from loss of usual pressure the Eustachian tubes thereupon close more firmly, and the faucial parts swell and stick together.

I can readily distinguish the act of opening the Eustachian tube from all other guttural ones, both by hearing and feeling. A tearing sound, or an irregular run of clicks, marks a slower, a sharp click a quicker opening of the tube, a *souffle* the rush of air through the patent tube, and a small crack the displacement of the membrana tympani. I frequently perceive these phenomena in deglutition, though, owing to the strong pressure of the current of ejected air in the fauces, more especially in eructation. Sometimes also in yawning, showing that a sundering contraction of the muscles of the pharynx and palate attends the opening of the tube.

With the tube patent I feel the membrana tympani, as expiration and inspiration alternate the greater amount of pressure in its two surfaces, oscillating from outwards to inwards, as the inner canthus of the eye, as reached by the nasal duct, may be seen to do. In violent explosive expirations, the strength of the membrane is

* A note shows, that though I speak particularly from these sources, the results rest on much broader grounds; and mentions how far anything like any portion of this paper has been previously published by myself or others.

severely tested; the mildest speaking, coughing, or sneezing even, is always disagreeably felt thereon.

But to pass to the attendant sonorous phenomena:—the rippling of the air in the tube at each elevation and depression of the ribs expresses itself by a *souffle,* and every word I utter is taken to the labyrinth directly through the tube with a force that proves annoying;—observations which plainly evince why the Eustachians are usually impervious, and why they almost never open except at that instant of deglutition, or of the reverse act, eructation, which occludes the glottis.

From numberless observations, I am able to affirm that the faculty of audition is not at all deteriorated by patency of the tubes, however the ordinary use of the ear may be perplexed by sounds entering the tube. Nor does stretching the membrana tympani, by augmenting or diminishing the aërial pressure on its inner surface, enfeeble hearing.

I will now turn to observations made upon my left ear when it was deafened. I show that the external meatus was unaffected; and if I rubbed my finger over the skin covering the bone behind the ear, or carried the ticking of a watch to the bottom of the meatus by means of a metallic probe, and then did the like to the other ear, I heard well, and as well upon one as the other. Hence the labyrinth and acoustic nerve remained healthy, and the drum alone was affected. Singing noises in the head had been developed just to the same extent as hearing had been blunted,—phenomena that for three weeks before an instantaneous cure remained *quite unchanged.*

The noises were caused by the circulation of the blood about the drum, for they rose and fell as the circulation was quick or otherwise. And I was led to the belief that these noises were not created by any morbid change of local circulation, *but that, by a morbid change in the acoustic properties of the tympanum, ordinary movements of the blood thereabouts were heard in a multiplied manner*; for the click and souffle from air entering the Eustachian tube, as heard in the healthy ear, were wonderfully magnified in the deaf one. The louder souffle, that of eructation, normally but very weak, even when the intruding air strongly forces outwards the membrana tympani, in the deaf ear was always a very pronounced *bruit.* And a couple of other sounds from distinct sources generated within the

site of the membrane are described, which, hardly audible in a normal ear, are loud in an ear thus diseased.

Thus a group of phenomena beckon to the inference, that this deafness had so modified the acoustic properties of the drum, as *both* to render all sonorous vibrations affecting the air within it by far more audible than before, and all those entering the meatus auditorius externus as much less audible than before. What physical cause can bring about these *inverse* effects?

1. If the fenestra rotunda be the chief portal for sound, no change at it could render one set of sounds more audible without doing so for the other also.

2. If sound be mainly conveyed to the labyrinth by undulatory displacements of the membrana tympani, causing *bodily* oscillation of the ossicles, the membrane could not be rendered more responsive to aërial waves falling upon one side of it without becoming equally so for those falling upon its other.

3. If the fenestra rotunda chiefly afford passage to sound, and the membrana tympani has acquired an abnormally high reflecting power, repelling vibrations that would heretofore have escaped through it from the drum back upon this fenestra, and those that fall upon its outer surface back through the meatus, effects of an *inverse* kind do result. This hypothesis, therefore, cannot be rejected without a careful consideration.

Let us inquire, then, what influence the existence of a membrana tympani would, under this supposition, exert on hearing. Sonorous vibrations impressed upon the walls of the head, that is, of the external meatus, are heard more loudly when we anyhow cover this canal so as to close it, as any cavity when closed resounds like an open one of greater size (J. Müller). In again testing this principle, I have used various materials for closing the meatus, have plugged the entrance, and laid the thing over it, and observe always that the smallest orifice in the occluding body detracts from the resonance; which I know to occur in the confined air, and not in the parietes of the canal, for my deafened ear was deaf to it. Such experiments, however, do not evince that the membrane aids *hearing* by resonance, but the contrary. Dealing with vibrations already existing in the walls of the cavity insulating the air, they do not at all imitate the case of vibrations passing into the tympanum *through a*

medium,—the membrane. As no substance can be applied over the meatus, however it be done, which does not hinder our hearing of external sounds just as much as it occasions resonance of parietal ones, the membrane on this supposition must in some degree or other be a positive detriment to the auditory function. Besides, were hearing aided by resonance within the drum, a patent Eustachian tube by allowing vibrations to disperse must impair hearing, which I know *not* to be the case. Again, if we assume the membrane to but slightly arrest the transition of sound from the outer to the tympanic air, to be, in short, an unavoidable impediment to hearing, fulfilling some non-acoustic purpose, the loss of it would not prove at once, as it does, a serious detriment to hearing rather than some benefit. I may append too, that were it but a trifling obstacle, the group of sounds occurring within it, so described, should be augmented by resonance in the external meatus, on its outlet being stopped; yet I can detect nothing of the sort. Further, I squeezed a plug of chewed brown paper, and one of dry paper, firmly into the bottom of the meatus of the healthy ear, against the membrane, covered the membrane with a stratum of wax, and filled the meatus with water; but in not one of these experiments were the said group of sounds rendered louder. So that it appears that the application to the membrane of even a highly reflecting surface fails to intercept and cause to return intra-tympanic sounds, which can only be because the membrane is difficult for such sound to pass through. But if the membrane highly resists the transition of aërial vibrations, it (the fenestra rotunda being the chief portal) is a serious detriment to hearing. Hence this fenestra cannot be of this acoustic consequence. And we must have recourse to the only other theory which suggests itself, which is—

4. *That the membrane and ossicles form the essential path for sonorous vibrations, which traverse it by the mode of condensation and rarefaction; that aërial ones impinging upon the outer surface of the membrane easily impress themselves upon its substance, and pass into the ossicles, whilst the inner surface presents a great obstacle to their escape into the air in the drum, and equally repels vibrations that fall upon it from this air.* Thus, when disease nullifies the great reflecting qualities of the inner surface, much of the sound from without passes into the drum and is wasted, or deafness re-

sults; whilst much of that in the drum enters the membrane, and some of it finds its way along the ossicles, and noises in the head are engendered.

Now I find that the cutaneous surface of the drum-head admits vibrations from air with very much greater facility than water does, that is, readily; for on filling the external meatus of the sound ear with water, and then letting it leak out again, I remarked that for more than half an hour afterwards septa of water were constantly forming themselves across the canal and producing much deafness, and then breaking again with a loud noise, and the deafness vanishing. After some evaporation the following instructive effects alone took place:—the membrane would attract a film of water over its surface, and deafness ensue; but on a gust of air plunging into the drum through the Eustachian tube, the membrane springing outwards with a smart smack, would throw off the fluid, and the hearing as *instantaneously* be restored. This would *gradually* wane away again by the re-attraction of the water, to be instantaneously regained again, and so on. But since the transition is easy between the membrane's outer surface and air, what has been said above shows that it must be difficult between the inner surface and air, and the statement in (4.) is demonstrated.

Accordingly the external layer of the membrane is formed of skin, a dry tissue of loose texture, penetrable by air, and coming into intimate relation with it; whilst the mucous membrane of the drum is, as it were, unparalleled not only for tenuity, but compactness and high vascularity, though it is barely possible to verify the presence of mucous exudation upon it, affording a glassy surface which is a formidable barrier to the passage of vibrations from it to air, and *vice versâ*; and this is so reflected, that the membrane and ossicles leading to the labyrinth lie *without* it, confining useful vibrations to their destined path, and excluding hurtful ones from it; and the mastoid cells help to further stifle such vibrations as by any accident intrude upon the air in the drum. The membrane of the fenestra rotunda, by its elasticity, protects the acoustic nerve from undue compression, &c. The membrana tympani avails acoustically by its *area*, whilst its flexibility, the joints in the ossicular chain, &c., are mere machinery for conveying, under all contingencies, vibrations to the fenestra ovalis, and provision against mechanical accidents. The

structure of the labyrinth admits of explanation, in a great degree, upon like principles.

The personal case of deafness studied in this paper was from a cold draught on the ear, a mere inflammation of the mucous lining of the drum, ultimately forming a layer of dried mucus upon the membrana tympani, which originally involving much air-bubbles, remained very permeable by air, and assimilated acoustically the inner face of the membrane to the cuticular outer one. The instantaneous dispersion of the noises and deafness was caused by the sudden peeling off of this false cuticle; whilst a film of water upon the cuticular face assimilates that to the inner one, when the ear excludes both tympanic and outer sounds from the labyrinth. Deafness produced by disease in the external meatus only yields noises when it propagates irritation so as to excite secretion of mucus on the inner face of the drum-head. Simple perforation of the drum-head only deafens in proportion to the extent of surface removed. If there co-exist a more or less fluid discharge from the drum, this spoils hearing by covering the cuticular surface of the membrane, though it may not deviate so much acoustically from that lined by mucous membrane as to very materially damage it. To remedy such deafness mechanically, we should first essay to rescue the cuticular face from the fluid by placing some material to draw off the discharge from it, so as to keep the membrane fit for its duties, and still exposed to aërial vibrations. If the mischief is so extensive that we are obliged to employ some disc to rest against the remaining ossicles as a substitute for the true membrane, we should try to form one with surfaces acoustically imitating those of the membrane itself.

The paper concludes by pointing out how the various injuries which have been known to occur by disease or otherwise to the different parts of the tympanum, are readily accounted for by the functional hypothesis here submitted.

February 25, 1858.

WILLIAM R. GROVE, Esq., Vice-President, in the Chair.

Charles Piazzi Smyth, Esq., was admitted into the Society.

The following communications were read :—

I. "Remarks on the interior Melting of Ice." By Professor WILLIAM THOMSON, F.R.S. In a Letter to Professor STOKES, Sec. R.S. Received January 23, 1858.

In the Number of the 'Proceedings' just published, which I received yesterday, I see some very interesting experiments described in a communication by Dr. Tyndall, "On some Physical Properties of Ice." I write to you to point out that they afford direct ocular evidence of my brother's theory of the plasticity of ice, published in the 'Proceedings' of the 7th of May last; and to add, on my own part, a physical explanation of the blue veins in glaciers, and of the lamellar structure which Dr. Tyndall has shown to be induced in ice by pressure, as described in the sixth section of his paper.

Thus, my brother, in his paper of last May, says, "If we commence with the consideration of a mass of ice perfectly free from porosity, and free from liquid particles diffused through its substance, and if we suppose it to be kept in an atmosphere at or above 0° Cent., then, as soon as pressure is applied to it, pores occupied by liquid water must instantly be formed in the compressed parts, in accordance with the fundamental principle of the explanation I have propounded—the lowering, namely, of the freezing-point or melting-point, by pressure, and the fact that ice cannot exist at 0° Cent. under a pressure exceeding that of the atmosphere." Dr. Tyndall finds that when a cylinder of ice is placed between two slabs of box-wood, and subjected to gradually increasing pressure, a dim cloudy appearance is observed, which he finds is due to the melting of small portions of the ice in the interior of the mass. The permeation into portions of the ice for a time clear "by the water squeezed against it from such parts as may be directly subjected to the pressure," theoretically demonstrated by my brother, is beautifully illustrated by

Dr. Tyndall's statement, that "the hazy surfaces produced by the compression of the mass were observed to be in a state of intense commotion, which followed closely upon the edge of the surface as it advanced through the solid. It is finally shown that these surfaces are due to the liquefaction of the ice in planes perpendicular to the pressure."

There can be no doubt but that the "oscillations" in the melting-point of ice, and the distinction between strong and weak pieces in this respect, described by Dr. Tyndall in the second section of his paper, are consequences of the varying pressures which different portions of a mass of ice must experience when portions within it become liquefied.

The elevation of the melting temperature which my brother's theory shows must be produced by diminishing the pressure of ice below the atmospheric pressure, and to which I alluded as a subject for experimental illustration, in the article describing my experimental demonstration of the lowering effect of pressure (Proceedings, Roy. Soc. Edinb. Feb. 1850), demonstrates that a vesicle of water cannot form in the interior of a solid of ice except at a temperature higher than 0° Cent. This is a conclusion which Dr. Tyndall expresses as a result of mechanical considerations: thus, "Regarding heat as a mode of motion," "liberty of liquidity is attained by the molecules at the surface of a mass of ice before the molecules at the centre of the mass can attain this liberty."

The physical theory shows that a removal of the atmospheric pressure would raise the melting-point of ice by $\frac{3}{400}$ths of a degree Centigrade. Hence it is certain that the interior of a solid of ice, heated by the condensation of solar rays by a lens, will rise to at least that excess of temperature above the superficial parts. It appears very nearly certain that cohesion will prevent the evolution of a bubble of vapour of water in a vesicle of water forming by this process in the interior of a mass of ice, until a high "negative pressure" has been reached, that is to say, until cohesion has been called largely into operation, especially if the water and ice contain little or no air by absorption (just as water freed from air may be raised considerably above its boiling-point under any non-evanescent hydrostatic pressure). Hence it appears nearly certain that the interior of a block of ice originally clear, and made to possess vesicles of water by

the concentration of radiant heat, as in the beautiful experiments described by Dr. Tyndall in the commencement of his paper, will rise very considerably in temperature, while the vesicles enlarge under the continued influence of the heat received by radiation through the cooler enveloping ice and through the fluid medium (air and a watery film, or water) touching it all round, which is necessarily at 0° Cent. where it touches the solid.

I find I have not time to execute my intention of sending you to-day a physical explanation of the blue veins of glaciers which occurred to me last May, but I hope to be able to send it in a short time.

WILLIAM THOMSON.

Jan. 21, 1858.

II. "On the Practical Use of the Aneroid Barometer as an Orometer." By Captain W. S. MOORSOM, Member of the Institution of Civil Engineers. Communicated by P. W. BARLOW, Esq. Received January 28, 1858.

A Government Commission to Ceylon in the beginning of 1857, led the author, as Chief Engineer in charge of the Expedition, to provide (among other instruments) some aneroids, as a means of saving time in ascertaining the levels of the mountain passes of that Island. The aneroids offered by makers did not appear sufficiently graduated to admit of minute observation, and at the author's suggestion Messrs. Elliott furnished a more complete vernier, which, however, was shown to be susceptible of material improvement.

With these comparatively imperfect instruments, it was shown that an elevation of 950 feet may be taken to correspond with the fall through the first inch of the aneroid; that about 970 feet more corresponds with the fall through the second inch, and about 1000 feet corresponds with the fall through the third inch. These altitudes having been checked by levels taken with the ordinary surveyor's spirit-level, it was shown that this experience corresponds with the Tables published by M. Bellville, within 1 per cent.

The thermometer, which is usually attached to the aneroid, is not a necessary adjunct, but is frequently useful, and always interesting. The compensations introduced to provide against variations of tempe-

rature, as affecting the results given by the instrument, were shown to be effectual without the aid of the thermometer.

The difficulties to be contended with in taking accurate observations were shown to be local variation, diurnal variation, and some irregularity in the action of the mechanical parts of the instrument itself. These difficulties were examined *seriatim*, and modes of approaching to their corrections were explained. The modes of compensation for variations of temperature affecting the instrument were shown as at present practised by the makers: the diaphragm-box being compensated by means of the introduction of a small portion of aëriform fluid, instead of being allowed to act with a perfect vacuum, and the metallic connexions between the diaphragm-box and the index being compensated by compound arms or connexions of steel and brass so adjusted as to neutralize mutually the respective contraction or expansion of each at variations extending to 100 degrees of temperature.

The mode now practised by makers of graduating the aneroid (when thus compensated) by comparison with a standard mercurial barometer, was stated, and it was suggested that improvement on this practice might be made by reference to standard elevations running up to 2000 feet at least in Great Britain. Practical examples were given of the use of the instrument in Ceylon, showing the variations of the aneroid (when properly checked) to lie between 1 foot and 6 feet, as compared with the surveyor's spirit-level: other examples were given of practice on the Great Western, South-Eastern, and North Kent Railways, varying from the true levels from 6 inches to 6 feet, over distances of between 300 and 400 miles.

The paper concluded with Tables in the Appendix, and with diagrams explanatory of the construction of the instrument; the Tables being intended to illustrate the effects of diurnal and also of local variation within the tropics (in Ceylon), and also in England.

March 4, 1858.

The LORD WROTTESLEY, President, in the Chair.

The President announced that the Chemical Society had met and adjourned, in order to attend the Bakerian Lecture, and would, with permission of the Meeting, be present.

The Lord Talbot de Malahide was admitted into the Society.

In accordance with the Statutes, the Secretary read the following list of Candidates for Election into the Society:—

Thomas Graham Balfour, M.D.
John Bateman, Esq.
Henry Foster Baxter, Esq.
Samuel Husbands Beckles, Esq.
Edward Mounier Boxer, Capt. R.A.
William Brinton, M.D.
Frederick Crace Calvert, Esq.
Thomas Russell Crampton, Esq.
Frederick Currey, Esq., M.A.
Hugh Welch Diamond, M.D.
Thomas Rowe Edmonds, Esq., B.A.
David Forbes, Esq.
S. W. Fullom, Esq.
Francis Galton, Esq.
Alfred Baring Garrod, M.D.
William Henry Harvey, M.D.
Rev. Samuel Haughton.
Henry Hennessy, Esq.
Henry Letheby, M.B.
David Livingstone, LL.D.
Edward Joseph Lowe, Esq.
John Lubbock, Esq.
David Macloughlin, M.D.
Capt. Rochfort Maguire, R.N.
Capt. William Searth Moorsom.
Robert William Mylne, Esq.
William Newmarch, Esq.
William Peters, Esq.
Henry Darwin Rogers, LL.D.
William Scovell Savory, M.B.
Sir Robert Schomburgk.
Edward Smith, M.D.
Warington Wilkinson Smyth, Esq., M.A.
Col. Andrew Scott Waugh, B.E.
Thomas Williams, M.D.
Bennet Woodcroft, Esq.

The Bakerian Lecture was delivered by John P. Gassiot, Esq., V.P.R.S., "On the Stratifications and Dark Bands in Electrical Discharges as observed in Torricellian Vacuums."

The Lecturer gave an exposition of the substance of a Paper, communicated by him under the above title, and illustrated his Lecture by a repetition, before the Society, of the Experiments described. The following is an abstract :—

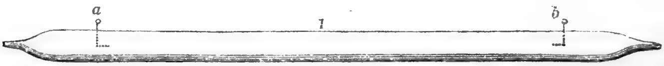
The author refers to the stratified appearance of the electrical discharge when taken from the terminals of a Ruhmkorff's induction-coil in the vapour of phosphorus, and in highly attenuated gases, first noticed by Mr. Grove (Phil. Trans. Part I. 1852, and Phil. Mag., Dec. 1852). Having witnessed the experiments of Mr. Grove, Mr. Gassiot in the same year examined the discharge in a barometrical vacuum in which the mercury had been carefully boiled, but he could not obtain any signs of stratification. These experiments were subsequently repeated by several continental electricians, whom he names, and who all describe the induction-discharge in a barometrical vacuum as intensely white, and filling the whole tube without stratification.

After alluding to the experiments of the Rev. Dr. Robinson (Proc. R. I. Acad., Dec. 1856), and to some recent improvements in the construction of the induction-coil, the author proceeds to describe apparatus which he had constructed for the more careful examination of the character of the induction-discharge. His first experiments were made on glass tubes about 10 inches long, in which the mercury could be lowered or raised to any required level by means of the air-pump. He also experimented with barometrical vacuums obtained by inverting a tube of about 44 inches in length, filled with boiled mercury, over a vessel containing that metal, and then sealing the tube 2 or 3 inches above the barometrical height.

The results obtained by these methods having been found unsatisfactory, the author had recourse to that first suggested by Mr. Welsh (Phil. Trans. 1856, p. 507), by which that gentleman constructed the large barometer at the Kew Observatory. Following

out the principles indicated by Mr. Welsh, by carefully removing all trace of moisture, and thoroughly cleaning the tubes before introducing the mercury, the author succeeded in obtaining Torricellian vacuums which exhibit the stratifications in a uniform and very marked manner.

The sealed tubes generally used by Mr. Gassiot are then described. They are made of the usual glass tubing, about an inch internal diameter, and of the form fig. 1.



They vary from 10 to 38 inches in length. In the latter case the platinum wires *a b* are about 32 inches apart. One tube is described 5 feet 3 inches in length, with wires 4 feet 9 inches apart.

With a tube prepared on Mr. Welsh's principle, and the usual-sized Ruhmkorff's induction-coil excited by a single cell of Grove's nitric acid battery, with or without a condenser, the phenomena of the stratified discharge can be seen and examined with ease, and without the trouble and uncertain manipulation of an air-pump, or the employment of phosphorous or other vapours.

If the discharges are made in one direction, a black deposit takes place on the sides of the tube nearest the negative terminal. This deposit is platinum in a state of minute division emanating from the wire, which becomes black and rough as if corroded. The minute particles of platinum are deposited in a lateral direction from the negative wire, and consequently in a different manner from what is described as occurring in the voltaic arc (De la Rive's 'Electricity,' vol. ii. p. 288), so that the luminous appearance of discharge from the induction-machine can in no way arise from the emanation of particles of the metal.

The author describes a series of experiments made in the apparatus first prepared, by which the mercury is lowered or raised in the vacuum tube; he describes the peculiar appearance when the mercury is made either positive or negative. In some instances, and particularly when, instead of wires, platinum balls $\frac{1}{8}$th of an inch in diameter were used for terminals, the stratifications instantly ceased when the mercury rose above the negative ball; but when the pole

of a magnet was presented to the positive ball, the stratifications were drawn to the length of two or three inches down the tube.

In the sealed tubes the stratified discharge was obtained by frictional electricity; and if a charged Leyden jar is discharged through the vacuum by a wet string, the stratifications are as distinct as from the induction-coil.

The author next proceeds to show, that by a single disruptive discharge of the primary current excited by a single cell, the entire tube, whatever may be its length, is filled with stratifications as far as the dark band near the negative wire; and from this experiment he is of opinion that the phenomenon cannot be in any way due to the vibrations of the contact-breaker. With one, two or three cells no appearance of a luminous discharge could be perceived on making contact, it only appeared on breaking. If, however, the intensity of the primary current is increased by using ten or more cells, stratifications appear on making as well as on breaking the contact of the primary circuit. These stratifications are always concave towards the positive terminal, and as the discharges, on making and on breaking, emanate from different terminals, their concavities are in opposite directions,—a fact which explains the different ways in which several electricians have described and figured the form of the discharge with the coil. These stratifications appear in quick succession, but they can always be separated in any part of the tube by a magnet.

Under certain conditions the positive discharge assumes a peculiar form, of which the author gives a drawing. He considers that this exhibits a direction of a force from the positive to the negative, centering to the axis of stratification, which cannot be connected with the passage of particles, and that the latter phenomenon, as it occurs in the voltaic arc, may be but the result of a secondary action.

The author notices the peculiar difference between the positive and negative discharge; he describes an apparatus by which both terminals could be made of surfaces of mercury, or the positive of a surface of mercury, and the negative of a wire, or the reverse. In this apparatus, moreover, the mercury at one end could be elongated 8 or 10 inches. When the mercury was negative, its entire surface was covered with a brilliant glow; when positive, the extreme point

of the mercury exhibited intense light, but the remainder of the surface appeared unaffected by the discharge. In order to test whether any signs of interference could be detected, he had a tube prepared with four wires, by which discharges could be observed when taken from separate coils, as shown in fig. 2, where *a b* and *a′ b′* are

platinum wires hermetically sealed, as in the previously described apparatus. Care was taken to manipulate with induction-coils giving discharges of equal intensity; but in no case did any sign of interference appear. The discharges, whether in the same or in opposite directions, mingled; the stratifications, having a tendency to rotate round the poles of a magnet and obeying the well-known law of magnetic rotations, could be separated by either pole.

If, instead of sealed wires, tin-foil coatings, *a b* (fig. 3), are placed

on the vacuum tube, and the coatings are attached to the terminals of the induction-apparatus, brilliant stratifications immediately appear in the portion of the vacuum between the coatings, but without any dark discharge. On approaching a powerful magnet, the stratifications divide into two equal series, in which the bands or strata are concave in opposite directions.

If a vacuum tube, with or without wires or coatings, is placed on the induction-coil, or on the prime conductor of an electrical machine, stratifications appear which are divided by the magnet. Having thus ascertained that there are two distinct forms of the stratified electrical discharge, the author, for the sake of clearness of expression, terms them the direct and the induced discharge. The direct discharge is that which is visible in a vacuum when taken from two wires hermetically sealed therein; this discharge has a tendency to rotate, as a whole, round the poles of a magnet. The induced discharge is that which is visible in the same vacuum when taken from two metallic coatings attached to the outside of the tube, or from one coating and one wire; this discharge is divided by the magnet,

and the two divisions have a tendency to rotate in opposite directions. The character of these two forms of electrical discharge can always be determined by the magnet.

The author concludes his paper in the following words:—"I refrain for the present from any observations as to the action of the magnet on the discharge. The intimate relation of magnetic and electric action has long since been shown; but the curious effect of the power of a magnet to draw out the stratifications from the positive terminal, and in some instances its powerful action on that portion of the discharge which exhibits the phosphorescent light in its greatest intensity, are worthy of further examination. In the preceding experiments my object was directed to the examination of the stratified and of the dark band discharge; at present I am inclined to the opinion that the stratifications in the positive, and the dark band between it and the negative glow, although apparently similar, are effects arising from distinct causes—the former from pulsations or impulses of a force acting in a highly attenuated but resisting medium, the latter from interference. I am at this time engaged in making further experiments for the elucidation of this novel and remarkable phenomenon."

March 11, 1858.

Dr. HOOKER, Vice-President, in the Chair.

The following communications were read:—

I. Notes of Researches on the Poly-Ammonias. By Aug. W. Hofmann, Ph.D., F.R.S. &c. Received February 4, 1858.

Former investigations had led me to some general conclusions regarding the molecular constitution of the organic bases, which I have communicated to the Royal Society, and which have been published in the 'Philosophical Transactions' (1850, p. 93; 1851, p. 357). My experiments had proved that each equivalent of hydrogen in

ammonium may be replaced by an equivalent of a mono-atomic electro-positive radical, such as methyl, ethyl, &c.;—a series of compound ammoniums being produced, the salts of which may be thus formulated:—

$$N\left\{\begin{matrix}H\\H\\H\\H\end{matrix}\right\}Cl.$$

$$N\left\{\begin{matrix}R'\\H\\H\\H\end{matrix}\right\}Cl.$$

$$N\left\{\begin{matrix}R'\\R'\\H\\H\end{matrix}\right\}Cl.$$

$$N\left\{\begin{matrix}R'\\R'\\R'\\H\end{matrix}\right\}Cl.$$

$$N\left\{\begin{matrix}R'\\R'\\R'\\R'\end{matrix}\right\}Cl.$$

R′ representing a mono-atomic electro-positive radical.

These successive substitutions were accomplished by the action of ammonia upon the bromides and iodides of the alcohol-radicals, which since that time have become most valuable agents of substitution in the hands of chemists.

All the bases produced by this process being derived from 1 equiv. of ammonium, contain 1 equiv. of nitrogen; they differ in this respect essentially from the majority of the alkaloids extracted from plants, and more particularly so from those which, like quinine, morphine, strychnine, &c., specially claim our interest. By far the greater number of the vegetable alkaloids contain 2 equivs. of nitrogen. In some vegetable and animal bases we find even 3 and 4 equivs. of nitrogen. The molecular construction of these bodies is still obscure, but it is extremely probable that they are derived from 2, 3 or 4 ammonia equivs., in which the hydrogen is more or less replaced by poly-atomic molecules, and that the stability of such complicated

structures essentially depends upon the substituting capacity of their replacing molecules.

It was long my intention to extend my researches to the poly-ammonium bases. But my attention has been specially called to the subject by the beautiful results obtained of late, especially in France, by the study of the poly-acid alcohols, by the experiments of M. Berthelot, and more particularly by the classical researches of M. Wurtz, which enable us to take a general view of this subject.

Taking as a point of departure the neutral compounds which are formed by the action of ammonia upon bibasic and tribasic acids, the *diamides* and *triamides,* derived respectively from 2 or 3 equivs. of ammonia, it became extremely probable that the action of ammonia upon poly-acid alcohols would give rise to poly-ammonium bases. In the conception of this analogy there appeared but little doubt that ammonia, under the influence of the bromides and iodides of bi-acid alcohols, would furnish a series of bi-ammonium bases, exactly as treatment of ammonia with the analogous compounds of mono-acid alcohols has given rise to the formation of the mon-ammonium bases above referred to. In other words, it was to be expected that a compound ether $R'' Br_2$ or $R'' I_2$ (R'' representing a bi-atomic electro-positive radical) would act upon two equivalents of ammonia, producing a series of salts expressed by the following formulæ :—

$$N_2\left\{\begin{matrix} R'' \\ H_2 \\ H_2 \\ H_2 \end{matrix}\right\}Br_2.$$

$$N_2\left\{\begin{matrix} R'' \\ R'' \\ H_2 \\ H_2 \end{matrix}\right\}Br_2.$$

$$N_2\left\{\begin{matrix} R'' \\ R'' \\ R'' \\ H_2 \end{matrix}\right\}Br_2.$$

$$N_2\left\{\begin{matrix} R'' \\ R'' \\ R'' \\ R'' \end{matrix}\right\}Br_2.$$

In endeavouring experimentally to verify this idea, it became necessary

to examine what had hitherto been done in this direction. Science possesses already some very interesting observations on the ammonia derivatives of bi-acid alcohols. About five years ago, soon after the publication of my experiments upon the action of ammonia upon bromide and iodide of ethyl, M. Cloëz* obtained a series of bases, on submitting ammonia to the action of the brominetted Dutch liquid ($C_4 H_4 Br_2$). Two of these bodies he described under the name of formylia and acetylia, whilst a third body subsequently obtained is designated by the term propylia†.

To these three bodies M. Cloëz attributes the following formulæ :—

Formylia	$C_2 H_3 N$
Acetylia	$C_4 H_5 N$
Propylia	$C_6 H_7 N$.

At a later period M. Natanson has studied the action of ammonia on the chlorinetted Dutch liquid ($C_4 H_4 Cl_2$). This reaction produces analogous results, but the number of bases is smaller, the chief product being a chloride, which contains a base either identical or isomeric with the acetylia of M. Cloëz.

When carefully considering the results obtained by M. Cloëz, it appeared to me probable that the bases which he describes, are in fact the di-ammonium compounds for which I was searching. The constitution assigned by M. Cloëz to his substances is not very probable. It is difficult to understand how the action of ammonia upon a compound like the Dutch liquid can produce simultaneously three bodies belonging to three different homologous families, the formyl-, acetyl-, and propyl-series. Our doubts are, however, increased if we examine into the physical characters of these bodies, especially if we consider their high boiling temperatures, and the differences between the boiling-points of the three bases :—

Formylia....	$C_2 H_3 N$	123°	difference 47.
Acetylia	$C_4 H_5 N$	170°	
Propylia	$C_6 H_7 N$	210°	difference 40.

Methylamin, $C_2 H_5 N$, which contains only 2 equivalents of hydrogen more than formylia, is at the common temperature a gas, and

* Instit. 1853, 213. † Cahours, Leçons de Chimie Générale, t. ii. p. 654.

liquefies only considerably below the freezing-point of water. Again, the differences of the boiling-points of substances, related in the way that the formulæ of M. Cloëz suppose, do not often exceed 20°, and very rarely rise to 40° and 47°.

All these difficulties disappear by submitting the formulæ of M. Cloëz to a slight alteration, and by regarding formylia, acetylia and propylia as the di-ammonium bases of the same series, of the ethylene series. If we adopt this view, the three bodies are derived from 2 of ammonia, in which 2, 4 or 6 equivalents of hydrogen are replaced respectively by 1, 2 or 3 equivalents of the bi-atomic molecule ethylene; and the formylia, acetylia and propylia of M. Cloëz present themselves as monethylene-diamine, diethylene-diamine and triethylene-diamine.

I have endeavoured experimentally to solve this question. The analysis of acetylia, which is remarkable for the definite character of its salts, appeared to promise an answer to it.

When repeating the beautiful experiments of M. Cloëz, I had occasion to confirm all the indications given by this able chemist, regarding the formation of the bases derived from bibromide of ethylene. The analysis, however, furnished a discrepant result.

M. Cloëz represents formylia by the formula

$$C_2\,H_3\,N,$$

when the hydrochlorate becomes

$$C_2\,H_3\,N,\ H\,Cl = C_2\,H_4\,N\,Cl.$$

When considered as a di-ammonium compound, this salt has the composition

$$C_4\,H_8\,N_2,\ 2\,HCl = C_4\,H_{10}\,N_2\,Cl_2 = 2C_2\,H_5\,N\,Cl.$$

The two formulæ only differ by one equivalent of hydrogen.

The analysis of a magnificently crystallized hydrochlorate has furnished me the following results:—

	Formula of M. Cloëz— $C_2\,H_4\,N\,Cl$.	New formula— $C_4\,H_{10}\,N_2\,Cl_2$.	Mean of analysis.
Carbon	18·32	18·04	17·87
Hydrogen	6·10	7·51	7·55
Chlorine	54·19	53·38	53·17

On preparing the free base by the action of hydrate of potassa

upon the hydrochlorate, I was surprised to find that this body retains hydrogen and oxygen in the proportion in which they exist in water, which cannot be separated by prolonged contact with, or by repeated distillation over, anhydrous baryta.

The analysis of the free base has given the following result:—

	Formula of M. Cloëz— $C_2 H_4 N O$.	New formula— $C_4 H_{10} N_2 O_2$.	Analysis. Mean.
Carbon	31·58	30·76	30·67
Hydrogen	10·52	12·82	12·97
Nitrogen	36·84	35·90	36·32

These numbers appear to me in favour of the formula which I propose for formylia; there remains but little doubt that acetylia and propylia are analogously constituted.

There remains yet to find the last term of the series, the tetrethylene-diammonium compound. Up to the present moment I have only established by experiment that the three lower bases are powerfully attacked by bibromide of ethylene, a non-volatile compound being produced possessing properties in every respect analogous to the character of tetramethyl- and tetrethylammonium.

If further experiments confirm the hypothesis which I have advanced, the action of ammonia on bi-bromide of ethylene would give rise to four compounds analogous to the bases which I have obtained by the action of bromide of ethyl.

Bromide of ethyl-ammonium.	$N\left\{\begin{matrix} C_4 H_5 \\ H \\ H \\ H \end{matrix}\right\} Br.$	Bibromide of ethylene-diammonium.	$N_2\left\{\begin{matrix} (C_4 H_4)'' \\ H_2 \\ H_2 \\ H_2 \end{matrix}\right\} Br_2.$
Bromide of diethyl-ammonium.	$N\left\{\begin{matrix} C_4 H_5 \\ C_4 H_5 \\ H \\ H \end{matrix}\right\} Br.$	Bibromide of diethylene-diammonium.	$N_2\left\{\begin{matrix} (C_4 H_4)'' \\ (C_4 H_4)'' \\ H_2 \\ H_2 \end{matrix}\right\} Br_2.$
Bromide of triethyl-ammonium.	$N\left\{\begin{matrix} C_4 H_5 \\ C_4 H_5 \\ C_4 H_5 \\ H_5 \end{matrix}\right\} Br.$	Bibromide of triethylene-diammonium.	$N_2\left\{\begin{matrix} (C_4 H_4)'' \\ (C_4 H_4)'' \\ (C_4 H_4)'' \\ H \end{matrix}\right\} Br_2.$
Bromide of tetrethyl-ammonium.	$N\left\{\begin{matrix} C_4 H_5 \\ C_4 H_5 \\ C_4 H_5 \\ C_4 H_5 \end{matrix}\right\} Br.$	Bibromide of tetrethylene-diammonium.	$N_2\left\{\begin{matrix} (C_4 H_4)'' \\ (C_4 H_4)'' \\ (C_4 H_4)'' \\ (C_4 H_4)'' \end{matrix}\right\} Br_2.$

The conception of diammonium-compounds has suggested to me

the idea to extend my observations also to the triacid-alcohols, and to submit ammonia to the action of the bodies

$$C_2\,H\,\,Br_3.$$
$$C_4\,H_3\,Br_3.$$
$$C_6\,H_5\,Br_3.$$

Analogy suggested the formation in this reaction of a series of triammonium-bases, the salt of which might be thus formulated:—

$$N_3\left\{\begin{matrix}R'''\\H_3\\H_3\\H_3\end{matrix}\right\}Br_3.$$

$$N_3\left\{\begin{matrix}R'''\\R'''\\H_3\\H_3\end{matrix}\right\}Br_3.$$

$$N_3\left\{\begin{matrix}R'''\\R'''\\R'''\\H_3\end{matrix}\right\}Br_3.$$

$$N_3\left\{\begin{matrix}R'''\\R'''\\R'''\\R'''\end{matrix}\right\}Br_3.$$

I have not yet succeeded in realizing these compounds by treating under several conditions ammonia by the above chlorides and bromides of triacid-alcohols. The processes which I have as yet tried have led to other transformations. A different result is, however, obtained by replacing the ammonia in these processes by amidogen-bases. In this reaction, and especially with aniline and chloroform, a series of beautifully crystallized alkaloids is formed, the study of which engages at present my attention.

In conclusion, I may remark that several of the known basic compounds appear to belong to the triammonium-type.

The cyanethine of Kolbe and Frankland may be viewed as such a compound—

$$C_{18}\,H_{15}\,N_3 = \left\{\begin{matrix}(C_6\,H_5)'''\\(C_6\,H_5)'''\\(C_6\,H_5)'''\end{matrix}\right\}N_3.$$

This substance appears to me to be derived from 3 equivs. of ammonia, in which 3 equivs. of hydrogen are replaced by 3 equivs. of the tri-atomic radical which chemists assume in glycerin-alcohol.

II. "Description of the Skull and Teeth of the *Placodus laticeps*, Ow., with indications of other new Species of *Placodus*, and evidence of the Saurian Nature of that Extinct Genus." By Prof. RICHARD OWEN, F.R.S. &c. Received February 6, 1858.

(Abstract.)

The author premises a brief sketch of the history of the discovery of the fossils referred by Count Münster, Professors Agassiz, Bronn, and Meyer to the Pycnodont family of Ganoid Fishes, under the generic name of *Placodus*; and then enters upon the anatomical grounds on which he concludes that the *Placodus* is a Saurian reptile. These are stated to be, principally,—1, distinct external bony nostrils, divided by an ascending process of the premaxillary, and bounded by that bone, the maxillaries and nasals; 2, orbits circumscribed below by the superior maxillary and malar bones; 3, temporal fossæ of great size and width, bounded externally by two zygomatic arches, the upper formed by the postfrontal and mastoid, the lower formed by the malar and squamosal; 4, the tympanic bone formed by one bony piece, with a trochlear lower articular surface; the limitation of the teeth to the premaxillary, maxillary, palatine, and pterygoid bones, in the upper jaw, with a demonstrated absence of a median vomerine series, such as exists in the true Pycnodonts. With these proofs of the reptilian nature of the *Placodus*, Prof. Owen combines others exemplifying its affinities to the Lacertian order, and more especially with that modification of *Lacertia* exemplified by the extinct genus *Simosaurus*, from the Muschelkalk.

The author then describes the dentition of the upper jaw of the specimen of *Placodus*, demonstrating the foregoing characters. It includes two premaxillary and three maxillary teeth, forming an outer or marginal series, and two teeth of larger size, forming an inner or palatal series, the last of which is described as the largest grinding tooth in proportion to the size of the head, hitherto known in the animal kingdom.

From the cranial and dental characters the author deduces the specific distinction of his specimen from previously described *Placodi*,

and proposes for it the name of *Placodus laticeps*, in reference to the great breadth of the skull, which equals the entire length, each measuring about 8 inches. All the teeth are implanted in distinct sockets, according to the thecodont type of the Lacertian order. The relation of the large temporal fossæ and of the wide span of the zygomatic arches, to the enormous muscular force required to work the crushing machinery of the jaws, is pointed out.

The structure of the bony nostrils, the orbits, the palate, with other particulars of the cranial anatomy of the *Placodus*, is next described in detail, and compared with the same characters in *Nothosaurus*, *Simosaurus*, *Pistosaurus*, and other Muschelkalk reptiles. The dentition of these Saurians, although, like *Placodus*, thecodont in respect of implantation, is of the ordinary crocodilian type in respect of form, adapted to the prehension of fishes; and there are no palatal teeth. But the author remarks that such teeth exist in the triassic Labyrinthodonts, with a disproportionate magnitude of certain teeth which offers a certain analogy with the dentition of *Placodus*. An account of the microscopic structure of the dentine, enamel, and osseous tissue of the *Placodus* is then given.

The extreme and peculiar modification of the teeth, in respect to form and size, adapting them to the crushing and pounding of hard substances, and the association of the *Placodus* with conchiferous mollusks in such abundance as to have suggested the terms 'Muschelkalk,' 'Terebratuliten-kalk,' and the like, for the strata containing them, concur in evincing the class whence the *Placodi* derived their chief subsistence; and the author points out the relation of a constant disposition of the teeth, in all the known species, to the readier cracking of shelly substance. A single row of teeth in the lower jaw is always opposed to a double row in the upper one, playing, with its strongest line of force, upon their interspace. Thus the crushing force below presses upon a part between the two points of resistance above, on the same principle on which a stick is broken across the knee; only here the fulcrum is at the intermediate point, the moving powers at the two parts grasped by the hands. It is obvious that a shell pressed between two opposite flat surfaces might resist the strongest bite; but, subjected to alternate points of pressure, its fracture is facilitated.

Certain Australian lizards present teeth with large rounded obtuse crowns, like those of certain *Placodi*, and have on that account received the name of *Cyclodus*, for their genus.

The author next proceeds to describe certain specimens of the mandible or under jaw of the genus *Placodus*. The first of these he refers to a species for which he proposes the name of *Placodus pachygnathus*. The second may probably be the lower jaw of the *Placodus Andriani*, Ag.; but should it prove to belong to a different species, the term *bombidens* would best express the specific peculiarity in the shape of the grinding surface of the teeth. A third species is named *Placodus bathygnathus*, in reference to the great vertical extent of the mandibular ramus.

All the above-described fossils are from the Muschelkalk member of the triassic series, near Bayreuth, Germany, and have been recently acquired for the Palæontological Series in the British Museum.

The Memoir is accompanied by numerous drawings.

March 18, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "On the probable Origin of some Magnesian Rocks." By T. STERRY HUNT, Esq., of the Geological Survey of Canada. Communicated by THOMAS GRAHAM, Esq., Master of the Mint. Received July 10, 1857.

The deposits of mineral matters from natural waters offer many points of interest to the geologist. Besides the rock-salt and gypsum, which in many cases have doubtless been formed by the spontaneous evaporation of saline waters, it is well known that many mineral springs charged with carbonic acid under pressure, deposit great quantities of earthy salts when they come to the surface, and that the travertines thus formed often constitute extensive masses. The

deposit from the hot alkaline saline springs of Carlsbad, which forms great beds, was found by Berzelius to consist chiefly of carbonate of lime, with portions of oxide or carbonate of iron, and small quantities of silica, strontia, phosphoric acid and fluorine; the analyses of other chemists have added to the list of elements met with in these and similar precipitates, manganese, zinc, cobalt, nickel, chromium, arsenic, antimony, tin, copper and lead. Carbonate of magnesia is however wanting, or present only in very small proportion in these deposits, and the same is true of the calcareous sinter from cold springs. The Carlsbad water, however, contains for 17 parts of carbonate of lime, 10 parts of carbonate of magnesia; but this latter salt, according to Berzelius, is only deposited after evaporation.

The analyses by Berzelius and Struve of the various carbonated waters of Germany, show that carbonate of lime is generally present in much larger quantities than carbonate of magnesia; and it is only in the waters of Püllna and the Elisenbrunnen of Kreuznach, which contain very little carbonic acid, that we find a large amount of carbonate of magnesia, with a small portion of carbonate of lime. The water of Püllna, according to Struve, contains in 1000 parts, 32·72 of solid matters, consisting of sulphates and chlorides of sodium, magnesium and a little calcium, besides ·10 of carbonate of lime and ·83 of carbonate of magnesia; it contains only $\frac{7}{100}$ths of its volume of carbonic acid gas.

In my analyses of the waters of the western basin of Canada, I have found many brine-springs, which, although rising from Lower Silurian limestones, hold no appreciable amount of earthy carbonates, but contain besides common salt, large quantities of chlorides of calcium and magnesium; they are in fact veritable bitterns. The mineral springs of these palæozoic strata appear to be in all cases connected with undulations producing disruptions of the strata, through which the subterranean waters find egress. In the almost undisturbed region of the west, the springs are consequently rare, but in the disturbed country further east, along the north-western limit of the Green Mountains, which are composed of these same palæozoic strata in an altered condition, the mineral waters become very abundant. Five or six springs, often differing in kind, may sometimes be found within a short distance along the same line of fault, but

where the strata become crystalline, the mineral waters are no longer met with.

In this eastern region the saline waters issuing from the same limestones as the springs just described, are generally more dilute than those of the west, and although, like them, containing but very little carbonic acid, deposit by boiling or evaporation large quantities of earthy salts, chiefly carbonate of magnesia. Many of these waters contain earthy chlorides, and are analogous to the Püllna spring, while others, still strongly saline, are alkaline from the presence of carbonate of soda. The solubility of the carbonate of magnesia in these waters is explained by the observations of H. Rose, who has shown that the partial precipitate produced in the cold, by carbonate of soda in a solution of a neutral salt of magnesia, is redissolved by an excess either of the magnesian salt or the alkaline carbonate, and is only thrown down from these solutions by heat. Longchamp has further remarked, that the precipitation by heat is rendered less complete in proportion as the carbonate, sulphate or hydrochlorate of the alkali is in excess, and that the precipitate at first formed under these circumstances is redissolved on cooling. I have verified this last observation in the case of these natural waters, from which the magnesian carbonate is only separated, when they are evaporated to a small volume. When thus evaporated, even at a very gentle heat, these mineral waters yield large quantities of granular carbonate of magnesia, often nearly pure.

With these facts in view, it is very easy to trace a relation between the saline waters, containing carbonate of magnesia, and another class of springs in which the predominant element is carbonate of soda, with small quantities of common salt, borax and earthy carbonates. These waters, although wanting in the west, are very abundant in eastern Canada, and rise from the same formations as the saline springs, but are most abundant in the argillaceous strata immediately overlying the lower limestones, which appear to be the source of the salines. These alkaline waters probably owe their origin to the slow decomposition of felspathic *débris* in presence of earthy carbonates. By the mingling of these solutions of carbonate of soda with the bitterns of the limestones, the carbonate of lime would be precipitated, except so far as an excess of carbonic acid were present, while car-

bonate of magnesia would remain in solution, as in the Carlsbad waters. The mixture of these alkaline springs with sea-water would yield similar results.

From my analyses of more than sixty of the different mineral springs of Canada, to be found in the published reports of the Geological Survey, I select a few characteristic waters of each class, giving here only approximatively the determinations of the principal ingredients for 1000 parts.

A. Saline waters, containing little or no earthy carbonate.

B. Alkaline waters, feebly saline.

C. Saline waters, holding abundance of earthy carbonates.

a. Neutral, containing earthy chlorides.

b. Alkaline, containing carbonate of soda.

Springs.	Solid matters.	Carb. soda.	Chlor. calcium.	Chlor. magnes.	Carb. lime.	Carb. magnes.
A. Whitby	46·30	...	17·53	9·54	·06	
„ Hallowell	68·00	...	15·90	12·90	...	...
„ Hallowell	36·00	...	9·20	9·40	...	...
B. Chambly	2·13	1·06	...	...	·04	·07
„ Nicolet	1 56	1·13	...	...	not determined	
„ Saint-Ours	·53	·13	...	...	·17	·13
„ Jacques-Cartier	·34	·19	...	...	·07	·03
„ Joly	·75	·23	...	...	·06	·02
C. *a.* Caledonia (V.)	14·64	...	·28	1·03	·12	·86
„ Saint-Léon	13·83	...	·07	·66	·35	·94
„ Caxton	13·65	...	·05	·37	·21	1·06
„ Plantagenet	13·16	...	·13	·24	·03	·89
„ Sainte-Geneviève	20·99	...	·60	2·05	·01	·75
„ Berthier	9·06	...	04	·08	·05	·83
C. *b.* Varennes	9·58	·32	...	...	·35	·35
„ Fitzroy	8·34	·59	...	...	·15	·78
„ Caledonia (1)	7·75	·05	...	...	·15	·52

Few of the above waters contain sulphates, but baryta and strontia are present in very many of them; the amount of these two bases in the Varennes spring is equal to ·016, while in that of Lanoroie, a water of the class B, containing 12·88 of solid matters, there were found ·030 of baryta and ·021 of strontia. Small quantities of silica, alumina, phosphoric acid, manganese and iron are present in all of these springs, and in the alkaline and many of the saline waters a portion of boracic acid; the borate is included with the carbonate

of soda in the above analyses. Bromine and iodine are found in all the saline waters. I have shown, in my analyses of five alkaline saline waters from Caledonia and Varennes, that the amount of carbonic acid is much less than is required to form bicarbonates with the soda, lime and magnesia which these waters contain, so that the magnesia must be held dissolved as a mono-carbonate. In the water of Chambly, on the contrary, there is no deficiency of carbonic acid, and the bases exist as bicarbonates. The temperatures of these springs range from 46° to 53° F.; some of them are therefore to be regarded as slightly thermal.

Interstratified with the shales and sandstones of the Quebec division of the Lower Silurian rocks, which immediately overlie the strata yielding these alkaline waters, are found thick beds of pure limestone, sometimes presenting the agatized structure and semi-translucency which characterize certain travertines, but at other times opaque, homogeneous, and including remains of orthoceratites, trilobites and other fossils. Associated with these beds of pure carbonate of lime are others which are magnesian, and contain considerable quantites of carbonate of iron, which causes them to weather reddish brown. These beds are always granular in texture, and contain a variable portion of siliceous sand; they often become conglomerate, enclosing pebbles of quartz and schist, or more frequently fragments of a pure compact limestone, seemingly identical with that of the beds just described. Thin layers of the ferruginous magnesian rock sometimes separate beds of the pure carbonate of lime, or form lenticular masses in its midst, and seem to replace its fossils. The pure limestones also sometimes form the base of a conglomerate, or are mixed with sand and argillaceous matters.

These magnesian rocks, like the pure limestones of this formation, occur in irregular and interrupted beds; they often attain a thickness of many yards, are destitute of fossils, and contain from ten to forty per cent., and even more, of sand or clay. The portion soluble in acids is sometimes a dolomite with carbonate of iron; at other times the lime is wanting, or present only in traces, and we have a ferruginous magnesite. In two previous notes presented to the Society, I have already explained the manner in which I suppose these siliceous carbonates to have been, in some parts of the forma-

tion, transformed into silicates, such as serpentine, talc, chlorite and pyroxene, by the subsequent intervention of heated solutions of alkaline carbonates.

It appears to me that we may explain the origin of these magnesian deposits, by the spontaneous evaporation of magnesian waters. If the waters of Carlsbad were to become stagnant above their deposited travertine, they would yield by evaporation beds of ferruginous dolomite, and waters like those of Caxton, Plantagenet, and Sainte-Geneviève would furnish carbonate of magnesia nearly free from lime. Nothing forbids us to suppose the existence of waters more highly charged than these with magnesian carbonate, formed perhaps by the action of carbonate of soda upon lagoons of sea-water, whose lime may be removed as carbonate, or by previous evaporation as sulphate. The lagoons in Bessarabia, supplied with the waters of the Black Sea, deposit annually large beds of rock-salt; and it would require only the intervention of waters like those of the natron lakes of Hungary and Egypt, to produce deposits of magnesian carbonate.

The conditions of these deposits at Pointe-Lévis and elsewhere in the Quebec group, seem to point to the existence of basins along an ancient sea-shore, which probably marked the first upheaval of the older Silurian strata. Beds of travertine were there formed, and then the sea flowed over these deposits and gave rise to the fossiliferous limestones; but there were intervals of disturbance, indicated by the conglomerates, and these movements, or the deposition of bars along the shore, gave rise to lagoons or basins cut off from the sea, where, by evaporation under the conditions which we have supposed, magnesian precipitates would be deposited. The absence of fossils in these beds is probably connected with the peculiar composition of the waters.

II. "A Fourth Memoir upon Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received February 11, 1858.

(Abstract.)

The object of the present memoir is the further development of the theory of binary quantics; it should therefore have preceded so much of my third memoir, vol. cxlvii. (1857), p. 627, as relates to ternary quadrics and cubics. The paragraphs are numbered continuously with those of the former memoirs. The first three paragraphs, Nos. 62 to 64, relate to quantics of the general form $(* \between x,y,\ldots)^m$, and they are intended to complete the series of definitions and explanations given in Nos. 54 to 61 of my third memoir; Nos. 68 to 71, although introduced in reference to binary quantics, relate or may be considered as relating to quantics of the like general form. But with these exceptions the memoir relates to binary quantics of any order whatever: viz. Nos. 65 to 80 relate to the covariants and invariants of the degrees 2, 3, and 4; Nos. 81 and 82 (which are introduced somewhat parenthetically) contain the explanation of a process for the calculation of the invariant called the discriminant; Nos. 83 to 85 contain the definitions of the catalecticant, the lambdaic and the canonisant, which are functions occurring in Prof. Sylvester's theory of the reduction of a binary quantic to its canonical form; and Nos. 86 to 91 contain the definitions of certain covariants or other derivatives connected with Bezout's abbreviated method of elimination, due for the most part to Professor Sylvester, and which are called Bezoutiants, Cobezoutiants, &c. I have not in the present memoir in any wise considered the theories to which the catalecticant &c. and the other covariants and derivatives just referred to relate; the design is to point out and precisely define the different covariants or other derivatives which have hitherto presented themselves in theories relating to binary quantics, and so to complete, as far as may be, the explanation of the terminology of this part of the subject.

III. "A Fifth Memoir upon Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received February 11, 1858.

(Abstract.)

The present memoir was originally intended to contain a development of the theories of the covariants of certain binary quantics, viz. the quadric, the cubic, and the quartic; but as regards the theories of the cubic and the quartic, it was found necessary to consider the case of two or more quadrics, and I have therefore comprised such systems of two or more quadrics, and the resulting theories of the harmonic relation and of involution, in the subject of the memoir; and although the theory of homography or of the anharmonic relation belongs rather to the subject of bipartite binary quadrics, yet from its connexion with the theories just referred to, it is also considered in the memoir. The paragraphs are numbered continuously with those of my former memoirs on the subject: Nos. 92 to 95 relate to a single quadric; Nos. 96 to 114 to two or more quadrics, and the theories above referred to; Nos. 115 to 127 to the cubic, and Nos. 128 to 145 to the quartic. The several quantics are considered as expressed not only in terms of the coefficients, but also in terms of the roots,—and I consider the question of the determination of their linear factors,—a question, in effect, identical with that of the solution of a quadric, cubic, or biquadratic equation. The expression for the linear factor of a quadric is deduced from a well-known formula; those for the linear factors of a cubic and a quartic were first given in my "Note sur les Covariants d'une fonction quadratique, cubique ou biquadratique à deux indéterminées," Crelle, vol. l. pp. 285 to 287, 1855. It is remarkable that they are in one point of view more simple than the expression for the linear factor of a quadric.

IV. "On the Tangential of a Cubic." By ARTHUR CAYLEY, Esq., F.R.S. Received February 11, 1858.

(Abstract.)

In my "Memoir on Curves of the Third Order," Phil. Trans. vol. cxlvii. (1857), I had occasion to consider a derivative which may be termed the "tangential" of a cubic, viz. the tangent at the point (x, y, z) of the cubic curve $(*\between x, y, z)^3=0$ meets the curve in a point (ξ, η, ζ), which is the tangential of the first-mentioned point; and I showed that when the cubic is represented in the canonical form $x^3+y^3+z^3+6lxyz=0$, the coordinates of the tangential may be taken to be $x(y^3-z^3) : y(z^3-x^3) : z(x^3-y^3)$. The method given for obtaining the tangential may be applied to the general form $(a, b, c, f, g, h, i, j, k, l\between x, y, z)^3$: it seems desirable, in reference to the theory of cubic forms, to give the expression of the tangential for the general form; and this is what I propose to do, merely indicating the steps of the calculation, which was performed for me by Mr. Creedy.

V. "On the Constitution of the Essential Oil of Rue." By C. GREVILLE WILLIAMS, Esq., Lecturer on Chemistry in the Normal College, Swansea. Communicated by Professor STOKES, Sec. R.S. Received February 15, 1858.

(Abstract.)

The essential oil of rue and its products of decomposition have been examined by several chemists. Will analysed it many years ago, and deduced the formula $C^{28} H^{28} O^3$ as the result of his analyses. The principal investigation of it was made by Gerhardt, who regarded it as the aldehyde of capric acid. The production of capric acid from it by the action of nitric acid, as observed by Gerhardt and also by Cahours, has been considered as corroborative of the 20 carbon formula. It is evident, however, that the formation of capric acid merely indicates the aldehyde to contain *not less* than 20 equivalents of carbon.

Some experiments made with a view to the production of certain new derivatives of capric aldehyde, led the author to believe the ideas generally entertained regarding the formula of the oil to be erroneous. Before continuing his experiments, he has therefore reinvestigated the nature of the oil itself.

In order to obtain the aldehyde in a state of purity, advantage was taken of the tendency of the aldehydes to combine with the alkaline bisulphites. The oil obtained from the ammoniacal bisulphite of the aldehyde was carefully analysed. The mean of eight very coincident analyses gave,—

Mean.			Calculation.	
Carbon......	77·71	C^{22}	132	77·65
Hydrogen ..	13·07	H^{22}	22	12·94
Oxygen	9·22	O^{2}	16	9·41
	100·00		170	100·00

The mean of two determinations of the density of the vapour* gave,—

Experiment (mean).	Theory $C^{22} H^{22} O^{2}$ = 4 vols.
5·870	5·874

The aldehyde, purified as above, was again converted into the ammoniacal bisulphite, from which the oil was a second time obtained. It gave on analysis,—

Carbon............	77·67
Hydrogen	12·93
Oxygen	9·40
	100·00

It is plain, therefore, that oil of rue contains an aldehyde of the formula $C^{22} H^{22} O^{2}$. Recent researches having demonstrated that no acid of the series $C^{n} H^{n} O^{4}$ with 22 equivalents of carbon has yet been isolated, and no other derivative with a 22 carbon formula being known, the author has given the name *enodyle* to the radical homologous with acetyle contained in this substance.

Enodic aldehyde is a colourless fluid of a fruity odour, quite different to that of the rue plant. Its density is 0·8497 at 15°. Agitation

* In order to prevent oxidation of the oil, the balloons were filled with hydrogen previous to immersion in the bath.

will cause it to solidify at 7° into a snow-white mass resembling camphor. Its boiling-point is 213°.

Rue oil yields a small portion of fluid boiling at 232°, containing the aldehyde of lauric acid. It was not obtained absolutely free from the first fluid. It contained:—

Experiment.		Calculation.		
Carbon	78·1	C^{24}	144	78·26
Hydrogen	12·9	H^{24}	24	13·04
Oxygen	9·0	O^2	16	8·70
	100·0		184	100·00

The oils accompanying the aldehydes, but which refuse to combine with the alkaline bisulphites, are of the terebinthinate class. The more volatile are composed chiefly of an isomer of oil of turpentine; the less volatile are hydrates apparently homologous with an isomer of borneol.

March 25, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "On the Relative Power of Metals and their Alloys to conduct Heat." By F. CRACE CALVERT, Esq., F.C.S., M.R. Acad. of Turin; and RICHARD JOHNSON, Esq., M. Phil. Soc. of Manchester. Communicated by Prof. STOKES, Sec. R.S. Received February 19, 1858.

(Abstract.)

After describing the apparatus employed, and the process followed to determine the conductibility of metals and alloys, the authors give the chemical means by which they purified the metals used in the experiments. Taking silver, which is the best conductor, as

1000, they have obtained the relative conducting powers of the following metals :—

Silver	1000	Forged iron	436
Gold, $\frac{1000}{1000}$	981	Tin	422
Gold, $\frac{991}{1000}$	840	Steel	397
Copper, rolled	845	Platinum	379
Copper, cast	811	Sodium	365
Mercury	677	Cast iron	359
Aluminium	665	Lead	287
Zinc, forged	641	Antimony, cast horizontally	215
Zinc, cast vertically	628	Antimony, cast vertically	192
Zinc, cast horizontally	608	Bismuth	61
Cadmium	578		

The precision obtained by this process is such, that the authors were able to determine the different conducting powers of the same metal, when rolled or cast, as shown above. They were also able to appreciate the influence of crystallization on conductibility, for they found that the conducting power of a metal was different when it was cast horizontally or vertically, from the different directions which the axes of crystallization took under these circumstances.

The importance of having the metals as pure as the resources of chemistry allow, is shown by the action which one per cent. of impurity exerts on the conductibility of a metal, in some cases reducing it one-fifth or one-fourth. Copper alloyed with one per cent. of various metals gave different conducting powers, in the same manner as Mr. Thomson has shown that the conduction of electricity by the same metal is affected by a similar amount of impurities.

Alloying a metal with a non-metallic substance also exerts an influence, as is shown in the case of the combination of iron with carbon, thus—

Forged iron	436
Steel	397
Cast iron	359

Similar results were obtained by combining small proportions of arsenic with copper.

The authors, with a view of ascertaining whether alloys are simple mixtures of metals, or definite compounds, made a large number of

alloys of various metals, using equivalent proportions, and determined their conducting powers. The general result obtained is, that alloys may be classed under the three following heads :—

1st. Alloys which conduct heat in ratio with the relative equivalents of the metals composing them.

2nd. Alloys in which there is an excess of equivalents of the worse conducting metal over the number of equivalents of the better conductor, such as alloys composed of 1Cu and 2Sn ; 1Cu and 3Sn ; 1Cu and 4Sn, &c., and which present the curious and unexpected result that they conduct heat as if they did not contain a particle of the better conductor ; the conducting power of such alloys being the same as if the square bar which was used in the experiments were entirely composed of the worse conducting metal.

3rd. Alloys composed of the same metals as the last class, but in which the number of equivalents of the better conducting metal is greater than the number of equivalents of the worse conductor ; for example, alloys composed of 1Sn 2Cu ; 1Sn 3Cu ; 1Sn 4Cu, &c. ; in this case each alloy has its own arbitrary conducting power, and the conductibility of such an alloy gradually increases and tends towards the conducting power of the better conductor of the two metals composing the alloy.

Experiments were also made with bars composed of various metals soldered together, in order to compare the results obtained with alloys with those afforded by the same metals when mixed.

The first part of the paper concludes with the conducting power of several commercial brass alloys.

The second part, which will shortly be published, will contain the conduction of heat by amalgams.

II. "On the Surface which is the Envelope of Planes through the Points of an Ellipsoid at right angles to the Radius Vectors from the Centre." By Arthur Cayley, Esq., F.R.S. Received February 22, 1858.

(Abstract.)

The consideration of the surface in question was suggested to me some years ago by Professor Stokes ; but it is proper to remark, that the curve which is the envelope of lines through the points of an

ellipse at right angles to the radius vectors through the centre occurs incidentally in Tortolini's memoir "Sulle relazione," &c., Tortolini, vol. vi. pp. 433 to 466 (1855), see p. 461, where the equation is found to be

$$\{4(a^4+b^4-a^2b^2)-3(a^2x^2+b^2y^2)\}^3$$
$$=\{9a^2(2b^2-a^2)x^2+9b^2(2a^2-b^2)y^2-4(a^2+b^2)(2a^2-b^2)(2b^2-a^2)\}^2,$$

an equation which is obtained by equating to zero the discriminant of a *quartic* function. Tortolini remarks that this equation was first obtained by him in 1846 in the 'Raccolta Scientifica di Roma,' and he notices that the curve is known under the name of Talbot's curve.

According to my method, the equation of the curve is obtained by equating to zero the discriminant of a cubic function, and the equation of the surface is obtained by equating to zero the discriminant of a quartic function.

The paper contains a preparatory discussion of the curve, and the surface is then discussed in a similar manner, viz. by means of the equations

$$x=X\left\{2-\frac{1}{a^2}(X^2+Y^2+Z^2)\right\},$$
$$y=Y\left\{2-\frac{1}{b^2}(X^2+Y^2+Z^2)\right\},$$
$$z=Z\left\{2-\frac{1}{c^2}(X^2+Y^2+Z^2)\right\},$$

which determine the coordinates x, y, z of a point on the surface in terms of X, Y, Z, the coordinates of a point on the ellipsoid. The surface, which is one of the tenth order, is found to have nodal conics in each of the principal planes, and also a cuspidal curve. The case more particularly considered is that for which $a^2>2b^2$, $b^2>2c^2$, and $a^2+c^2>3b^2$, and the memoir contains a figure showing the form of the surface for the case in question. The equation of the surface is obtained by the elimination of X, Y, Z between the above-mentioned equations and the equation $\frac{X^2}{a^2}+\frac{Y^2}{b^2}+\frac{Z^2}{c^2}=1$, as already remarked. This is reduced to the determination of the discriminant of a quartic function, and the equation of the surface is thus obtained under the form $I^3-27J^2=0$, where I and J are given functions of the coordinates.

III. "Some Remarks on the Physiological Action of the *Tanghinia venenifera*." By Professors A. Kölliker of Würzburg, and E. Pelikan of St. Petersburgh. Communicated by Sir B. C. Brodie, Bart. Received March 1, 1858.

The famous poison-tree of Madagascar was described for the first time by Aubert du Petit Thouars in his 'Genera Madagascarensia,' under the name of *Tanghinia venenifera*. At a later period, Sir W. Hooker published a good description, with a figure of this tree, named by him *Cerbera Tanghin* (see Botanical Magazine, pl. 2968), so that nothing is wanted with regard to the botanical knowledge of this plant. On the other hand, the physiological effects of its poisonous parts have not been hitherto investigated. All we know is, that the fruit of the Tanghinia is a strong poison, and is used in Madagascar as an ordeal poison in the most strange and revolting way. The only experiment on animals made by Ollivier, showed that 12 grains kill a dog in some hours, but this experiment gave no further insight into the real action of the Tanghinia. We hope, therefore, that the Royal Society will take some interest in the experiments which we undertook with this poison, of which the following is a short abstract.

The poison used by us was the alcoholic extract of the leaves and small stems of the Tanghinia, prepared from dried specimens, which Prof. Pelikan had received from Count Seyderitz of Mecklenburg. About one centigram of this extract was sufficient to show the full effect of the poison on frogs, when introduced into a wound of the back. It acted also when given by the mouth, but in this case a somewhat larger dose was required to produce a full effect.

The observed symptoms were the following :—

1. First of all, viz. in about 5 to 15 minutes, the *heart* was affected and stopped in its action, in such a way that the ventricle became contracted and very small, whilst the auricles remained dilated, but were also paralysed.

2. The *voluntary* and *reflex movements* were at first not at all affected ; but some time—from half an hour to one hour—after the paralysis of the heart, they became weaker and weaker, and gradually ceased totally without any sign of spasms or tetanus.

3. In the third place the Tanghinia has a great influence upon the *voluntary muscles*, which become paralysed. This action begins very soon, and we have been able to show, with the aid of the *myographion* of Volkman, that as soon as the heart is paralysed, the muscles also begin to lose their force. Nevertheless, the total paralysis of these organs is not observed till after six hours and more, that is to say, when the muscles have been preserved in a temperature of 14° to 16° R. In a temperature of 4° to 6° R., the irritability of the poisoned muscles may last for double this time, as is usual with all poisoned muscles and nerves; but even in this case it disappears long before that of the non-affected muscles.

4. If muscles which have lost their irritability through the Tanghinia are put into a solution of common salt of from $\frac{1}{2}$ to 1 per cent., their power of contraction reappears after a certain time, but only when they have been preserved at the lower temperature of 5° to 6° R.

5. Lastly, the *nerves* also are *paralysed* by the Tanghinia, and, as far as we were able to pursue this question, under the same circumstances as the muscles, only perhaps a little earlier.

From all this it follows that the Tanghinia is a *paralysing*, and above all, a *muscular poison*. As far as we have been able to follow its action, it resembles very much the Upas Antiar, only its power would seem to be a little less strong.

Professor Stokes drew the attention of the Meeting to some photographic specimens illustrative of the recent researches of M. Nièpce de Saint Victor on "a New Action of Light" (Comptes Rendus, March 1 and 8). They were presented to the Society by the author through Mr. Grove, F.R.S.

The Society adjourned over the Easter vacation to Thursday, April 15.

April 15, 1858.

The LORD WROTTESLEY, President, in the Chair.

Major-General Boileau was admitted a Fellow of the Society.

The following communications were read:—

I. "On Tangential Coordinates." By the Rev. JAMES BOOTH, LL.D., F.R.S. Received March 20, 1858.

Many years ago, after I had taken my degree, I was much interested in the study of the original memoirs on reciprocal curves and curved surfaces, published in the 'Annales Mathématiques' of Gergonne, and in the works of such accomplished geometers as Monge, Dupin, Poncelet, and Chasles. In the course of my own researches, it occurred to me that there ought to be some some way of expressing by common algebra the properties of such reciprocal curves and surfaces, some method which would, on inspection, show the relations existing between the original and derived surfaces. I was then led to the discovery of a simple method and compact notation from the following considerations. But before I state them, it is proper to mention that I published the discovery in a little tract which I printed at the time, of which the title was, 'On the Application of a New Analytic Method to the Theory of Curves and Curved Surfaces.' This little tract, which is now out of print, as only a few copies were printed, excited but little attention. Nor is this to be wondered at. Mathematical researches, and, indeed, I might add, scientific pursuits in general, command but small attention in this country, unless they promise to pay. The obscurity of the author, and the remoteness of a provincial press, still further account for the little notice it obtained. Besides, it must in fairness be added, that the materials were hastily and crudely thrown together; that to save space, the demonstrations were for the most part omitted, and that the principles on which the method rests were not so clearly explained as to enable an ordinary reader,—who had to incorporate with his own thinking the notions of another,—to pursue the train of argument, or

the successive steps of a proof with facility and conviction. This may to some extent also explain why the method has hitherto received so little countenance as not to be admitted into any elementary work on the application of the principles and notation of algebra to the investigation and discussion of the properties of space. But the addition of a new method of investigation to those already in use, the development of its principles, with illustrations of the mode of its application, are surely not of less value to a philosophical appreciation of what that is in which mathematical knowledge truly consists, than the giving of problems, which, while they embody no general principle, are yet often difficult to solve; and when solved, frequently afford no clue by which the solution may be rendered available in other cases.

The radical vice in mathematical instruction in this country and in our time would seem to be, that knowledge of principles and familiarity with methods of investigation are subordinated to nimble dexterity in the manipulation of symbols, and to cramming the memory with long formulæ and tabular expressions.

Again, it often happens that an investigation, which, if pursued by one method, would prove barren of results or altogether impracticable, when followed out from a different point of view and by the help of another method, not unfrequently leads by a few easy steps to the discovery of important truths, or to the consideration of others under a novel aspect. Hence the multiplication of methods of investigation tends widely to enlarge the boundaries of science.

My object in the following paper will be to show that problems of great difficulty, some of which have not hitherto been solved, while others by the ordinary methods admit only of complicated and tedious modes of proof, may by this method be treated with singular brevity and remarkable simplicity. I will first premise a few simple principles.

When two figures in the same plane, or more generally in space, are so related that one is the *reciprocal polar* of the other, then to every point in the one corresponds a plane in the other; to every right line in the one a right line also in the other; to any number of points in the same right line in the one, as many planes all intersecting in the same right line in the other; to any number of points in the same plane in the one, as many planes all meeting in the same point in the other. I might easily proceed to any length with this

enumeration of the reciprocal properties of curves and curved surfaces. Hence given a series of points, lines, and planes, we may construct a series of as many planes, lines, and points, according to a fixed and simple law.

Now we know that in the application of algebra to geometry by the method of coordinates, a point is determined in position by its projections on three coordinate planes, or by three equations, that is by three conditions. A right line may in like manner be determined when we are given the positions of two points in it; and a plane is determined by one condition, which is called its equation. But in the inverse method, a point should be determined by one condition, a right line by two, and a plane by three. Again, a right line may be determined by considering it as joining two fixed points, or as the common intersection of two fixed planes. Now all these conditions may be expressed by taking as a new system of coordinates the segments of the common axes of coordinates between the origin and the points in which they are met by a moveable plane. Thus if these segments be designated by the symbols X, Y, Z, the three equations which determine a plane are

$$X = \text{constant}, \quad Y = \text{constant}, \quad Z = \text{constant}.$$

Again, the equation in (x, y, z) of a plane passing through a point of which the coordinates are xyz, and which cuts off from the axes of coordinates the segments X, Y, Z, is $\frac{x}{X} + \frac{y}{Y} + \frac{z}{Z} = 1$. Now this is the *projective*, or common equation of the plane, if we make x, y, and z vary, and consider X, Y, Z as constant. But we may invert these conditions, and consider x, y, z constant, while X, Y, and Z vary. And the equation now, instead of being the common equation of a fixed plane, becomes the inverse or tangential equation of a fixed point. In this latter case let α, β, and γ be put for x, y, and z, and $\frac{1}{\xi}$, $\frac{1}{\upsilon}$, $\frac{1}{\zeta}$ for X, Y, Z; then the equation may be written

$$\alpha\xi + \beta\upsilon + \gamma\zeta = 1,$$

which may be called the tangential equation of a point.

Moreover, as the continuous motion of a point, in a plane suppose, subjected to move in accordance with certain fixed conditions expressed by a certain relation between x and y may be conceived to describe a curve, so the successive positions of a straight line cutting

off segments from the axes of coordinates, having a certain relation to each other, may be imagined to wrap round or envelope a certain curve, just as we may see a curve described on paper by the successive intersections of a series of straight lines. Hence there are two distinct modes according to which we may conceive all curves to be generated, namely by the motion of a tracing-point, or the successive intersections of straight lines; by a pencil or straight edge, as a joiner would say. These conceptions are the logical basis of the methods by which the principles and notation of common algebra are generalized from the discussion of the properties of abstract number to those of pure space. The former view gave rise to the method of *projective* coordinates, the latter suggests the method of *tangential* coordinates, a term which I was the first, I believe, to invent and apply.

It is sometimes very easy to express both the projection and tangential equations of the same curve or curved surface; it is frequently a matter of extreme difficulty.

Thus, if the projective equation of an ellipsoid be

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1,$$

its tangential equation will be

$$a^2\xi^2+b^2v^2+c^2\zeta^2=1,$$

a, b, c being, as in the preceding equation, the semiaxes.

Again, if we take the evolute of the ellipse whose equation is $y^{\frac{2}{3}}+nx^{\frac{2}{3}}=m^{\frac{2}{3}}$, the tangential equation of the same curve is

$$\xi^2+n^3v^2=m^2\xi^2v^2.$$

I shall not attempt to introduce into this abstract the formulæ for the transformation of coordinates, or the several elementary expressions which belong to this system, and which must be investigated and known before the method can be used as an instrument of investigation or analogy. My object is rather to give a specimen of the method in the solution of some very difficult problems, and to show how it may be made a powerful instrument of analytical investigation.

On the Surface of the Centres of Curvature of an Ellipsoid.

It is well known to geometers that the lines of greatest and least curvature at any point on the surface of an ellipsoid are at right angles to each other, and that they may be constructed by the inter-

sections of two confocal hyperbolas, a single-sheet one and a double-sheet one. It is also known that these three surfaces are reciprocally orthogonal, or that any two of them cut the third along its lines of curvature where the three intersect in a point. If we fix on the ellipsoid as the surface whose lines of curvature are in question, and normals be drawn to the surface of the ellipsoid along any given line of curvature, the radii of curvature will not only lie on these normals at the successive points, but they will all, taken indefinitely near to each other, constitute a developable surface, and the line of centres of curvature will constitute its edge of regression. Hence if we draw tangent planes to the two hyperboloids at this point, they will intersect in the normal to the ellipsoid, and will also be tangent planes to the above developable surface. Let the equation of the ellipsoid be

$$\frac{x'^2}{a^2}+\frac{y'^2}{b^2}+\frac{z'^2}{c^2}=1,$$

or as the surfaces are confocal, we may put $a^2-b^2=h^2$, $a^2-c^2=k^2$. Hence this equation may be written

$$\frac{x'^2}{a^2}+\frac{y'^2}{a^2-h^2}+\frac{z'^2}{a^2-k^2}=1. \quad . \quad . \quad . \quad . \quad (1)$$

Let α be the transverse axe of the hyperboloid passing through the point $x'y'z'$, and we shall have

$$\frac{x'^2}{\alpha^2}+\frac{y'^2}{\alpha^2-h^2}+\frac{z'^2}{\alpha^2-k^2}=1. \quad . \quad . \quad . \quad . \quad (2)$$

Now the tangential equation of this hyperboloid is

$$\alpha^2\xi^2+(\alpha^2-h^2)v^2+(\alpha^2-k^2)\zeta^2=1. \quad . \quad . \quad . \quad (3)$$

But the equation of the tangent plane to the hyperboloid at the point $(x'y'z')$ is

$$\frac{xx'}{\alpha^2}+\frac{yy'}{(\alpha^2-h^2)}+\frac{zz'}{\alpha^2-k^2}=1; \quad . \quad . \quad . \quad . \quad (4)$$

and as the planes which touch the ellipsoid and hyperboloid at the common point (x', y', z') are at right angles to each other, we have, moreover,

$$\frac{x'\xi}{a^2}+\frac{y'v}{a^2-h^2}+\frac{z'\zeta}{a^2-k^2}=0. \quad . \quad . \quad . \quad . \quad (5)$$

Hence eliminating x', y', z', and α, we shall have, finally,

$$(\xi^2+v^2+\zeta^2)^2=\left[\frac{\xi^2}{a^2}+\frac{v^2}{b^2}+\frac{z^2}{c^2}\right](a^2\xi^2+b^2v^2+c^2\zeta^2-1). \quad . \quad (6)$$

This is the tangential equation of the "*surface of centres of curvature*," or as it may for brevity be called, *the surface of centres.*

This surface in general consists of two sheets, one generated by one centre of curvature, the second sheet by the other centre. Let a perpendicular P on a tangent plane to the surface of centres make the angles λ, μ, ν with the axes of coordinates, then $P\xi=\cos\lambda$, $Pv=\cos\mu$, $P\zeta=\cos\nu$, and the last equation may be written

$$1=\left[\frac{\cos^2\lambda}{a^2}+\frac{\cos^2\mu}{b^2}+\frac{\cos^2\nu}{c^2}\right](a^2\cos^2\lambda+b^2\cos^2\mu+c^2\cos^2\nu-P^2). \quad (7)$$

Now the first member of this equation represents $\frac{1}{R^2}$, the inverse semidiameter squared of the original ellipsoid, making the angles λ, μ, ν with the axes, and $a^2\cos^2\lambda+b^2\cos^2\mu+c^2\cos^2\nu=P_1^2$ is the square of the perpendicular on a tangent plane to the ellipsoid parallel to the tangent plane to the surface of centres. Hence

$$P^2=P_1^2-R^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Whence we have this remarkable property of the surface of centres:—

Any two parallel tangent planes being drawn to the surface of centres and to the ellipsoid, the difference of the squares of the coincident perpendiculars let fall upon them from the centre is always equal to the square of the coinciding semidiameter of the ellipsoid.

We may reduce the original equation (6) to the form

$$\begin{aligned}b^2c^2\xi^2+a^2c^2v^2+a^2b^2\zeta^2=(b^2-c^2)^2a^2\zeta^2v^2+(a^2-b^2)^2c^2\xi^2v^2\\+(a^2-c^2)^2b^2\xi^2\zeta^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)\end{aligned}$$

By giving to ζ a set of constant values, we might determine the tangential equations of the sections made in the plane of xy by the cone whose vertex is in the axis of z, and which envelopes the surface of centres.

But it will be better to determine the sections of the surface made by the principal planes, and this may be effected by putting ζ, v, ξ successively equal to ∞ and 0. Hence we shall have in the planes of yz, xz, and xy, the sections whose tangential equations are

$$\left.\begin{aligned}&(a^2-b^2)^2c^2v^2+(a^2-c^2)^2b^2\zeta^2=b^2c^2\\&c^2v^2+b^2\zeta^2=(b^2-c^2)^2\zeta^2v^2\end{aligned}\right\}\text{ in the plane of } zy,$$

$$\left.\begin{aligned}&(b^2-c^2)^2a^2\zeta^2+(a^2-b^2)^2c^2\xi^2=a^2c^2\\&a^2\zeta^2+c^2\xi^2=(a^2-c^2)^2\xi^2\zeta^2\end{aligned}\right\}\text{ in the plane of } xz,$$

$$\left.\begin{aligned}&(a^2-c^2)^2b^2\xi^2+(b^2-c^2)^2a^2v^2=a^2b^2\\&b^2\xi^2+a^2v^2=(a^2-b^2)^2\xi^2v^2\end{aligned}\right\}\text{ in the plane of } xy.$$

Hence the sections of the surface of centres in the principal planes are two in each; one an ellipse, the other the evolute of an ellipse.

On the umbilical lines of Curvature.

Among the French mathematicians there has been much difference of opinion as to the nature of the lines of curvature which pass through the *umbilicus* of the ellipsoid. Some hold with Monge and Dupin, that the two lines of curvature which everywhere else on the surface are at right angles to each other, here merge into one. This is such a violation of the law of continuity, that others adhere to the opinion of Poisson and Leroy, to the effect that at the umbilicus the radii of curvature are all equal, and that there is an infinite number of rectangular systems of lines of equal curvature all passing through the umbilicus.

An examination of the surface of centres will demonstratively show that the latter opinion is the correct one.

For this purpose let a tangent plane to the surface of centres be drawn through the umbilical normal. Now the *projective* coordinates of the umbilicus are

$$x'=a\sqrt{\frac{a^2-b^2}{a^2-c^2}}\,y'=0, \quad z'=c\sqrt{\frac{b^2-c^2}{a^2-c^2}}; \quad . \quad . \quad (10)$$

and the segments of the axes of x and z cut off by the normal are

$$\bar{z}=\frac{\sqrt{(b^2-c^2)(a^2-c^2)}}{c}, \quad \bar{x}=\frac{\sqrt{(a^2-c^2)(a^2-b^2)}}{a}. \quad (10^*)$$

Hence the tangential equations of the normal in the plane of xz are

$$\zeta^2=\frac{c^2}{(a^2-c^2)(b^2-c^2)}, \quad \xi^2=\frac{a^2}{(a^2-c^2)(a^2-b^2)}. \quad . \quad . \quad (11)$$

Now substituting these values of ζ and ξ in the equation (9) of the surface of centres, we shall have for the value of v^2 the following expression:—

$$\left[(a^2-b^2)+(b^2-c^2)+(c^2-a^2)\right]v^2=$$

$$a^2b^2c^2\left[\frac{(a^2-b^2)+(b^2-c^2)+(c^2-a^2)}{(a^2-b^2)(a^2-c^2)(b^2-c^2)}\right], \text{ or } v=\frac{0}{0}. \quad . \quad (12)$$

Hence an infinite number of tangent planes may be drawn through the umbilical normal to the surface of centres.

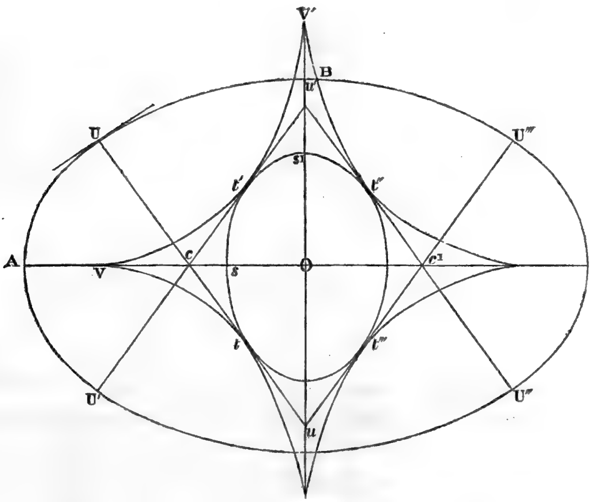
The principal sections of the surface of centres in the mean plane, or in the plane of xz, the plane of the greatest and least axe, possess some very curious properties.

The tangential equations of these sections are

$$\left.\begin{array}{l} c^2\xi^2+a^2\zeta^2=(a^2-c^2)^2\xi^2\zeta^2 \\ (a^2-b^2)^2c^2\xi^2+(b^2-c^2)^2a^2\zeta^2=a^2c^2 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Now the former of these is the tangential equation of the evolute of an ellipse, while the other is that of an ellipse whose semiaxes are the radii of curvature at the extremities of a and c in the planes of xy and zy diminished by a and c.

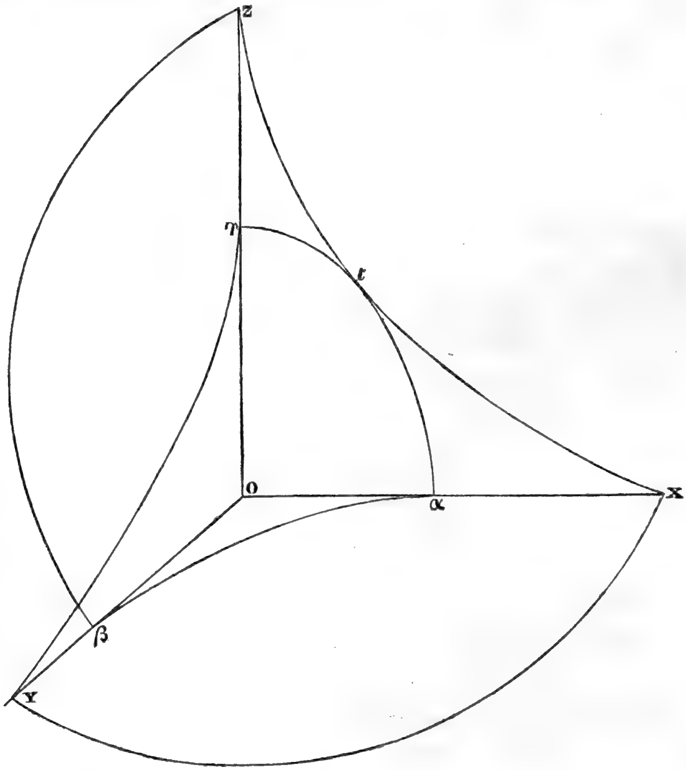
It is easy to show, that if through the four umbilici of the ellipsoid normals to the surface be drawn, they will lie in the plane of xz, they will touch the evolute *internally* and the ellipse *externally* in the same points, so that the lozenge formed by the four normals will be *inscribed* in the evolute and *circumscribed* to the ellipse, and the distance of the point of contact to the umbilicus will be equal to $\frac{b^3}{ac}$.



The respective areas of the lozenge, of the *inscribed* ellipse, and of the *circumscribed* evolute, are connected by relations independent of the axes of the ellipsoid.

It is in these four points, and in these four points only, that the two sheets of the surface of centres touch each other. We should find on investigation, that the points of intersection of the sections of the

surface of curvatures in the other principal planes are the one set real, while the other are imaginary, as in the subjoined figure.



It may easily be shown, as in the preceding figure, that in the principal planes of the surface of the centres of curvature, the vertices of the diameters of the evolute and ellipse are the vertices of the ellipse and evolute in the adjoining plane. Thus the semiaxes OX, $O\alpha$ of the evolute and ellipse in the plane of XZ are the semiaxes of the ellipse and evolute in the plane of XY.

There are many other curious properties of this surface which will be developed in the memoir.

Before passing from this surface, I would mention that the fundamental property of the surface of centres suggests a simple property of the evolute of an ellipse.

Parallel tangents being drawn to an ellipse and its evolute, and perpendiculars from the centre let fall upon them, the difference of the squares of these perpendiculars is equal to the square of the semi-diameter of the ellipse which coincides with the perpendiculars.

I will proceed with a few other applications of the method. For example,—

A surface of the second order touches seven given planes, to find the locus of its centre.

Let the tangential equation of the given surface be

$$\alpha\xi^2+\alpha_{,}\upsilon^2+\alpha_{,,}\zeta^2+2\beta\upsilon\zeta+2\beta_{,}\xi\zeta+2\beta_{,,}\xi\zeta+2\gamma\xi+2\gamma_{,}\upsilon+2\gamma_{,,}\zeta=1,$$

and let the twenty-one coordinates of the seven given planes be ξ', υ', ζ'; $\zeta'', \upsilon'', \zeta''$; $\xi''', \upsilon''', \zeta'''$, &c. Substituting these values successively in the preceding equation, we shall have seven linear equations by which we may eliminate the six quantities $\alpha, \alpha', \alpha''$; β, β', β''. The resulting equation will also be linear, and of the form

$$L\gamma+M\gamma_{,}+N\gamma_{,,}+1=0,$$

which is the equation of a plane. Now γ, $\gamma_{,}$, and $\gamma_{,,}$, as may be shown, are the projective coordinates of the centre of the surface. Hence the centre of the surface moves along a plane. When there are eight planes, we may then eliminate γ or $\gamma_{,}$, and the two resulting equations will become

$$L\gamma+M\gamma_{,}-1=0, \quad L'\gamma+N\gamma_{,,}-1=0,$$

or the centre will move along a right line.

Again, perpendiculars are let fall from n points on a plane, the sum of the squares of which is constant, the plane will envelope an ellipsoid.

Should the sums of the squares be varied, the successive surfaces will all be confocal ellipsoids.

To show that if two surfaces of the second order are enveloped by a cone, they may also be enveloped by a second cone.

Let the vertex of the cone be taken as the origin of coordinates, and let their tangential equations be

$$\alpha\xi^2+\alpha_{,}\upsilon^2+\alpha_{,,}\zeta^2+2\beta\upsilon\zeta+2\beta_{,}\xi\zeta+2\beta_{,,}\xi\upsilon+2\gamma\xi+2\gamma_{,}\upsilon+2\gamma_{,,}\zeta=1,$$
$$a\xi^2+a_{,}\upsilon^2+a_{,,}\zeta^2+2b\upsilon\zeta+2b_{,}\xi\zeta+2b_{,,}\xi\upsilon+2c\xi+2c_{,}\upsilon+2c_{,,}\zeta=1;$$

and as the common tangent planes must pass through the origin,

$\xi v \zeta$ are the same in the equations of the two surfaces; but at the origin $\frac{1}{\xi}=0$, $\frac{1}{v}=0$, $\frac{1}{\zeta}=0$. At this point let $\xi=\phi\zeta$, $v=\psi\zeta$. Substituting these values in the preceding equations and dividing by $\zeta=\infty$,

$$\left.\begin{array}{l}\alpha\phi^2+\alpha_{,}\psi^2+\alpha_{,,}+2\beta\psi+2\beta_{,}\phi+\beta_{,,}\phi\psi=0\\ a\phi^2+a_{,}\psi^2+a_{,,}+2b\psi+2b_{,}\phi+2b_{,,}\phi\psi=0\end{array}\right\};$$

and as these equations represent the same tangent plane, they must be identical. Hence we shall have, introducing an equalizing factor λ,

$$a=\lambda\alpha,\quad a_{,}=\lambda\alpha_{,},\quad a_{,,}=\lambda\alpha_{,,},\quad b=\lambda\beta,\quad b_{,}=\lambda\beta_{,},\quad b_{,,}=\lambda\beta_{,,}.$$

Making these substitutions in the preceding equations, they become

$$\alpha\xi^2+\alpha_{,}v^2+\alpha_{,,}\zeta^2+2\beta\zeta v+2\beta_{,}\xi\zeta+2\beta_{,,}\xi v+2\gamma\xi+2\gamma_{,}v+2\gamma_{,,}\zeta=1,$$

$$\lambda\alpha\xi^2+\lambda\alpha_{,}v^2+\lambda\alpha_{,,}\zeta^2+2\lambda\beta\zeta v+2\lambda\beta_{,}\xi\zeta+2\lambda\beta_{,,}\xi v+2c\xi+2c_{,}v+2c_{,,}\xi=1.$$

Multiplying the former equation by λ, and subtracting from it the latter, we get

$$(\lambda\gamma-c)\xi+(\lambda\gamma_{,}-c_{,})v+(\lambda\gamma_{,,}-c_{,,})\zeta=\lambda-1,$$

the tangential equation of a point which is the vertex of the second enveloping cone.

Now the *projective* coordinates or the xyz of this point are

$$x=\frac{\lambda\gamma-c}{\lambda-1},\quad y=\frac{\lambda\gamma_{,}-c_{,}}{\lambda-1},\quad z=\frac{\lambda\gamma_{,,}-c_{,,}}{\lambda-1}.$$

Again: as at the beginning of this abstract we assumed the well-known property that three confocal surfaces of the second order which meet in a point intersect each other at right angles, so if a tangent plane be drawn to three concyclic surfaces, the three points of contact, two by two, will subtend right angles at the centre. The proof of this is very simple. Let the tangential equations of two concyclic surfaces be

$$a^2\xi^2+b^2v^2+c^2\zeta^2=1,\quad a_{,}^2\xi^2+b_{,}^2v^2+c_{,}^2\zeta^2=1.$$

Subtract these equations one from the other, and we shall have

$$(a^2-a_{,}^2)\xi^2+(b^2-b_{,}^2)v^2+(c^2-c_{,}^0)\zeta^2=0.$$

And as these surfaces are concyclic,

$$\frac{1}{a_{,}^2}-\frac{1}{a^2}=\frac{1}{k^2},\quad \frac{1}{b_{,}^2}-\frac{1}{b^2}=\frac{1}{k^2},\quad \frac{1}{c_{,}^2}-\frac{1}{c^2}=\frac{1}{k^2}.$$

Making these substitutions,

$$a^2 a_{\prime}^{2} \xi^2 + b^2 b_{\prime}^{2} v^2 + c^2 c_{\prime}^{2} \zeta^2 = 0\,;$$

and if λ, μ, ν are the angles which a semidiameter r through one of the points of contact makes with the axes,

$$\cos\lambda = \frac{x}{r} = \frac{a^2\xi}{r}, \quad \cos\mu = \frac{b^2 v}{r}, \quad \cos\nu = \frac{c^2\zeta}{r}.$$

Similarly, for another point of contact and semidiameter r', we have

$$\cos\lambda' = \frac{a_{\prime}^{2}\xi}{r'}, \quad \cos\mu' = \frac{b_{\prime}^{2} v}{r'}, \quad \cos\nu' = \frac{c_{\prime}^{2}\zeta}{r'}\,;$$

whence

$$\cos\lambda\cos\lambda' + \cos\mu\cos\mu' + \cos\nu\cos\nu' = 0,$$

or r and $r_{\prime}$ are at right angles.

But facility of proof is not the sole advantage of this method. It enables us to bring prominently into view that great principle of *duality* which is involved in all our geometrical investigations. This principle may be familiarly stated in the form, that every geometrical theorem or mathematical truth has its double. As an illustration of this, let us take the tangential equation of the surface of curvature,

$$(\xi^2 + v^2 + \zeta^2)^2 = \left[\frac{\xi^2}{a^2} + \frac{v^2}{b^2} + \frac{\zeta^2}{c^2}\right](a^2\xi^2 + b^2 v^2 + c^2\zeta^2 - 1),$$

and instead of ξ, v, ζ, write down x, y, z, introducing the constant r to render the equation homogeneous, and it becomes

$$(x^2 + y^2 + z^2)^2 = \left[\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2}\right](a^2x^2 + b^2y^2 + c^2z - r^4). \quad . \quad (14)$$

Now this surface has properties which are one by one reciprocal to those of the "surface of centres." As, for example, in each of the principal planes, the sections of the surface are ellipses and curves whose equations are of the form $A^2x^2 + B^2y^2 = x^2y^2$.

In the mean section the ellipse will touch this curve in four points, through which four lines being drawn parallel to the axis of y, they will lie wholly on the surface.

The formulæ which exhibit the relations between the *projective* and *tangential* coordinates of the same curve or curved surface are simple and symmetrical. They are given here without demonstration.

Let $\Phi = \phi(\xi, v, \zeta) = 0$ be the tangential equation of a curved sur-

face, and let x, y, z be the projective coordinates of the point of contact of the tangent plane; then

$$\left.\begin{aligned}
x&=\frac{\dfrac{d\Phi}{d\xi}}{\dfrac{d\Phi}{d\xi}\xi+\dfrac{d\Phi}{dv}v+\dfrac{d\Phi}{d\zeta}\zeta}\\
y&=\frac{\dfrac{d\Phi}{dv}}{\dfrac{d\Phi}{d\xi}\xi+\dfrac{d\Phi}{dv}v+\dfrac{d\Phi}{d\zeta}\zeta}\\
z&=\frac{\dfrac{d\Phi}{d\zeta}}{\dfrac{d\Phi}{d\xi}\xi+\dfrac{d\Phi}{dv}v+\dfrac{d\Phi}{d\zeta}\zeta}
\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(15)$$

By the help of these three equations and the original equation $\Phi=\phi(\xi, v, \zeta)=0$, we may eliminate ξ, v, ζ, and obtain the final equation in x, y, z.

Again, let $F=f(x, y, z)=0$ be the *projective* equation of a curved surface. The tangential coordinates ξ, v, ζ of the tangent plane drawn through the point (xyz) may be found from the following expressions:—

$$\left.\begin{aligned}
\xi&=\frac{\dfrac{dF}{dx}}{\dfrac{dF}{dx}x+\dfrac{dF}{dy}y+\dfrac{dF}{dz}z}\\
v&=\frac{\dfrac{dF}{dy}}{\dfrac{dF}{dx}x+\dfrac{dF}{dy}y+\dfrac{dF}{dz}z}\\
\zeta&=\frac{\dfrac{dF}{dz}}{\dfrac{dF}{dx}x+\dfrac{dF}{dy}y+\dfrac{dF}{dz}z}
\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(16)$$

As an application of this method, let it be required to find the expressions for the projective coordinates of the surface of the centres of curvature.

If we apply the general expressions (15) to the particular equation (9), we shall have

$$
\left.\begin{aligned}
x &= \frac{\xi\left[\left(\frac{a}{b}-\frac{b}{a}\right)^2 v^2+\left(\frac{a}{c}-\frac{c}{a}\right)^2\zeta^2-\frac{1}{a^2}\right]}{\frac{\xi^2}{a^2}+\frac{v^2}{b^2}+\frac{\zeta^2}{c^2}} \\
y &= \frac{v\left[\left(\frac{b}{c}-\frac{c}{b}\right)^2 \xi^2+\left(\frac{b}{a}-\frac{a}{b}\right)^2\zeta^2-\frac{1}{b^2}\right]}{\frac{\xi^2}{a^2}+\frac{v^2}{b^2}+\frac{\zeta^2}{c^2}} \\
z &= \frac{\zeta\left[\left(\frac{c}{a}-\frac{a}{c}\right)^2 v^2+\left(\frac{c}{b}-\frac{b}{c}\right)^2\xi^2-\frac{1}{c^2}\right]}{\frac{\xi^2}{a^2}+\frac{v^2}{b^2}+\frac{\zeta^2}{c^2}}
\end{aligned}\right\} \quad . \; . \; . \quad (17)
$$

The foregoing propositions will give some idea of the fertility of this method, and of the ease and simplicity of its application. I propose to develope it systematically in the memoir, of which this is merely a specimen. Many other systems of coordinates may be imagined, such as the parallel system of Chasles, or the curvilinear ordinates of Lame; but it may be questioned whether there is any system so directly reciprocal to the Cartesian method as this of Tangential Coordinates.

Note. Since the above abstract was written, my attention has been drawn to the results of an elaborate investigation of the *projective* equation of the surface of the centres of curvature, by the Rev. G. Salmon, Fellow of Trinity College, Dublin, and published in the Quarterly Journal of Pure and Applied Mathematics of Feb. 1858.

Although this surface has been familiarly known to the continental mathematicians since the time of Monge, none of them have ventured to grapple with the enormous difficulties which stand in the way of exhibiting its *projective* equation, or its equation in xyz. These difficulties have been surmounted by Mr. Salmon; and the resulting equation, which is of the twelfth degree, contains no fewer than eighty-three terms.

II. Extract of a Letter to Admiral FitzRoy, F.R.S., from Captain Pullen of H.M.S. 'Cyclops,' dated Aden, March 16, 1858. Communicated by Admiral FitzRoy. Received April 15, 1858.

My first sounding for temperature at any depth, was in 32° 13′ N., long. 19° 5′ W., where at 400 fathoms the minimum temperature was 51°·5, the surface at the time being 70°. The water brought up in the bottle was of greater density than we have since found it, namely, 1031 at temp. of 70°, whilst at the surface it was 1026. Supposing that you will see all the registered depths, &c. sent to Captain Washington, I do not enter into full detail here. The next time, I sent two thermometers down at 500 and 800 fathoms; at the greater depth, 44°·5; at the lesser, 50° was the minimum temperature. But now I began to observe some alterations in the indexes of the instruments, that of the maximum column not returning to the surface in the same position in which it stood on starting, viz. close to the mercury (brought to the surface temperature by being kept sufficiently long in the water alongside, and then compared with the deck-thermometer in constant use for that observation). Now I know from former experience that these indexes will shift by shaking the instrument, and with much less force than is frequently communicated to it by a shake of the line, on its passing up and down. From this we may infer that the minimum index also moves, how much it is impossible to say. And on looking over the results obtained during the voyage, I find that but few of the maximum indexes have come in standing at the point they started with. I therefore draw your attention to the fact, that such remedy may be applied as will obviate the defect. I have found another fault in thermometers before now (but then it was at a *low* temperature, and not Six's self-registering instrument that was employed). During my second winter at Fort Simpson, I, every three hours throughout twelve of the twenty-four, registered twenty-one thermometers, eighteen of Adie's and three of Negretti's, on glass scales. I never found Adie's at any temperature to differ more than a degree and a half from each other. Negretti's, when they ranged low, say twenty below zero, I have found twenty, eighteen, and

twelve degrees lower than Adie's. Here, in a high temperature (eighty degrees), I find three differing from the deck-thermometer, as well as from the other three (having only six remaining), being six, seven, and ten degrees higher. The correction (but a few tenths of a degree) can be allowed for certainly, but this difference may not be the same at a lower temperature, therefore it occasions a serious drawback to the efficiency of these instruments; and I always feel doubtful about the results. As yet I have only used those that agree nearest with each other.

My next sounding was in 10° 7′ N., long. 27° 32′ W., the position of the Hannah Shoal, no bottom with 2000 fathoms of line. There is 15 marked in some charts on this shoal. In 4° 16′ N. and 28° 42′ W., two thermometers were sent down to 1500 and 1000 fathoms, the greater depth showing a minimum 39°·4, the lesser 42°·5. No specimen of bottom had yet been brought up, as all the soundings hitherto, except those for the Devil's Rock in the Bay of Biscay, as well as the determinations of temperature, had been taken from a boat with small lines; so in the next cast I sounded from the ship with a large line,—the regular deep-sea line—and combined the experiments. Two thermometers were sent down on the line; and the sinker was down, by intervals, at about 1080 fathoms. The valve brought in a plentiful supply of bottom, and the thermometers showed a minimum temperature of 38°·5 at the *lowest* depth, and 46°·2 at 680 fathoms. This was in latitude 2° 20′ N., longitude 28° 44′ W., 90 miles from Saint Paul's Island. The specimen of bottom was a fine light greyish sand.

Drawing now to the Equator, I determined, if possible, to get a cast directly on it, and also the temperatures; accordingly the boat took the cast for bottom, while from the ship an endeavour was again made to combine the experiments. It failed, however, I am sorry to say, resulting in the loss of a large portion of the line, and two instruments sent down with it. From this I felt fully convinced that the uncertainty of concluding when the weight is down, from the intervals, is such, that the sounding becomes of little value, as far as the true depth is concerned.

[An extract of Capt. Pullen's Journal is here given,—showing the uncertainty of judging by the 'intervals' as to the time of reaching the bottom, and the consequent liability to pay out too much

line; also the increased strain then occasioned by the friction of the under current on the over-long line, and the great risk of the latter giving way on being pulled in; and stating the conclusion of the writer, that, in order to arrive at true results, soundings for bottom and temperature must not be combined.]

I have before this noticed how irregular the intervals always were when getting temperatures, particularly when more than one thermometer was on the line, but had never thoroughly considered the cause, nor the results likely to follow from the increased weight on the line; for although the addition of the thermometer (weighing about six pounds in the water-bottle) gives rapidity to the descent of the weight, its bulk offers great resistance on coming in, consequently the line has more tendency to break. And when it is intended to send down more than one thermometer, the line must be stopped to attach the addition, which at once checks the rapidity of descent, and the line has then to be let off the reel with more force, impossible to apply equably; and the intervals become so irregular, that all certainty of when the sinker is at the bottom is lost, and you feel at a loss when to stop.

From this experience I think I may say we have profited, for not a single fathom of line has been lost since, although going double the depth; once, too, with a fresh breeze with 2380 fathoms of line out; and it was with the greatest difficulty we could get the deck engine to reel it in again, and then only by putting all the Watch on to assist with their weight. I now began to observe more regularly for the temperatures, and with a stouter line than that usual for sounding, kept exclusively for the purpose. After crossing the Equator, I sent the thermometers down at nearly every tenth parallel, three at a time, at twelve, eight, and four hundred fathoms, reserving portions of the water brought up to send home for analysis.

In latitude 26° 46′ S. and longitude 23° 52′ W., nearly midway between where Sir James Ross has sounded without getting bottom, I got 2700 fathoms. A single thermometer was sent down to this 2700 fathoms depth, secured just above the sinker, and came in showing a minimum temperature of 35° F.; and the bottom brought up in the valve was a very fine brown-coloured sand.

In this case the common deep-sea line was used, and weighted with 120lbs. sinker (Brook's detaching), just one-fifth of its break-

ing strain; and the rapidity of descent has hardly if ever been equalled in speed by smaller lines when weighted nearly up to their breaking strain, as shown in the American soundings. One hour from the time we let go, the intervals showed that the weight was down.

I ran the easting down between the parallels of 35° and 38° S., from the Cape of Good Hope eastward outside Mauritius in the Indian Ocean, in the route of many doubtful dangers, and on nearing them the lead was brought into play. The first was the 'Brunswick,' on which is marked 85 fathoms, deep enough certainly for any ship that swims; but to clear up all doubt, two casts were obtained not far from its position, of 1410 and 1102 fathoms, without reaching bottom. Then comes the 'Atalanta,' having three positions, one on our Admiralty Charts, and two from Horsburgh, giving it as an extensive shoal under water, with pointed rocks on its western part. I passed from the westward between the northern position, and the first in order to the southward, and got four deep casts, besides several of 50 and 80 fathoms. The first was 1110 fathoms down by intervals, but no valve coming in, it appearing to have been broken off from the rod by striking some hard substance, either at the bottom or on its passage up or down, I immediately determined on getting another cast, although darkness was coming on. The weather fortunately was calm with very little sea on; so stationing a number of lanterns, the lead was at once dropped over the bows, and as satisfactory a cast was got as could possibly be wished, the sinker striking bottom by intervals at 1120 fathoms, thus proving the correctness of the first sounding, and finally, the valve coming in with a sufficient portion of bottom to prove it again. This specimen consisted of what appeared to the eye very fine sand covering a hard substance (coral I suspect), but under the microscope it was found to be some of the most beautiful specimens of Diatomaceæ that can be imagined. I send home these specimens, small as the quantity is, being quite sufficient for examination. The next morning further to N.E. with 800 fathoms, no bottom: another cast still further N.E., bottom with 900 fathoms, bringing up another specimen of the same sort of sand as last night, with a small pebble amongst it.

[Capt. Pullen here explains how his attention was drawn to parts of the sea where the surface was covered, for a considerable space, with

a white milky substance, apparently of animal nature, in large patches, with strips of deep blue water between; producing an effect which, viewed from a distance with a glass, had very much the appearance of breakers; and he suggests that some of the reports of shoals and breakers between the parallels of 35° and 40° may have no better foundation than the phenomenon in question.]

Steering now to pass to the east of Mauritius, a little south of the parallel of 20°, about 90 miles from the land I got no bottom with 1375 fathoms of line, which gave me the first idea, that what I had before thought of the Indian Ocean not being so deep as the Atlantic was wrong. Proceeding northward, I passed west of Cargados Garazos, Saya de Malha, east of Seychelles, and crossed the Equator in 58° 20′ E.; getting a cast 9 miles south of it with 2380 fathoms, no bottom. This is the sounding alluded to in a former part of this letter.

Forty or fifty miles west of the northern part of Cargados, 1400 fathoms of line reached the bottom. In 14° 41′ S., 58° 43′ E., no bottom with 1570 fathoms; 10° 30′ S., and 58° 52′ E., no bottom with 1320 fathoms. At this cast I sent down three thermometers at the 1320, 880, and 440; the minimum at the greatest depth 51°·5, at the centre depth 41°·5, and at the least depth 51°·5. The maximum tell-tales at the two least depths came in all right, but that of the greatest depth had fallen 6°; and its minimum showing an increasing temperature after passing through the colder stratum, is quite proof enough of its tell-tale falling down too, at least down the column instead of remaining up at what it once must have been in passing through that stratum, which the tell-tale of the thermometer at 880 fathoms shows the temperature of.

Winds now light and northerly. I got close to the doubtful George Island, and three quarters of a mile west of its southern point, bottom was not reached with 2000 fathoms of line. I then passed over it nearly a mile within its southern point, and having no signs of being on shore, I conclude that no such island ever existed in the position given it on our charts; and I find no account of it in Horsburgh.

Steaming now for Rose Galley Rocks, five miles south of the most western of them, I got bottom with 2254 fathoms of line, and brought up a plentiful supply of bottom, as well as the minimum

temperature 35°. A thermometer was sent down with the weight yesterday at 2000 fathoms, and returned with a minimum temperature of 38°·5. Now 35° was the minimum temperature at 2700 fathoms in the Atlantic, further south than this cast, which was near Rose Galley Rocks. I am therefore inclined to think that this is about the minimum temperature of the great depths of the Ocean, and that it commences soon after passing 2000 fathoms.

III. "On The Stereomonoscope, a new Instrument by which an *apparently* Single Picture produces the Stereoscopic Illusion." By A. Claudet, Esq., F.R.S. Received March 10, 1858.

In a former paper "On the Phenomenon of Relief of the Image formed on the ground glass of the Camera Obscura," which I communicated to the Royal Society on the 8th of May 1856, after having investigated the cause of that extraordinary fact and tried to explain it, I found that the images produced separately by the various points of the whole aperture of an object-glass are visible only when the refracted rays are falling on the ground glass in a line nearly coinciding with the optic axes; so that when both eyes are equally distant from the centre of the ground glass, each eye perceives only the image refracted in an oblique direction on that surface from the opposite side of the object-glass. Consequently each side of an object-glass, in proportion to its aperture, giving a different perspective of a solid placed before it, the result is an illusion of relief as conspicuous as when looking naturally at the objects themselves.

From the consideration of these singular facts, unnoticed before, I was led to think that it would be possible to construct a new Stereoscope, in which looking with both eyes at once on a ground glass at the point of coalescence of the two images of a stereoscopic slide, each refracted by a separate lens, we could see it on that surface in the same relief which is produced by the common stereoscope.

This instrument, as may be perceived at once, is nothing more than an ordinary camera obscura supplied with two lenses, each mounted on a sliding frame in order to be able to give them, according to the focal distance, the horizontal separation necessary for pro-

ducing on the ground glass the coalescence of the images of the two sides of a slide placed before the camera.

The slide itself being cut in two parts, the two images can also, moving in a groove, be separated in a horizontal direction, until they are sufficiently apart to be refracted on the ground glass by the two lenses in the most oblique direction consistent with the production of a well-defined image; for it is to the increased degree of obliquity of the refracted rays in falling on the ground glass that is due the more effective extinction or evanescence of the image for the eye whose axis consequently deviates in a greater degree from the line of refraction.

By the same principles which produce the phenomenon of relief of the image formed on the ground glass of the camera obscura, the right picture of the slide, being obliquely refracted on the ground glass by the right lens in a line coinciding with the axis of the left eye, is visible only to that eye; and the left picture, being refracted obliquely by the left lens in an opposite direction coinciding with the right eye, is only visible to that eye. Consequently each eye seeing only one image, and that image having its own perspective, the optic axes have to converge more or less according to the angular separation of the similar points of the two coincident images; and by the different degrees of convergence producing single vision of these various similar points, we have the sensation of the comparative distances of the objects represented on the ground glass.

Before having constructed this new stereoscope and tried its effect, it would have been hasty on my part to pretend that its success was certain, and for this reason I took care in my former paper to propose it as a mere speculative idea suggested by the phenomenon I had discovered, without vouching for the result. Indeed it was not long before I had to congratulate myself on my caution, when I found that, the truth of my experiments being questioned and the deductions from these experiments denied, my proposed stereoscope was declared impossible, as being founded on principles completely at variance with the laws of optics.

However, these remarks did not shake my conviction, and after the usual difficulties, I have now the satisfaction of being able to prove that I was perfectly right, and that I had not been led astray by any erroneous notion in my analytic and synthetic deductions.

I have constructed the instrument which I propose to call the *Stereomonoscope*, as it exhibits in perfect relief a picture which *appears single* on the ground glass of the new instrument, and *as single* as the image of the camera obscura has always been supposed to be.

The instrument, in its present rough state, is undoubtedly very imperfect and susceptible of many improvements which time and experience will suggest. I present it as the result of a first attempt, hoping that it will be found curious as illustrating a new and interesting scientific fact and producing an effect quite unexpected in optics.

April 22, 1858.

Major-General SABINE, Treasurer and V.P., in the Chair.

Professor Julius Plücker, Foreign Member, was admitted into the Society.

The following communications were read :—

I. "On the Differential Stethophone, and some new Phenomena observed by it." By S. Scott Alison, M.D., Assistant Physician to the Hospital for Consumption. Communicated by Prof. Tyndall, F.R.S. Received March 22, 1858.

Engaged for some years in investigations into the phenomena of audition, I have become cognizant of some facts which I believe have hitherto remained unnoticed, and which are certainly not generally known to physicists and physiologists.

The first of which I shall treat is the restriction of hearing external sounds of the same character to one ear, when the intensity is moderately, yet decidedly greater in one ear than in the other, the hearing being limited to that ear into which the sound is poured in greater intensity. The sound is heard alternately in one ear and in the other, as it is conveyed in increasing degrees of intensity, and hearing is suspended alternately in one ear and in the other, as the sound is conveyed in lessening degrees of intensity.

Sound, as is well known, if applied to both ears in equal intensity,

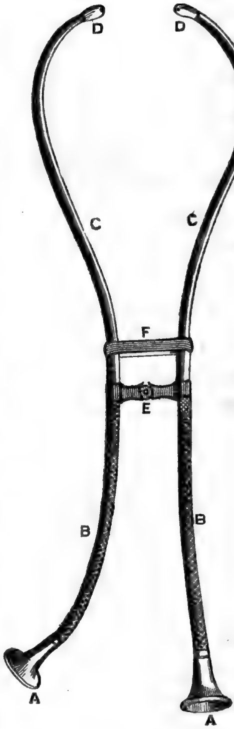
is heard in both ears; but it will be found, if the intensity in respect to one ear be moderately yet decidedly increased, by bringing the sounding body nearer that ear than the other, or otherwise, as by the employment, in respect to one ear, of a damper or obstructor of sound, or in respect of the other ear, by the employment of some intensifier, or good collector or conductor of sound, the sound is heard in that ear only which is favoured and has the advantage of greater intensity.

There is little doubt that this law holds with regard to sounds passing through the air, and carried to the ear in the ordinary manner, without the aid of any mechanical contrivance, as for instance those of a watch placed in front of the face; but as the restriction of hearing to one ear, and its suppression in the other, admit of being rendered more obvious by an apparatus that shall collect sound, prevent its diffusion through the air, and carry it direct to the ear, I propose to give the results of experiments made with an instrument which I have invented for hearing with both ears respectively, and which, as it is specially adapted for the auscultation of differences in the sounds of different parts of the chest, I have named the Differential Stethoscope, or Stethophone.

The results thus procured will be more satisfactory than those obtained by ordinary audition; a sound will be increased as a visual object is magnified by the microscope, and as both ears are similarly dealt with, a perfect parity of conditions will hold in respect of both ears.

The differential stethophone (see figure) is simply an instrument consisting of two hearing-tubes, or trumpets, or stethoscopes, provided with collecting-cups and ear-knobs, one for each ear respectively. The two tubes are, for convenience, mechanically combined, but may be said to be acoustically separate, as care is taken that the sound, once admitted into one tube, is not communicated to the other. The tubes are composed of two parts nearly equal in length, one near the ear-knob, made of metal (C); while the other part, near the collecting-cup, is made of metal wire (B), to impart flexibility. The ear-end is curved, so as to approach the ear, and is supplied with an ivory knob (D) for insertion into the meatus externus. The other end of the tube, being intended to collect sound, is supplied with a hollow cup, or

receiver (A) made of wood, or some such material. The mechanical construction of this instrument is borrowed from the stethoscope contrived by Dr. Caman of New York, and intended by its inventor for the purpose of hearing with both ears sounds emanating from *one* point, and collected into one cup. The two tubes are brought near together, a few inches in front of the face, by means of a connecting-bar (E), but calculated to prevent the transmission of sound from one tube to the other. This bar is supplied with a joint, which permits the tubes to be freely moved, as is necessary in applying the knobs to the ears. The two knobs are kept steadily in the ears by means of an elastic band (F) connecting the two tubes near the bar, already described.



The instrument being fitted into the ears, with the knobs directed upwards, and the cups being applied equally near to, or upon a sounding body, say the inflating lung, or a watch, and the conditions for collecting sound being the same, the sound is heard with both ears, as in ordinary hearing. But if one cup be removed a little, say a half or a quarter of an inch from the *watch* (for we shall now adopt it), and the other cup be left upon the watch, the sound is heard with that ear only which is connected with the cup placed upon the watch, and the sensation in the hearing ear is so marked, as to leave the mind in no doubt whatever that it is through that ear we become conscious of the sound. If the cup placed upon or nearer the watch, be removed a little further than the other cup, so as to be less favourably situated for collecting sound, say one inch from the watch, the ear connected with it becomes totally unconscious of sound, and the sensation of hearing is most unequivocally felt in the ear, and in that ear only, which but a moment before was utterly deaf to it. If one

cup be placed upon the middle of the watch, and the other on the edge, the watch sound is heard in that ear only which is connected with the cup placed upon the *middle.*

These experiments may be thus varied, and the result will in reality be the same, though apparently more remarkable. The watch, being held in the air, at the distance of about an inch from one ear, is heard distinctly beating into that ear only; but if the watch be now connected with the collecting-cup of the tube of the stethophone, inserted into the other ear, the sound, being greatly magnified, is heard in this ear, and in it only, the ear in which the sound had been primarily heard being now altogether insensible to it, or unaffected by it as far as our consciousness is concerned. The sensation of sound is transferred from one ear to the other, although the watch is allowed to remain in close proximity to the ear that is now deaf to its sound.

A watch placed upon or inside the cheek, is heard to beat in that ear which is nearer; but if the opposite ear be connected with it by means of one of the arms of the stethophone, or by a common flexible stethoscope, the watch sound is no longer heard in the ear nearer the watch, but in the ear further from it, which is now in reality brought into nearer connexion with it, by means of the hollow tube.

Sounds, produced in whatever material, are alike subject to this law, so far as my experiments have yet been made.

The medium in which sounds are produced does not alter this law. A watch ticking, or a bell ringing, either in the air or under water, affords the same results.

Sounding bodies give the same results when covered with soft or hard materials. A watch placed in one corner of a box, a few inches square, and an inch deep, is heard to beat in that arm of the stethophone only which is near it. By this means, and by successive movements of the instrument, and by attending to degrees of intensity, the exact position of the watch may be with certainty indicated. Or this may be effected by successively excluding those parts which fail to cause hearing in one of the ears.

The interposition of a body calculated to obstruct the sound at its entrance into one of the cups of the stethophone, causes the sound to be heard in that ear only which is connected with the cup which remains free from obstruction. This admits of ready proof, by

applying the two cups as much as possible equally on the middle of a watch about an inch above it, and by placing two fingers held together between one cup and the watch. When this is done, the watch is heard to tick into the ear that remains free from obstruction.

The removal of an obstructing body from one cup, while it is allowed to remain in operation with the other, causes sound which had been equally heard with both ears, to be heard in that one only which is connected with the cup freed from the obstructing body. Thus, if the fingers be interposed between the watch and the cups held equally over it, and the fingers be separated under one of the cups, so as to permit of atmospheric communication, the sound is heard in that ear only which is connected with this cup, and not at all in the other.

The effect of intensification of a sound in one ear depriving the other ear of all sensation of that sound, is interestingly shown by placing the *tubes* of the instrument across a block of wood with the cups hanging in the air. While both cups are left open, and a tuning-fork in vibration is placed between the two tubes, the sound is heard with both ears; but if one cup be closed with the hand, or with leather, and the other be left open, the sensation of sound is restricted to that ear connected with the closed cup. The sound in the tube connected with the closed cup is rendered more intense by the closure, the escape of sound is obstructed, and reverberation takes place. By virtue of the intensification, sensation is monopolized by one ear, and is lost in the other. The result and the mechanical conditions are much the same as in the experiments of Mr. Wheatstone with a tuning-fork held upon the head, presently to be referred to.

It is worthy of observation, that in order that a sound previously heard with, or in both ears, as in the above experiments, may be appreciated or felt in one ear only, it is not necessary that the stethophone, or other conducting instrument, be placed in the cavity of the meatus externus. It is sufficient for this result that the instrument be placed near the meatus, so as to give it an advantage of intensity over the opposite cavity. When the instrument is to be held only near the meatus, care should be taken not to touch the external ear, so that there may be no conduction by that part from contact, which would vitiate the experiment. The result is perfectly satisfactory and conclusive, although the remarkable sensation of

pouring in of sound into the ear is less marked,—a fact sufficiently intelligible from the diffusion of sound which must take place outside the ear, when the extremity of the tube is held there, and is not inserted into the meatus. It is therefore obvious that the restriction of hearing to one ear, under the conditions specified, is not due to closure of the meatus externus, the cause of the augmentation of sound in some experiments of Mr. Wheatstone, to be shortly referred to.

The remarkable phenomenon of the restriction of hearing to one ear, above described, seems not to be without important signification. It holds apparently in virtue of a law seemingly established for the purpose of enabling man and the lower animals to determine the direction of the same sound, with more accuracy than could be done had a judgment to be formed between the intensity of two similar sensations in the two ears respectively. All source of error is removed by there being only one sensation, although there may be two impressions. This law of a stronger impression in one ear, rendering us unconscious of a weaker, but similar impression in the other, has an analogue, though perhaps an imperfect one, in the sense of touch. Very strong impressions upon one part of the body cause such acute sensations, that minor impressions of the same kind upon another part are frequently not felt, in fact, produce no sensation.

The only observations bearing upon this law which I have been able to discover, are some by Mr. Wheatstone, in a paper entitled "Experiments on Audition," published in the 'Quarterly Journal of Science, Art, and Literature,' vol. ii. New Series, 1827. These experiments are intended to show the augmentation which the sensation of autophonic sound, and the sounds of a tuning-fork applied to the head, acquires when the ear is closed, although the perception of external sounds is diminished. Mr. Wheatstone shows that a vocal sound is heard louder in that ear that is closed, say with the finger, than in the other. He also shows, that the sound of a tuning-fork placed upon the head is heard louder in that ear which is closed than in the other which remains open, even though the tuning-fork may be brought nearer the open ear than the closed one. These experiments, Mr. Wheatstone says, prove that "sounds *immediately* communicated to the closed meatus externus are very greatly magnified;" and he adds, "it is an obvious inference, that if external

sounds can be communicated to act on the cavity in a similar manner, they must receive a corresponding augmentation."

This distinguished philosopher constructed the instrument named a Microphone, for the purpose of augmenting weak sounds upon this principle, *i. e.* the augmentation of sound by closure of the ears; and he informs us that it "is calculated for hearing sounds when it is in *immediate* contact with sonorous bodies," and that "when they are diffused by their transmission through the air, this instrument will not afford the *slightest assistance.*" This instrument is spoken of in connexion with the augmentation of sound, and not in reference to the limitation of sound to one ear, or to the comparison of sensations in the two ears. The remarkable, and, to the uninitiated mind, the wonderful fact, made known more than thirty years ago by Mr. Wheatstone, that a tuning-fork held upon the head close to an open ear is not heard in this ear, but in the opposite ear, provided it be closed with the finger, or by some other means, proved that sounds communicated to the skull were exclusively heard in the closed ear. In the case of the tuning-fork, the fact made known by Mr. Wheatstone is undoubted. The rationale of the phenomenon appears to be this:—The vibrations of the tuning-fork are communicated to the bones of the head, and through them to the ears, including their bones, cartilages, and contained air; but in the case of the closed ear, the vibrations are permitted no egress or escape, as in the open ear; reverberations take place, and the consequence is, that the sound is not duly moderated; and in virtue of the law I have just enunciated, the sensation of sound is restricted to the closed ear. When the tuning-fork, duly sounding, is held in the air, and not connected directly with the head, the closed ear remains insensible to it, and the sound is heard exclusively in the open ear.

Mr. Wheatstone's interesting observation relates to a head-sound not duly moderated, as in the opposite and open ear, and virtually more intense, and comes within the general law advanced in this paper, which embraces all sounds, whether internal or external, viz. that a sound of the same character in the presence of both ears, if conveyed by any means to one ear, or to the nerve of that ear, more intensely than to the other, is heard in the more favoured ear only.

It seems necessary, in Mr. Wheatstone's experiments, that the bones of the head shall vibrate freely; weak sounds, such as gentle blow-

ing, will not succeed; and if the tuning-fork be placed immediately under the open ear, and passed upon the soft parts, little fitted for vibration, between the mastoid process of the temporal bone and the lower jaw, the sound is heard in this ear, and not in the closed ear.

It may perhaps be well, before proceeding further, to acknowledge that I am well aware it has been long known that a very loud sound conveyed into one ear will render the other ear insensible to sound of a weak or low character. But the phenomenon which I have ventured to bring under the consideration of the Royal Society differs from this well-known and readily admitted fact in this important particular, that no very great loudness is required, and that no very great augmentation of sound in one ear over that in the other is necessary in order to restrict the sense of hearing to one ear, and to deprive the less favoured ear of the sense of hearing which it had previously enjoyed. A moderate, yet a decided increase of intensity is all that is required to remove the sense of hearing from the less favoured ear, and to cause the more favoured organ to be alone sensible to the sound.

When sound is proceeding into the two ears, but in consequence of its reaching one ear in greater intensity than the other is heard only in one ear, the sensation of hearing in the favoured ear, though strictly limited to it, is augmented by the sound entering the less favoured ear, although it entirely fails to cause a sensation there, or to produce a consciousness of sound in that organ. The more sound collected by the less favoured ear, as long as the amount is less than that conveyed to the other ear, the more the sensation of sound is augmented in the more favoured ear. The intensity of sensation in the more favoured ear increases in a ratio with the increase of sound in the less favoured ear, until the intensity of sound is the same, or nearly the same, in both ears, when the sensation experienced is the ordinary one of hearing with two ears.

This fact admits of satisfactory proof in this way:—A watch is placed on a table equidistant from both ears. The stethophone is applied to the ears; one cup is placed within an inch of the watch, while the other is turned away from it, at the distance of some inches. As the further cup is brought nearer and nearer the watch, the sound, *always* confined to the more favoured ear, is gradually and steadily intensified, until the two cups are, or are about to be, similarly

placed, at which moment the sensation ceases to be restricted to one ear, and has acquired its greatest intensity. This fact proves, that though the sensation of hearing be confined to the ear to which sound is communicated with greater intensity, we profit by the sound which is conveyed into the other ear, though failing to produce a sensation or a consciousness of sound there, by its serving to augment very materially the sensation of sound in the more favoured ear. The less favoured ear thus augments the sensation which we experience, at the same time that it fails to interfere with the aid which the sensation confined to one ear affords us as to the direction of external sounds.

The sounds of which we have been treating as differently affecting the two ears, according to the intensity with which they are respectively communicated, are of the same character, though differing in intensity. It is sounds of the same character only which exhibit the phenomenon of restriction in virtue of moderately different intensity. The sounds must emanate from the same sounding body, or from bodies sounding similarly. A little difference in *character* will cause the experiment of restriction to fail.

Thus, if two bells, differing considerably in character, be rung respectively in the two ears, one louder and graver than the other, the louder and graver sound does not render the other ear insensible to the weaker sound of the weaker bell. Both ears hear perfectly, but the loud, grave sound is heard in one ear, and the weak sound is heard in the other.

If, instead of one watch, we place two together, having sounds of different character, as for instance one low and grave, and another loud and sharp, and the two arms of the stethophone be placed over them respectively, the sounds of both watches are heard, but the sound of one is heard in one ear, and the sound of the other is heard in the other ear. The loudness of the sound in one ear does not increase the weakness of the sound in the other; or, in other words, the intensity of the sensation produced by the weak watch in the one ear is not reduced by the sensation produced by the loud watch in the other ear.

The sound of a watch ticking continues to be heard in one ear, although a large-sized bell is made to ring at the other; and I have not perceived that the sensation produced by the watch is at all

impaired by the bell. A whistling lung-sound heard in one ear, is not rendered less obvious by a loud blowing lung-sound in the other. A hissing murmur at the apex of the heart conveyed into one ear, and a rasping sound at the base conveyed into the other, are both heard without alteration in the ears to which they are respectively conveyed.

By virtue of these two laws,—1st, that sounds of the same character are restricted to that ear into which they are conveyed in greater intensity, and 2nd, that sounds differing in character may be heard at the same time in the two ears respectively, even if they be made to reach the ears in different degrees of intensity,—it is possible to analyse a compound sound, or one composed of two sounds, and to divide it into its component parts. In order to effect a division of a compound sound, it is only necessary that the two sounds of which it is composed may respectively be heard at certain points, in greater and lesser intensity, and that the respective cups of the stethophone be placed at these points. The ear connected with the cup placed where one half of the sound is in greater intensity, hears that half sound only, and the ear connected with the cup placed where the other half of the sound is in greater intensity, hears *that half* sound only. The sound is divided into two parts, and one is heard in one ear, and the other part in the other ear. For example, a compound sound composed of the two sounds of two watches placed together upon a table, with the unassisted ear is distinctly heard in its compound state, and cannot be divided into its two constituent parts. With the stethophone this is readily done. One cup is placed where the sound of one watch is in greater intensity, and the other is placed where the sound of the other watch is in greater intensity, and the result is obtained of one watch only ticking in one ear, and of the other watch only ticking in the other ear. The greater intensity of each watch-sound in one ear has rendered all hearing of it in the other ear impossible, and as each watch-sound in its greater intensity is conveyed to different ears, one is heard in one ear only, while the other is heard in the other ear only. Without the stethophone, or some such instrument, this analysis could not be made; the ordinary stethoscope will not succeed, for wherever it is placed it conveys the mixed or compound sound to the ear. If the naked ear be applied over or upon the

watches, the same result follows; and it is the same if instead of two arms of the stethophone we employ only one. This remarkable separation of the components of a sound may be effected also when the sounding bodies are enclosed in a box capable of transmitting sound, or when separated from us by the interposition of materials capable of conducting sound; and by successive trials and comparisons of intensity at different places, and by a process of exclusion of those parts which fail to cause sensation, the respective positions of two adjacent sounding bodies may be predicated. If, for example, we have two watches, A and B, enclosed in a box, and through one cup, A, we hear watch A, and with the other cup, B, we hear watch B, we may conclude that cup A is nearer watch A than cup B is, and so on. In the same manner we may auscultate the morbid sounds of the heart. By cup A, placed at the apex, and cup B placed at the base, we hear separately the morbid sounds of the two parts; for example, a blowing murmur at the apex in one ear, and a rasping murmur at the base in the other ear. This we are enabled to do, although at any intermediate point with the single ear, either with or without a stethoscope, we hear the conjoined two sounds. It is obvious that with the stethophone we not only succeed in separating sound, but that this instrument, or some similar contrivance, affords the only possible means of hearing, with two ears at once, sounds emanating from the same region or surface, for the sides of the head can be applied, of course, to the same sounding surface only in turn or succession. With this instrument we, as it were, place our ears in our hands, apply them where we choose, and listen with them both at adjacent or distant points of the same surface, at one and the same instant of time.

It is not unlikely that the property which the stethophone possesses of pointing out with precision where sound is most intense, may be very usefully employed. It seems possible that it might be turned to account in discovering the points where operations in military mining may be going on.

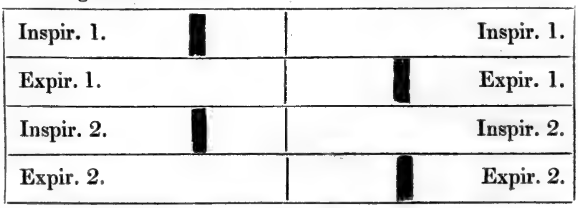
It is, however, in the practice of medicine only that the differential stethophone has been hitherto applied, and it may be here permitted to me to point to some of the chief purposes for which it is adapted, and for which it has been employed.

In respect to respiration, we may compare at once, and without

the inconvenience of moving the head, or the ordinary stethoscope, from place to place, the extent of the respiratory sounds in different parts, so that a very minute difference, an excess in one part or a deficiency in another, may with certainty be discovered. Differences in quality, such as softness or roughness, are readily recognized. The increased length and loudness in one part is accurately contrasted with the healthy conditions of another part. In cases where the *in*spiration has been very full in one place, in order to compensate for deficiency in another place, and where the *ex*piration was long and coarse in the deficient part, I have heard the inspiratory sound only in one ear, and the expiratory sound in the other ear. The sounds were respectively restricted to the two parts, and they alternated in a very marked manner. One part has remained silent while the other has been heard to sound, and this has been silenced when the other has awoke the ear.

The diagram represents the sounds occurring alternately in two sides of the chest in a *consumptive* patient. The dark spots represent the *sounds*.

Healthy. Right side of chest. — Unhealthy. Left side of chest.



The influence which the acts of respiration exert in heightening and lowering the murmurs in veins, say of the neck, in persons affected with a thin and watery condition of blood, is well exhibited by placing one arm of the stethophone on the chest and the other upon the veins.

When the respiration in two parts is alike in character, but decidedly louder in one part than in another, the sound in the weak side is lost. While this loss proves, in a very emphatic manner, the important fact of deficiency, it of course for the time deprives us of the opportunity of judging of the quality of the deficient inspiration; but this is readily obviated by removing the cup of the instrument from the full respiring part, and then the deficient respiration is immediately

heard through the other cup. Thus while the two sounds, being of like character, and one being more intense than the other, can be heard only in one ear at the same time, an admirable opportunity is obtained for contrasting the extent, and some of the qualities, of the sounds of the two parts, by placing the cups alternately and rapidly upon the two spots respectively. Vocal extussive resonance in two parts of the thorax, is well contrasted with the two tubes employed at once, or in immediate succession.

The sounds of the two sides of the heart, and of the valves of the two great arteries proceeding from that organ, are, by means of the stethophone, very advantageously dealt with. By placing it over the two sides of the heart, or the origin of the two arteries, we ascertain the character and loudness of the sounds of these parts. One cup being placed over the aorta, and the other over the pulmonary artery, if the sounds they collect differ in character, one sound is heard in one ear, and another in the other ear. We may have at the same moment an aortic murmur and a healthy pulmonary artery sound, one sound in one ear, and another sound in the other ear. But when it is desired to listen to each sound singly and in succession, the instrument will still be available, for the cups may be applied singly and in succession, thus affording ample means for contrast.

In cases of disordered heart, in which it is desired to discover whether the sounds of the two sides of the heart are synchronous, the stethophone affords the most satisfactory mode of investigating the fact. With it, we virtually place our two ears over the two sides of the heart; and if one side sounds at all after the other, the fact is made known, and the end of one sound and the beginning of another are clearly and distinctly defined. With the ordinary stethoscope this is impossible; for where one sound is heard, the other may be inaudible, and long before the head or stethoscope can possibly be adjusted at another part, the second sound has taken place, and is long since over.

In conclusion, I may perhaps be permitted to say, that the differential stethophone proves a great auxiliary in examining the heart with the cardioscope or sphygmoscope, which I had the honour to exhibit to this Society two years ago. While the latter instrument exhibits the movements of the heart, the stethophone informs us of their sounds, in a more complete manner than can be otherwise effected; and from the stethophone permitting of auscultating two

parts at once, and with the eyes directed to the chest, the relation of the movements and of the sounds, normal or abnormal, of this most important organ is very fully and satisfactorily made out.

Postscript. Received April 22, 1858.

In connexion with that part of my paper which treats of the restriction of hearing to the closed ear, I desire to add the fact which I have ascertained within the last few days, that if one ear be closed wholly or partially at its external part, *i. e.* at the meatus externus, by disease or by congenital malformation, while the other ear is healthy, the sound of the tuning-fork, applied to any part of the head, is heard only in the closed ear. This fact holds, although the closed ear is totally unaffected by sounds conveyed through the external air.

I have further to mention the fact, that all persons, deaf in one ear, whom I have lately examined, with one exception, hear the sound of the tuning-fork applied to the head in that ear only that is deaf to external sounds. A man who has been totally deaf in one ear for thirty years, in consequence of a violent blow upon the head, had the tuning-fork applied over the forehead. He started, and said that he heard only in the ear which had been deaf during that long course of time. In such cases I have been disposed to believe that, amidst other lesions of the organ of hearing, there may be present an obstruction or closure, that a reverberation takes place, and that thus a restriction of hearing is secured for the diseased organ.

II. "On the Stratification of Vesicular Ice by Pressure." By Professor William Thomson, F.R.S. In a Letter to Professor Stokes, Sec. R.S. Received April 3, 1858.

In my last letter to you I pointed out that my brother's theory of the effect of pressure in lowering the freezing-point of water, affords a perfect explanation of various remarkable phenomena involving the internal melting of ice, described by Professor Tyndall in the Number of the 'Proceedings' which has just been published. I wish now to show that the stratification of vesicular ice by pressure observed on a large scale in glaciers, and the lamination of clear ice described by Dr. Tyndall as produced in hand specimens by a

Brahmah's press, are also demonstrable as conclusions from the same theory.

Conceive a continuous mass of ice, with vesicles containing either air or water distributed through it; and let this mass be pressed together by opposing forces on two opposite sides of it. The vesicles will gradually become arranged in strata perpendicular to the lines of pressure, *because of the melting of ice in the localities of greatest pressure and the regelation of the water in the localities of least pressure, in the neighbourhood of groups of these cavities.* For, any two vesicles nearly in the direction of the condensation will afford to the ice between them a relief from pressure, and will occasion an aggravated pressure in the ice round each of them in the places farthest out from the line joining their centres; while the pressure in the ice on the far sides of the two vesicles will be somewhat diminished from what it would be were their cavities filled up with the solid, although not nearly as much diminished as it is in the ice between the two. Hence, as demonstrated by my brother's theory and my own experiment, the melting temperature of the ice round each vesicle will be highest on its side nearest to the other vesicle, and lowest in the localities on the whole farthest from the line joining the centres. Therefore, ice will melt from these last-mentioned localities, and, if each vesicle have water in it, the partition between the two will thicken by freezing on each side of it. Any two vesicles, on the other hand, which are nearly in a line perpendicular to the direction of pressure will agree in leaving an aggravated pressure to be borne by the solid between them, and will each direct away some of the pressure from the portions of the solid next itself on the two sides farthest from the plane through the centres, perpendicular to the line of pressure. This will give rise to an increase of pressure on the whole in the solid all round the two cavities, and nearly in the plane perpendicular to the pressure, although nowhere else so much as in the part between them. Hence these two vesicles will gradually extend towards one another by the melting of the intervening ice, and each will become flattened in towards the plane through the centres perpendicular to the direction of pressure, by the freezing of water on the parts of the bounding surface farthest from this plane. It may be similarly shown that two vesicles in a line oblique to that of condensation will give rise to such

variations of pressure in the solid in their neighbourhood, as to make them, by melting and freezing, to extend, each obliquely *towards* the other and *from* the parts of its boundary most remote from a plane midway between them, perpendicular to the direction of pressure.

The general tendency clearly is for the vesicles to become flattened and arranged in layers, in planes perpendicular to the direction of the pressure from without.

It is clear that the same general tendency must be experienced even when there are bubbles of air in the vesicles, although no doubt the resultant effect would be to some extent influenced by the running down of water to the lowest part of each cavity.

I believe it will be found that these principles afford a satisfactory physical explanation of the origin of that beautiful veined structure which Professor Forbes has shown to be an essential organic property of glaciers. Thus the first effect of pressure not equal in all directions, on a mass of snow, ought to be, according to the theory, to convert it into a stratified mass of layers of alternately clear and vesicular ice, perpendicular to the direction of maximum pressure. In his remarks "On the Conversion of the Névé into Ice*," Professor Forbes says, "*that the conversion into ice is simultaneous*" (and in a particular case referred to "*identical*") "*with the formation of the blue bands*;.... and that these bands are formed where the pressure is most intense, and where the differential motion of the parts is a maximum, that is, near the walls of a glacier." He farther states, that, after long doubt, he feels satisfied that the conversion of snow into ice is due to the effects of pressure on the loose and porous structure of the former; and he formally abandons the notion that the blue veins are due to the freezing of infiltrated water, or to any other cause than the kneading action of pressure. All the observations he describes seem to be in most complete accordance with the theory indicated above. Thus, in the thirteenth letter, he says, "the blue veins are formed where the pressure is most intense and the differential motion of the parts a maximum."

Now the theory not only requires pressure, but requires difference of pressure in different directions to explain the stratification of the vesicles. Difference of pressure in different directions produces the "differential motion" referred to by Professor Forbes. Further,

* Thirteenth Letter on Glaciers, section (2), dated Dec. 1846.

the difference of pressure in different directions must be continued until a very considerable amount of this differential motion, or distortion, has taken place, to produce any sensible degree of stratification in the vesicles. The absolute amount of distortion experienced by any portion of the viscous mass is therefore an index of the persistence of the differential pressure, by the continued action of which the blue veins are induced. Hence also we see why blue veins are not formed in any mass, ever so deep, of snow *resting* in a hollow or corner.

As to the direction in which the blue veins appear to lie, they must, according to the theory, be something intermediate between the surfaces perpendicular to the greatest pressure, and the surfaces of sliding; since they will commence being formed exactly perpendicular to the direction of greatest pressure, and will, by the differential motion accompanying their formation, become gradually laid out more and more nearly parallel to the sides of the channel through which the glacier is forced. This circumstance, along with the comparatively weak mechanical condition of the white strata (vesicular layers between the blue strata), must, I think, make these white strata become ultimately, in reality, the surfaces of "sliding" or of "tearing," or of chief differential motion, as according to Professor Forbes's observations they seem to be. His first statement on the subject, made as early as 1842, that "the blue veins seem to be perpendicular to the lines of maximum pressure," is, however, more in accordance with their mechanical origin, according to the theory I now suggest, than the supposition that they are *caused* by the tearing action which is found to take place along them when formed. It appears to me, therefore, that Dr. Tyndall's conclusion, that the vesicular stratification is produced by pressure in surfaces perpendicular to the directions of maximum pressure, is correct as regards the mechanical origin of the veined structure; while there seems every reason, both from observation and from mechanical theory, to accept the view given by Professor Forbes of their function in glacial motion.

The mechanical theory I have indicated as the explanation of the veined structure of glacial ice is especially applicable to account for the stratification of the vesicles observed in ice originally clear, and subjected to differential pressure, by Dr. Tyndall; the formation of the vesicles themselves being, as remarked in my last letter*, anticipated

* See Proceedings for February 25, 1858.

by my brother's theory, published in the 'Proceedings' for May 1857.

I believe the theory I have given above contains the true explanation of one remarkable fact observed by Dr. Tyndall in connexion with the beautiful set of phenomena which he discovered to be produced by radiant heat, concentrated on an internal portion of a mass of clear ice by a lens; the fact, namely, that the planes in which the vesicles extend are generally parallel to the sides when the mass of ice operated on is a flat slab; for the solid will yield to the "negative" internal pressure due to the contractility of the melting ice, most easily in the direction perpendicular to the sides. The so-called negative pressure is therefore least, or which is the same thing, the positive pressure is greatest in this direction. Hence the vesicles of melted ice, or of vapour caused by the contraction of melted ice, must, as I have shown, tend to place themselves parallel to the sides of the slab.

The divisions of the vesicular layers into leaves like six-petaled flowers is a phenomenon which does not seem to me as yet so easily explained; but I cannot see that any of the phenomena described by Dr. Tyndall can be considered as having been proved to be due to ice having mechanical properties of a uniaxal crystal.

April 29, 1858.

J. P. GASSIOT, Esq., Vice-President, in the Chair.

The following communications were read:—

I. "An Account of the Weather in various localities during the 15th of March, 1858 (the day of the Great Solar Eclipse); together with Observations of the Effect produced by the Diminution of Light upon the Animal and Vegetable Kingdoms." By Edward Joseph Lowe, Esq., F.R.A.S., F.G.S., F.L.S., F.Z.S. &c. Communicated by Thomas Bell, Esq., P.L.S. Received April 1, 1858.

[Abstract.]

By the author's request, observations were made at 9 A.M., 11 A.M., and from noon every fifteen minutes up to 2^h 16^m, at 3 P.M.,

4 P.M., 5 P.M., and 9 P.M., and these observations, consisting of the "Temperature in shade," "Wet-Bulb Thermometer in shade," "Temperature on grass in sunshine," "Temperature in sunshine," "Amount of cloud and direction of wind," have been arranged in Tables according to their distance from the annular path. The observations were made at the following stations;—

Somerton, Towcester, Isham, Peakirk, on the central line; Teignmouth, Little Bridy, and Bicester, within 10 miles of the central line; Exeter, Gloucester, Grantham, and Belvoir Castle, within 25 miles; Truro, Guernsey, Helston, Aldershott, Berkhampstead, Hereford, Royston, Norwich, and Highfield House, within 50 miles; London, Tottenham, Ventnor, and Southampton, within 75 miles; Uckfield, Leeds, Scarborough, Wakefield, Hawarden, Old Trafford, and Chorlton, within 100 miles; and Hastings, Fairlight, Lampeter, North Shields, Silloth, Liverpool, Stonyhurst, Durham, Edinburgh, Culloden, Isle of Man, Aberdeen, Orkney, Armagh, Belfast, Utrecht, and Vienna, above 100 miles from the central line. Readings of the barometer and extra remarks are appended at the close of the Tables.

The differences between the sun-thermometers in the air and on the grass are not so marked as might be expected, for it happens that in March and October their readings nearly approach each other. In winter the temperature in sunshine on the grass is considerably below that in the air, whilst in summer this condition is reversed. The dry-bulb thermometer fell at the middle of the eclipse from 2 to 4 degrees, the average being about 2¼ degrees; to this, however, must be added an extra amount, on account of the time of day at which the eclipse took place; had there been no eclipse, the temperature would necessarily have risen. The wet-bulb thermometer did not fall quite so much, as the air became more charged with water at the centre of the eclipse, the result of the phenomenon. Thermometers in sunshine (even where overcast) fell twice as much as those in shade.

At the majority of stations the early morning was exceedingly fine, and the sky almost free of clouds, yet before the eclipse commenced the sky became overcast and continued so. It seemed quite evident that the clouds were formed *in situ*. Durham, Edinburgh, Scarborough, Uckfield, London, Norwich, Southampton, Royston, Leicester, Belvoir Castle, Little Bridy, Isham, and Guernsey, were all places in which the weather previous to the eclipse had been more

or less free of clouds, and yet all were enveloped by cloud before noon.

The following features were very apparent:—The wind, although brisk before and during the progress of the eclipse, considerably moderated at the time of greatest obscuration, becoming brisk again afterwards. The darkness, although *felt*, was by no means so great as had been expected; yet this was in a great measure owing to the overcast sky. The pupil of the eye was not contracted by strong light, consequently it was able to take in the diminished light over a larger surface, diminishing the effect of darkness to our senses. Practically it was dark; the impossibility of reading the instruments at Isham, Towcester, and Grantham, was a certain measure. I have seen greater apparent darkness produced by a storm, and yet the darkness was not such as to prevent instruments being read. The contracted landscape was well shown at Isham and Highfield House. The change in the colour of the landscape was almost universally remarked, as well as the great stillness at the time of greatest obscuration. A solar halo occurred in the Orkney Islands during the time of greatest obscuration. Rooks everywhere returned to their rookeries; fowls prepared to go to roost; peafowl actually went to roost; turkies hastened home; cocks crowed; sparrows appeared frightened; song-birds sang as in early morning, and kept up their song all afternoon. Bees returned to their hives. Cows seemed to have imagined that milking-time had arrived. The crocus and hepatica closed their flowers. An effect on sea animals was not observable; the *Actinia crassicornis*, which always expands in the evening, did not open during the eclipse.

II. "On the Structure and Functions of the Hairs of the Crustacea." By Campbell De Morgan, Esq. Communicated by George Busk, Esq. Received March 13, 1858.

(Abstract.)

The object of this communication is to determine, by the observation of their anatomical relations, the uses of the hairs and similar appendages to the shell of the Crustacea. The author mentions the observations of those who have of late specially investigated this subject. M. Lavalle noticed the connexion at times of the canals of the

hairs with canals penetrating the whole thickness of the shell, and the *occasional* continuity of the matter which filled the hairs with that which exists in the corresponding canal of the shell. M. Hollard says that the canals of the shell which correspond to the hairs, are occupied by membranous investments, which embrace the base of the hairs, and seem to receive an extension of the nutrient system. He suggests that amongst other functions, the hairs may possibly be connected with that of general sensibility. Dr. Häckel in a recent publication has shown that the canals of the shell and hair are lined by a continuation of the outer layer of the soft internal integument, which he calls the chitinogenous layer. He describes minutely the structure of the inner integument, and his account on the whole agrees with that given by Milne-Edwards; but he does not recognize the presence in the canals, of any of the elements of the inner integument except the external cuticular or chitinogenous layer; nor the connexion of these canals with the corium which lies beneath it, and which receives abundantly nerves and vessels.

According to the investigations of the author, it is with this deeper, vascular and nervous layer that the contents of the hair-canals and of the corresponding canals in the shell are especially connected. This can be readily seen in parts where the shell is thin, as in the foot-jaws for example. In a section made in such a situation, the canals leading to the hairs will be found to be often nearly as large as the bases of the hairs to which they correspond. They are lined by a thick membrane, which invests the cup-shaped cavity in which the hairs are implanted, and becomes so closely connected with the bulb of the hair itself, that it is often dragged out with it when the hair is pulled out. The cells and other elements of the deeper layer of the internal integument fill up the canal and pass on into the hairs.

Where the shell is thick, as in the claw of a lobster, the sheaths which are connected with the hair-bulbs and line the shell-canals can be demonstrated in the manner adopted by Mr. Tomes to show the existence of the dentinal fibres. If a section of a part of the shell of the claw where the hairs are implanted, and which has been previously softened in dilute acid, be torn through, the sheaths will usually be dragged out, and will be seen projecting from the torn edges, their contents often remaining in them. The connexion of the inner in-

tegument with these sheaths may be seen in sections of the claw with the integument still adhering to it, when on carefully tearing away the latter, its prolongations into the sheaths will be dragged out. That the hairs have some especial and important connexion with the inner vascular and nervous layer of the integument of the lobster's claw and elsewhere, seems probable from the observations made by the author on the contents of the claw. The terminal moveable piece, the pollex, and the prolongation of the metatarsus which it opposes, the index, do not contain muscular fibre, but are filled entirely by a soft pulpy mass of corium. The nerves of the limb are large, but only some small branches will be found to go to the muscles; the principal nerves pass on and terminate in the pulp which fills the opposing pieces of the claw. The author believes that it is the office of the hairs to establish a communication between the outer surface and this inner, and no doubt highly sensitive pulp, and that this is rendered still further probable by the comparison of the claws on the two sides. In the smaller claw the edges are sharp, and have fine tubercles along their margin; and the hairs are placed in a regular series of short tufts on each side of the tubercles, beyond which they do not project. But on the larger crushing claw, the tubercles are massive, and no hairs are seen projecting above the surface. If, however, a section be made, it will be seen that a communication is established between the inner pulp and the surface by means of an abundant series of canals which terminate in bulbous extremities, sometimes projecting beyond the surface, sometimes lodged in depressions in the shell. This arrangement may be found in other parts; and in the crab's claw, where the tubercles are deficient, these hairless pulp-cavities almost entirely replace the hairs.

Here, then, lodged within the densest part of the shell, is a structure richly supplied with nerves, shut off from other parts of the body, and having communication with the surface only through the medium of canals, which are sometimes continued into short bristles, and sometimes terminate in mere bulbs. As a prehensile organ, the claw needs sensibility, but no force which the animal could exercise could make any impression on the parts within, through its dense tuberculated edges. On the other hand, it is difficult to assign any office to the bristles, and still more to the bulbs, mechanical or otherwise, unless it be that which has been suggested,—that, establish-

ing, as they do, a communication between the external surface and the nervous structure within, they communicate impressions, and are in fact tactile organs.

The author had satisfied himself, before the appearance of Dr. Häckel's paper, that the hairs were connected with the inner layers of the corium, and not with the chitinogenous membrane only; and he had seen indications in the lobster and larger Crustacea of an arrangement of the pulp corresponding to the arrangement of the hairs. In the smaller Crustacea, especially in the shrimps, he found a remarkable confirmation of his views. In the flabelliform processes, and even in the claws in these animals, he found that the structures within the shell were arranged in the form of tubes corresponding to the hairs, through which passed from the deeper parts, fibres which were prolonged into the hair-canals. In the claw the nerve was traced to the inner termination of these tubes. The tubes in some instances merged internally into the general mass of the corium; in others they were truncated. Externally, or towards the margins, they presented open orifices, through which the fibres passed. The fibres, when drawn out from the hair-canals, often presented the plumose or serrated character, according to the form of hair to which they belonged. They could be traced for some distance down the tubes, and at times completely through them, but their deep connexions could not be clearly made out. Several modifications of this arrangement are described and figured. The author believes that the facts brought forward are sufficient to establish that the hairs of the Crustacea are probably organs by which external impressions are communicated to the internal sensitive parts.

III. "Note on the Measurement of Gases in Analysis." By A. W. Williamson, Ph.D., F.R.S., Professor of Chemistry in University College, and W. J. Russell, Ph.D. Communicated by Dr. Williamson. Received April 6, 1858.

In Bunsen's admirable method of gas analysis, considerable time and trouble are expended in observing the exact temperature and pressure to which the gas is subjected at the time of measurement; and also in calculating from these data the volume which the gas would occupy at the normal temperature and pressure. Frankland's

excellent apparatus, on the other hand, protects the gas from the influence of variations of atmospheric pressure, and, under favourable conditions, even from the influence of change of temperature; but the complication of this apparatus, and its liability to derangement, seem likely somewhat to limit its use.

If, when a fall of temperature takes place, we could diminish the pressure on the gas exactly in proportion to the diminution of elasticity which it undergoes, such fall of temperature would evidently not alter the volume of gas in the eudiometer. In like manner a rise of temperature might, if known, be counteracted by lowering the eudiometer-tube. The same remarks apply to variations of barometric pressure; as an increase of this influence might be counterbalanced by raising the eudiometer, and a diminution by depressing it.

It is therefore a question of some interest to find, for any atmospheric temperature and pressure, at what height of the eudiometer the enclosed gas will occupy the same volume as at the normal temperature and pressure. This is easily found by introducing a standard quantity of air into a tube over mercury, marking off the height of the mercury in the tube at the normal temperature and pressure; then, at any other temperature or pressure, raising or lowering the tube in the mercurial trough so as exactly to bring the enclosed air to its normal volume. The mercurial pressure needed for this purpose is evidently the same as that needed under the same circumstances for the reduction of any quantity of gas to the volume which it would occupy at the normal temperature and pressure.

The apparatus we use in applying this principle to gas analysis (fig. 1) consists essentially of the ordinary Bunsen's eudiometer, and a "pressure-tube," which is simply a tube of some 6 or 7 inches in length, and about the diameter of an ordinary eudiometer. It is closed at one end, and to the other is fixed a smaller tube of about the same length. Such a quantity of air is introduced into this pressure-tube, that when it is inverted in the trough the mercury stands at a convenient height in the narrow tube. At this point a mark is made, which indicates the height of mercury needed at any temperature or pressure to reduce the enclosed air to its original volume. The mercurial trough which we have used differs only from the ordinary one in being provided with a well at one end, thus

Fig. 1.

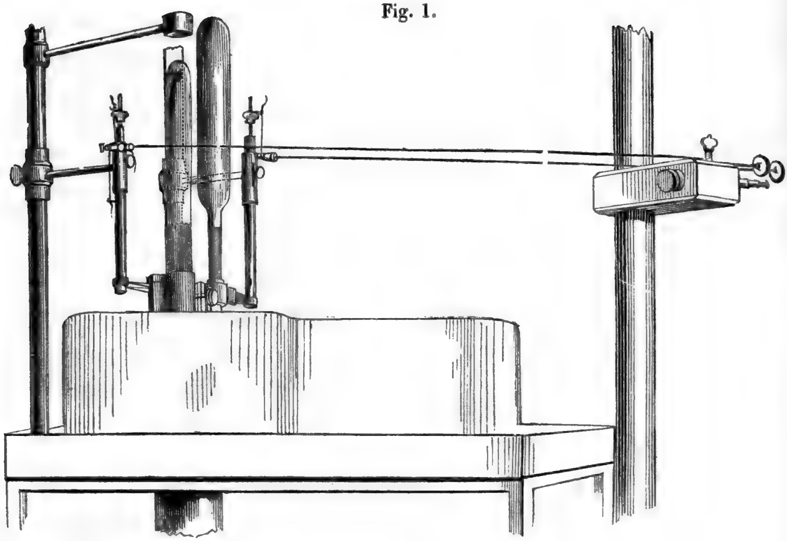
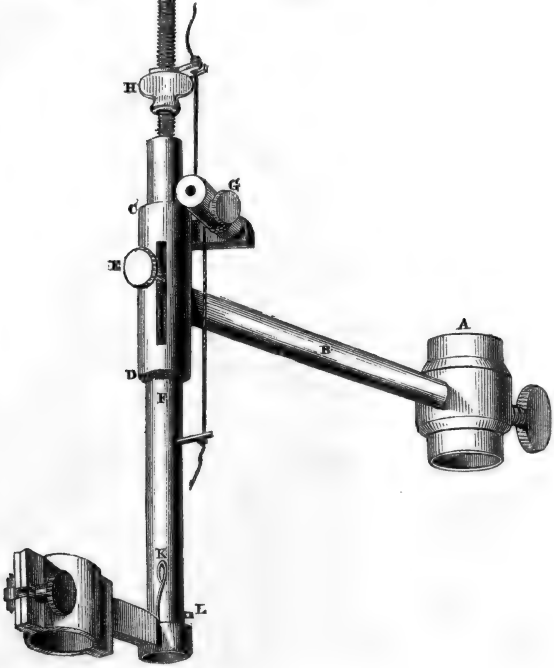


Fig. 2.



enabling the operator to raise or depress the eudiometer at pleasure, so as always to bring the gas which it contains to the same pressure as the air in the pressure-tube. Both the eudiometer and the pressure-tube are held in a perpendicular position by means of clamps which slide on upright rods. Each clamp is provided with a simple kind of slow movement, by which the tube can be raised or lowered by the operator whilst he is looking through a horizontal telescope at a suitable distance. We place the pressure-tube in front of the eudiometer, and by means of the fine adjustment bring the column of mercury in the small tube exactly to the normal mark. The eudiometer is then adjusted, also by means of the slow movement, so that the top of its meniscus (as seen through the horizontal telescope) exactly coincides with the top of the meniscus in the pressure-tube. This is easily done; for the diameter of the pressure-tube is considerably smaller than that of the eudiometer, and the meniscus in the latter can be clearly seen on both sides of the meniscus in the pressure-tube.

By this method we are able to obtain very accurate results with considerably less trouble than by Bunsen's method, and also without having any calculations to perform. The following analyses made during very stormy weather, of air deprived of its carbonic acid by potash, gave results amongst which the greatest difference was only four hundredths of a per cent. (·04).

I.

Volume of air taken		144·81
After addition of hydrogen		234·50
After explosion		144·00
Nitrogen	79·168	
Oxygen	20·832	
	100·000	

II.

Volume of air taken		139·55
After addition of hydrogen		229·07
After explosion		141·89
Nitrogen	79·176	
Oxygen	20·824	
	100·000	

III.

Volume of air taken			148·1
After addition of hydrogen			236·04
After explosion			143·30
	Nitrogen	79·139	
	Oxygen	20·861	
		100·000	

IV.

Volume of air taken			149·14
After addition of hydrogen			248·57
After explosion			155·28
	Nitrogen	79·150	
	Oxygen	20·850	
		100·000	

We are still engaged in experiments on this and some other points of gas analysis, and hope to have the honour of communicating our results before long.

DESCRIPTION OF THE FIGURES.

Fig. 1 represents the whole apparatus.

Fig. 2 represents the clamp with the fine adjustment attached to it.

A is the part which slides up and down the vertical rod; it is furnished on the inside with a small steel peg which moves in a groove, thus causing this arm always to remain in the same plane.

C D is a tube through which the rod F carrying the clamp passes.

E is a screw which retains the rod F in its place, and by means of which the friction of the rod passing through the tube can be increased.

G is the fine adjustment. As this small cylinder is turned round to the right or to the left, so the string either above or below it is wound on to it, and consequently the rod F raised or lowered.

H is merely an arrangement by which the string can always be tightened.

K is a peg so placed with regard to the stop L, that when, by turning the clamp round, it is pressed against the stop, the tube is then in the right position for applying the final adjustment and reading off.

IV. "On the Theory of Internal Resistance and Internal Friction in Fluids; and on the Theories of Sound and of Auscultation." By ROBERT MOON, Esq., M.A., late Fellow of Queen's College, Cambridge. Communicated by ARTHUR CAYLEY, Esq. Received April 3, 1858.

(Abstract.)

The author shows in the first instance, that when sound is propagated along a cylindrical tube filled with air, the compression which takes place in any element calls forth a resistance which diminishes the velocity of the particles in the element, at the same time that the dilatation which takes place in any element calls into play a force which will tend to increase the velocity of the particles in the element. He considers that the amount of the force thus called into play (whether it be accelerative of, or retarding the motion) in an element of given magnitude in a given indefinitely short interval, will depend solely on the amount of compression or dilatation developed in the element in the interval, and the state of density in the element at the time; and he is thus led to the conclusion, that to the ordinary equation for the transmission of sound through a column of air must be added a term of the form

$$\pm b^2\left(\frac{dy}{dx}\right)^{-1}\frac{d^2y}{dx\,dt},$$

where x denotes the distance from the origin of the element when the air is at rest, y the same distance at the time t when the air is in motion, b^2 a constant depending on the compressibility of air under given circumstances; so that the accurate equation of sound (variation of temperature being neglected) will stand

$$\frac{d^2y}{dt^2}=a^2\left(\frac{dy}{dx}\right)^{-2}\frac{d^2y}{dx^2}\pm b^2\left(\frac{dy}{dx}\right)^{-1}\frac{d^2y}{dx\,dt}, \quad . \quad . \quad . \quad (1)$$

in which equation the upper or lower sign of b^2 is to be taken according as the motion of the particles is in the direction in which x is measured positively, or the contrary.

On the same principles the author shows that, in the case of elastic fluids, the general equations of motion, when internal resistance is taken into account, must be written as follows:—

$$\left.\begin{aligned}\frac{1}{\rho}\frac{dp}{dx}&=\mathrm{X}-\frac{d(u)}{dt}\pm k^2\rho\frac{du}{dx}\\ \frac{1}{\rho}\frac{dp}{dy}&=\mathrm{Y}-\frac{d(v)}{dt}\pm k^2\rho\frac{dv}{dy}\\ \frac{1}{\rho}\frac{dp}{dz}&=\mathrm{Z}-\frac{d(w)}{dt}\pm k^2\rho\frac{dw}{dz}\end{aligned}\right\};\quad\ldots\ldots\quad(2)$$

where ρ denotes the density; X, Y, Z the impressed forces acting on the element; u, v, w the resolved parts of the velocity parallel to the coordinate axes; $\frac{d(u)}{dt}$ the total differential coefficient of u with respect to t, &c.; and k^2 replaces the b^2 of the preceding case. The author considers that, for moderate ranges of density, the above equations accurately represent the whole internal resistance.

It is next shown, that when the fluid is inelastic, the same equations will represent the motion, provided that we obliterate ρ in the terms involving k^2.

The force of internal friction in an elastic fluid in which the whole motion takes place parallel to the axis of x, and in which the whole lateral variation of motion transverse to the axis of x occurs in a direction parallel to the axis of z, is then shown to be properly represented by $\pm n^2\rho\frac{du}{dz}$, where n^2 is a constant depending on the nature of the fluid; the sign of the term to be introduced into the equation of motion being determined by the consideration that friction must always be a retarding force. The author thence derives the conclusion, that in order to represent the effect of internal friction in the motion of an elastic fluid, we must add to the first of equations (2) a term of the form

$$\pm n^2\frac{u}{\mathrm{V}^2}\cdot\rho\psi,$$

where

$$\mathrm{V}^2=u^2+v^2+w^2,$$

and

$$\psi^2=\left(u\frac{d\mathrm{V}}{dy}-v\frac{d\mathrm{V}}{dx}\right)^2+\left(w\frac{d\mathrm{V}}{dx}-u\frac{d\mathrm{V}}{dz}\right)^2+\left(v\frac{d\mathrm{V}}{dz}-w\frac{d\mathrm{V}}{dy}\right)^2;$$

and similarly with regard to the other two equations. When the fluid is inelastic, the terms in the equations of motion depending upon friction will be identical with those in the preceding case if we obliterate from the latter ρ.

Reverting to the equation of sound, which (neglecting terms of the second order) may be put under the form

$$\frac{d^2y}{dt^2}=a^2\frac{d^2y}{dx^2}\pm 2ae\frac{d^2y}{dx\,dt},$$

the author next shows that if the initial disturbance consist of a condensation alone, it will be transmitted with the velocity $a(1-e)$ in the direction in which its particles are moving; and that if it consists of a rarefaction alone, it will be transmitted with the velocity $a(1+e)$ in the direction contrary to that in which its particles are moving. It is here shown also incidentally, that whether the resistance be taken into account or not, the particles of a wave of condensation must all move in the same direction, which will be the direction of transmission; and the particles of a wave of rarefaction will all move in the same direction, which will be contrary to that of transmission.

In confirmation of the conclusion that waves of rarefaction are transmitted more rapidly than waves of condensation, the author adduces the fact, that when explosions of gunpowder have taken place, the glass in windows has been observed to break outwards rather than inwards.

It is then suggested, that, as when sound is produced, a condensation and rarefaction of air usually occur in immediate succession, if both kinds of disturbance were capable of affecting the human ear, we should hear sounds double; and as we know practically that this is not the case, it is contended that only one kind of disturbance, *i. e.* either rarefaction alone, or else condensation alone, can stimulate the ear.

It is shown to be *à priori* probable, that if one of the two classes of aërial disturbance is suppressed by the ear, that one would be disturbance by condensation, inasmuch as waves of rarefaction being swifter, would better perform the duty entrusted to them: and it is pointed out that if the sensation of sound is produced by aërial rarefactions alone, a difficulty attending the received theory will be obviated, by reason of the velocity deduced upon that theory being too small.

The author considers, however, that the question, whether either and which of the kinds of aërial disturbance is suppressed, can only be satisfactorily determined by examination of the ear itself. He

accordingly endeavours to establish, by arguments derived from the structure of the ear, that aërial rarefactions are alone capable of stimulating that organ in man. These arguments are briefly as follows:—

1. The tympanal membrane being convex inwards, a condensation could only affect the air in the tympanal cavity by stretching the membrane, which would cause an expenditure of force; whereas a rarefaction would produce the effect by a simple flexure of the membrane.

2. The sense of hearing being certainly produced by the motion of the fluid in the labyrinth, which is a closed vessel filled with an incompressible fluid, the requisite motion could not be produced by a compression of the atmosphere.

3. The disposition of the muscles of the ear is such as is calculated to assist and regulate the impressions produced by rarefactions rather than those produced by condensations.

4. The existence of the Eustachian tube is indispensable to the action of the organ (when all its other parts are in a normal state), on the supposition that sound is occasioned by rarefaction; whereas its uses are not satisfactorily predicable on the contrary hypothesis.

The author observes, that if rarefactions alone produce sound, it follows that a simple contraction of the muscles of the ear will render sounds inaudible. It follows also, on the same hypothesis, that a more delicate exercise of the same muscles will render the organ minutely susceptible to the influence of certain sounds, to the exclusion of others. It is urged also, that, admitting the action of these muscles to be to a large extent involuntary, there can be no doubt that by practice a great degree of command may be acquired over them. The author conceives that we may in this way account for the facility acquired by many persons of reading and writing, and of carrying on intricate trains of thought, without being disturbed by, or being conscious of, the noises around them. He conceives also that the same mode of explanation may be applied to account for the power of appreciating and analysing the most complex harmonies possessed by persons having a fine musical ear; which the author considers to be as certainly the result of specific mental and muscular training, as the faculty of vocalization, or the art of playing on a keyed instrument.

The author concludes by observing, that the equations for the transmission of an undulation along a musical string require a similar correction to that introduced in the case of aërial vibrations. The discussion of this branch of the subject he reserves for a future opportunity.

May 6, 1858.

The LORD WROTTESLEY, President, in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates recommended by the Council for Election into the Society:—

Thomas Graham Balfour, M.D.
Edward Mounier Boxer, Captain R.A.
Frederick Currey, Esq.
David Forbes, Esq.
Alfred Baring Garrod, M.D.
William Henry Harvey, M.D.
The Rev. Samuel Haughton.
Henry Hennessy, Esq.
David Livingstone, LL.D.
John Lubbock, Esq.
Henry Darwin Rogers, LL.D.
William Scovell Savory, Esq., M.B.
Warington Wilkinson Smyth, Esq.
Lieut.-Col. Andrew Scott Waugh, B.E.
Thomas Williams, M.D.

The following communications were read:—

I. "On the Influence of Heated Terrestrial Surfaces in disturbing the Atmosphere." By THOMAS HOPKINS, Esq. Communicated by WILLIAM FAIRBAIRN, Esq. Received April 13, 1858.

(Abstract.)

In this paper the author stated that the Hadleian theory of winds, which is now the one generally recognized, is not supported by the evidence of facts, but rests on assumptions founded on imaginary effects of the partial expansion of the atmospheric gases by heat. It is assumed in that theory, that when the tropical heat expands these gases, they rise and flow away laterally in the higher regions towards the poles, from which they return to the tropics in the lower regions. But it was contended by the writer of the paper, that such heating

of the gases merely expands them, without making them rise and overflow to other parts. The theory of Halley, once generally adopted, represented that the air was greatly heated in the particular part where the sun was nearly vertical, which made the air rise in that part alone, admitting cooler air to flow into the place of that which had ascended, and produced an influx of cool air below, from all parts around, to the heated part, and an overflow above from it. But in time experience showed that this hypothesis was not in accordance with facts, and it was abandoned. The theory of Hadley, which has been since adopted, substitutes the whole tropical belt, for the heated locality of Halley, which travelled with the sun in his daily course; but the supposed rise of air in the tropical belt, with an overflow above and an influx below, was asserted to be equally unsupported by experience, and, being unproved, may be fallacious. The rise of heated air in a chimney, sometimes pointed at as an illustration, was shown to be not analogous to that which takes place when the sun heats the air unequally in different latitudes; if it were, the theory of Halley would be true, and cool air would flow from all parts around to the greatly heated locality, just as cool air passes to a fire, and, when heated, up a chimney. It was then shown that it is gravitation which establishes an equilibrium of pressure in the atmosphere, and that direct solar heating of the surface of the earth and the air near to it, does not destroy that equilibrium. The sun by heating the gases merely expands them, in proportion to the increase of temperature in the part near the surface, and the gases over every portion of the hemisphere that is exposed to the action of the sun is proportionally heated, expanded and raised without any overflow of air taking place; leaving the equilibrium of pressure undisturbed by such heating. The solar heat merely raises the air that is near the surface, over the most heated latitudes, a little higher than the adjoining less heated, the difference in the rise in the various latitudes, from the polar to the tropical regions, being successively small; and as there is no alteration produced in *the weight of any vertical column* of the atmosphere, in any latitude, there is neither overflow of air above, nor disturbance of the equilibrium of pressure. The great disturbances that take place in the atmosphere were then maintained to be caused by the heat which is conveyed, from the surface of the globe, in vapour to different parts of the atmosphere

at various heights, and liberated in those parts when the vapour is condensed into liquid. This liberation of heat creates ascending currents in the parts locally affected, when horizontal winds, produced by gravitation, blow over the surface towards the ascending currents to re-establish the disturbed equilibrium. This process, by heating the air in the middle regions, was asserted to have been proved to be the cause, not only of the great trade-winds and the monsoons, but of the storms and local winds over the different regions of the globe.

II. "Notes of Researches on the Poly-Ammonias." By A. W. Hofmann, LL.D., F.R.S.—No. II. Action of Chloroform upon Aniline. Received April 15, 1858.

In a former Note addressed to the Royal Society (Proceedings, vol. ix. p. 150), I have alluded to some new alkaloids which are produced by the action of the bromides of triatomic alcohols upon the primary amidogen bases.

I have since examined more minutely one of these bodies. At the common temperature, chloroform and aniline may be left in contact for a considerable time without any change becoming perceptible. Even at the temperature of boiling water scarcely any reaction takes place. But on exposing for ten or twelve hours a mixture of about equal volumes of chloroform and aniline in sealed tubes to a temperature of 180° or 190° C., a hard brown crystalline mass is obtained, which consists chiefly of the hydrochlorates of aniline and of a new crystalline base.

To obtain this compound in a state of purity, the brown crystalline mixture formed in the digester-tubes is triturated with a small quantity of water, thrown upon a filter and washed with water. The first washings chiefly consist of hydrochlorate of aniline, which base separates in oily globules on addition of potassa to the filtrate. By testing the filtrate in this manner from time to time, it is found that the basic body separated by addition of potassa gradually exhibits a tendency to solidify, and ultimately falls as a yellowish-white crystalline precipitate. The residue upon the filter is now dissolved in warm (not boiling) water, separated by a filter from a brown resinous insoluble substance, and precipitated by ammonia or potassa. The crystalline precipitate obtained in this manner is washed till free from

alkali, and repeatedly crystallized from weak spirit. It is difficult to obtain it perfectly white, a yellowish substance, which appears to be partly formed during the process of solution, adhering with great pertinacity.

Thus obtained, the new base is a white crystalline powder; frequently it is obtained in minute scales, generally of a yellowish tint. It is insoluble in water, but readily dissolves in alcohol and ether. From the hot solution in these solvents it is precipitated by water as a yellow oil, solidifying on cooling with crystalline structure. It is easily dissolved by acids, with many of which it forms crystalline compounds. From the saline solutions thus produced the base is reprecipitated by potassa and also by ammonia. The salts of the new base are not very stable; their solutions, especially when heated for some time, inevitably contain more or less aniline, the crystalline base itself undergoing changes which I have not yet sufficiently examined.

The analysis of the new compound presents some difficulty. Even after protracted exposure over sulphuric acid in the exsiccator, it retains a small quantity of water, while a temperature of 100° is apt to decompose it.

The nature of the body was, however, readily established by the examination of a perfectly stable hydrochlorate, and also of a very definite platinum-salt.

The results obtained in the analysis of these salts establish for the new base the formula

$$C_{26}\,H_{12}\,N_2 = \left.\begin{matrix} (C_{12}\,H_5)_2 \\ (C_2\;H\;)''' \\ H \end{matrix}\right\} N_2.$$

It is obviously formed by the substitution of the triatomic molecule $(C_2\,H)'''$ for 3 equivalents of hydrogen in 2 molecules of aniline, which thus coalesce into a diamine molecule. Accordingly the base might be called diphenyl-formyl-diamine, that is, diammonia, in which 2 equivalents of hydrogen are replaced by 2 molecules of phenyl, and 3 equivalents of hydrogen by 1 molecule of formyl, 1 equivalent of hydrogen remaining unreplaced. Its formation is expressed by the equation

$$4\underbrace{(C_{12}\,H_7\,N)}_{\substack{\text{Phenylamine} \\ \text{aniline.}}} + \underbrace{C_2\,HCl_3}_{\substack{\text{Terchloride} \\ \text{of formyl} \\ \text{(chloroform).}}} = \underbrace{C_{26}\,H_{12}\,N_2\,HCl}_{\substack{\text{Hydrochlorate of} \\ \text{diphenyl-formyl-} \\ \text{diamine.}}} + 2\underbrace{(C_{12}\,H_7\,N_1\,HCl)}_{\substack{\text{Hydrochlorate of} \\ \text{phenylamine.}}}$$

As seen from this equation, the new base, although unmistakeably corresponding to 2 molecules of ammonia, like many other poly-ammonias, is monoacid.

The analysis of the hydrochlorate leads in fact to the formula

$$C_{26} H_{12} N_2, HCl.$$

The platinum-salt contains

$$C_{26} H_{12} N_2, HCl, PtCl_2.$$

The new derivative of aniline undergoes several remarkable changes which require further elucidation.

III. "Note sur un Organe, placé dans le Cordon Spermatique, et dont l'existence n'a pas été signalée par les Anatomistes." Par F. Giraldès, Professeur Agrégé de la Faculté de Médecine, &c. Communicated by Sir Benjamin Brodie, Bart. Received April 19, 1858.

(Abstract.)

In this paper the author gives an account of certain tubular and vesicular bodies which he has discovered in the spermatic cord, and which he considers to be the remains of the Wolffian body of the embryo.

The structures in question, which he proposes to designate collectively by the term "Corps Innominé," form a small group situated behind the tunica vaginalis, between that membrane and the spermatic vessels, and extending usually from the head of the epididymis as high as the point where the membrane is reflected forwards from the cord; sometimes, however, reaching much higher up, or, on the other hand, being more concentrated in the neighbourhood of the epididymis.

The "Corps Innominé" is found in the new-born infant, and in a more or less modified condition at all later periods of life; it has also been met with in the lower animals. To facilitate its detection and examination, the author has found it advantageous previously to render the surrounding tissues transparent, by macerating the spermatic cord in an acid solution, for which purpose he recommends the use of tartaric or citric acid in the infant, and of dilute nitric acid in the adult.

After this preparation minute whitish specks make their appearance in the situation above mentioned, which, when examined with the microscope, are found to be produced by small vesicles and convoluted tubes of varied and curious shape, surrounded by a plexus of small blood-vessels, which are distributed on their parietes. The tubes are short, tortuous, and for the most part beset with very unequal-sized and irregular varicose dilatations, and sometimes with short branches ending in rounded swollen extremities. The vesicles are round or oval, like short closed segments of a varicose tube, and generally with irregularly protruding outline. The walls of both vesicles and tubes are formed of a fibrous connective tissue, and lined with epithelium, and they enclose a consistent but clear fluid, holding in suspension epithelium-particles and transparent granules. This description applies to the condition of the structures in question as found in the infant, and up to the age of from six to ten years; after this they begin to be atrophied, so that although still present in the adult, they are usually less marked; but, on the other hand, they then sometimes contain a more consistent liquid, and are occasionally dilated, so as to constitute certain forms of cysts known to occur in the spermatic cord.

As already stated, the author regards the "Corps Innominé" as formed by the atrophied remains of the Wolffian body, and therefore comparable to the so-called "organ of Rosenmüller," which is found in the broad ligament of the uterus, and represents the vestiges of the Wolffian body in the female; and as certain vesicular productions in the broad ligament may take their rise from these remnants, so the author, as before remarked, has satisfied himself that the origin of some of the cysts of the spermatic cord may be traced to dilatation of the tubular elements of the "Corps Innominé" in the male.

The paper is illustrated by drawings representing the objects in their natural size and situation, and also as seen under the microscope.

IV. "On *Chondrosteus*, an Extinct Genus of Fish allied to the Sturionidæ." By Sir PHILIP DE MALPAS GREY EGERTON, Bart., F.R.S. Received April 20, 1858.

(Abstract.)

Before the conclusion of his great work on Fossil Fishes, Professor Agassiz recognized in some fragmentary remains found in the lias strata at Lyme Regis, unmistakeable evidence of the existence, at that period of the earth's deposition, of a representative of the still extant family of the Sturgeons. To this extinct fish he assigned the name *Chondrosteus*. The author of the present memoir has been enabled, by the examination of numerous specimens more recently acquired, to describe in some detail the external features of the fish, and the structural peculiarities of those portions of the exo- and endo-skeleton which have been preserved. In the former respect the fossil differs from the recent sturgeon in having a shorter and deeper trunk, in the greater vertical expanse and wider divergence of the lobes of the caudal fin, in the median position of the dorsal fin, and in the absence of dermal plates on the back, belly, and flanks. Before describing the cranial anatomy, the author points out certain homologies between the head-plates of the recent sturgeon and the epicranial bones of the teleostean fishes, more especially with reference to the parietals, mastoids and frontals; and explains that these conclusions have resulted from the examination of the inner table of skull, where the relative position and proportions of the component plates are constant, however much the outer or dermal layer may vary.

The remainder of the memoir is devoted to detailed descriptions of such parts as are preserved in the several specimens; and the author concludes by stating as the result of his investigations, that Professor Agassiz was right in referring the liasic fish to the Sturionidæ; that in some respects it evidenced a transitional form between the latter family and the more typical ganoids; that its food was similar to that of the existing members of the family, but that it was procured in a tranquil sea, rather than in the tumultuous waters frequented by sturgeons of the present time.

The Society then adjourned to Thursday, May 20.

May 20, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "On the Resistance of Tubes to collapse." By WILLIAM FAIRBAIRN, Esq., C.E., F.R.S. &c. Received April 21, 1858.

(Abstract.)

The object kept in view by the author of these researches was to determine the law which governed the resistance of cylindrical tubes to external uniform pressure. The anomalous condition in which these constructions have been placed in reference to the internal flues of boilers, and the frequent fatal accidents from explosions produced by collapse, have imperatively called for inquiry into the causes which have led to these unfortunate results. Ever since the first introduction of the steam engine as improved by Watt, and especially since the increased demand for its construction, and its application to almost every branch of industry and every system of transit, the consideration of all circumstances which may affect its economy and security, has become of vast public importance.

During the more early period which followed its first introduction, the form of boiler and its powers of resistance to strain, were considerations of much less importance than at present. Then the force of steam, or the pressure under which it was generated, was only about one-eighth, and in some cases less than a sixteenth of what it now is. Besides, the fertile genius of Watt had provided against accident, by a self-acting apparatus, which regulated not only the pressure, but the supply of water to the boiler. Since that time a total change has taken place in the construction and working of the steam engine; and boilers which were perfectly safe at 7 lbs. upon the square inch, are absolutely inadequate for generating steam at 40 lbs. to 50 lbs. on the square inch. This being the case, it follows that every precaution becomes urgently necessary which may serve to increase the strength, and equalize the resisting power of vessels containing

an element of such potent influence, and yet so essential to the comforts and enjoyments of civilized life.

Entertaining these views, the author goes on to say, that hitherto it has been considered an axiom in boiler engineering, that a cylindrical tube, placed in the position of an internal flue, is equally strong in every part when subjected to uniform external pressure; the length not affecting the strength of a flue so placed. This rule is, however, only true when applied to tubes of infinitely great length, and it is very far from true when the length of the tube does not exceed certain limits, and when the ends are retained in form by being riveted to the boiler, and thus prevented from yielding to external pressure. These facts were fully demonstrated by the experiments related in the Paper, which, for obvious reasons were conducted under circumstances as nearly as possible analogous to those now in actual operation upon a larger scale. With this view, a large and powerful cylinder, 8 feet long, and 2 feet in diameter, was prepared for the reception of the tubes; and being acted upon by hydraulic pressure, collapse was produced, and the results recorded, as fully explained in the Paper. It will suffice here to state the more important conclusions derived from the investigation, which fell under the following heads: viz.—1st, the strength of tubes as affected by length; 2nd, the strength of tubes as affected by diameter; lastly, the strength of tubes as affected by thickness of metal.

1. On the first head, *the strength as affected by length*, the results are conclusive and interesting. Within the limits of from 1 foot 6 inches to about 10 feet in length, it is found that the strength of tubes similar in every other respect, and supported at the ends by rigid rings, varies inversely as the length, as may be seen from the following results obtained with 4-inch tubes.

Resistance of 4-inch tubes to collapse.

Diameter. Inches.	Thickness of Plates. Inches.	Length. Inches.	Collapsing Pressure. lbs. per square inch.
4	·043	19	137
4	·043	60	43
4	·043	40	65

The remarkable differences in the resisting pressure of the above similar tubes will be at once apparent, and it will be found by calcu-

lation that they follow the law of inverse proportion, the same as those of larger dimensions, the strengths diminishing as the lengths are increased.

The same law of resistance is maintained in 6-inch tubes, giving, for a tube 30 inches long, 55 lbs., and for one 59 inches long only 32 lbs. on the square inch, as the pressure of collapse. Again, in 8-inch tubes we have, in a long series of experiments, 32 lbs. per square inch in a tube 39 inches long, and 39 lbs. in one 30 inches long. In the same manner all the experiments on tubes of 10 and 12, and up to 18 inches in diameter may be compared, and the law of resistance is in like manner shown to hold true in every case. Discrepancies to a certain extent do certainly occur; but they are comparatively small, and, as they appear to follow no law, are evidently to be accounted for from defects in the construction of the tubes inseparable from such a mode of research.

2. *The strength as affected by the diameter.*

A precisely similar law is found to hold in relation to the diameter. Tubes similar in other respects vary in their resistance to collapse inversely as their diameters; and with a view of testing this law, we may place the calculated pressure beside that derived from experiment, as under:—

Resistance of tubes to collapse 5 feet long.

Diameter. Inches.	By Experiment. lbs. per square inch.	By Calculation. lbs. per square inch.	Variation. lbs.
4	43·0		
6	32·0	28·6	−3·4
8	20·8	21·3	+0·7
10	16·0	17·2	+1·2
12	12·5	14·3	+1·8

The above variations are slight when compared with the resisting powers of the tubes; they are doubtless caused by the varying rigidity of the iron, or by defects in the cylindrical form. Similar results follow in the experiments on tubes 2 feet 6 inches long; and although some slight variations occur, they are nevertheless not more than might have been anticipated within the ordinary limits of error.

3. *The strength of tubes as affected by thickness.*

In these experiments it is found that the tubes vary in strength according to a certain power in the thickness; the index of which,

taken from the mean of the experiments, is 2·19, or rather higher than the square.

Combining the above laws into a general expression, we have, as the formula for the strength of tubes subjected to a uniform external force,

$$P = C \times \frac{k^{2 \cdot 19}}{L \times D},$$

where P is the collapsing pressure, k the thickness of the plates, L the length of the tube, which should not be less than 1·5, or greater than 10 feet; D the diameter, and C a constant to be determined by the experiments. For tubes of greater length than those above specified, a variable quantity, dependent upon the length, must be introduced; and the value of this has yet to be determined. For ordinary practical calculations the following formula will probably, however, afford the needful accuracy:—

$$P = 806{,}300 \times \frac{k^2}{L \times D}.$$

Thus, for example, take a tube or boiler-flue 10 feet long, 2 feet diameter, and composed of plates $\frac{1}{4}$ inch thick; and the collapsing pressure will be

$$P = 806{,}300 \times \frac{\cdot 25^2}{10 \times 24} = 210 \text{ lbs.}$$

per square inch or nearly so.

Some experiments have also been made upon elliptical tubes; and the results have been most conclusive as to the weakness of such forms in resisting external pressure. No tubes in use for boilers should ever be made of that form.

With regard to cylindrical internal flues, the experiments indicate the necessity of an important modification of the ordinary mode of construction, in order to render them secure at the high pressures to which they are now almost constantly subjected. If we take a boiler of the ordinary construction, 30 feet long, 7 feet in diameter, and with one or more flues 3 feet diameter, it will be found that the outer shell or envelope is from three to three and a half times as strong in resisting an internal force as the cylindrical flues which have to resist the same external force. This being the case, it is evident that the excess of strength in those parts of the vessel subjected to tension, *is*

actually of no use so long as the elements of weakness are present in the other parts subjected to compression.

To remedy these defects, it is proposed to rivet strong rings of angle iron at intervals along the flue—thus practically reducing its length, or in other words increasing its strength to a uniformity with that of the exterior shell. This alteration in the existing mode of construction is so simple, and yet so effective, that its adoption may be confidently recommended to the attention of all those interested in the construction of vessels so important to the success of our manufacturing system, and yet fraught with such potent elements of disaster when unscientifically constructed or improperly managed.

II. "On some Remarkable Relations which obtain among the Roots of the Four Squares into which a Number may be divided, as compared with the corresponding Roots of certain other Numbers." By the Rt. Hon. Sir FREDERICK POLLOCK, F.R.S., Lord Chief Baron. Received April 26, 1858.

(Abstract.)

The first property of numbers mentioned in this paper is best illustrated by an example—

$$13^2 = 169 \qquad 15^2 = 225.$$

These odd numbers may be divided into 4 squares, and the roots may be so arranged that they will have this relation to each other: the middle roots will be the same, and the exterior roots will be, the one 2 more, the other 2 less than the corresponding roots of the other. Putting the roots below the number and comparing them, the result is obvious.

169	225
0,3,4,12	−2,3,4,14
−2,4,7,10	−4,4,7,12
−4,5,8,8	−6,5,8,10
−6,4,9,6	−8,4,9,8

Each of the numbers may be divided into 4 squares in 4 different ways with this result, that the two middle roots of each are the same; and as to the exterior roots they differ by 2, the one being 2 more

the other 2 less than the corresponding roots of the other. So comparing 15^2 with 17^2,

225	**289**
4,3,10,10	6,3,10,12
6,5,10,8	8,5,10,10

the result is the same; and it is true of all adjoining odd squares. The paper contains a Table of odd squares (up to 27^2), compared in this manner with the odd square immediately before it and after it.

It is then shown that the same property continues when the 2 odd squares are increased by any the same even number—

49	**81**
0,2,3,6	−2,2,3,8
51	**83**
−1,3,4,5	−3,3,4,7

and also when they are (within certain limits) diminished by the same even number. It is then shown that a similar property belongs to the even squares $+1$, as seen below,

16 + 1 = 17	**36 + 1 = 37**
+1,0,0,4	−1,0,0.6
0,2,2,3	−2,2,2,5
37	**65**
−1,2,4,4	−3,2,4,6

and also to these numbers increased or decreased by the same even number.

If, instead of comparing the adjoining squares, the alternate squares be compared, a similar result is obtained; the middle roots are the same, the exterior roots differ by 4 instead of 2.

The proof of this property depends upon a general property of all odd numbers and upon a general theorem.

The property of odd numbers is this, that every odd number can be divided into 4 squares in such manner that 2 of the roots will be equal, 2 will differ by 1, 2 will differ by 2, &c. as far as the number is capable (from its magnitude) of having roots large enough to form the difference required: thus in the No. 39 there cannot be roots having a difference of 9; for the least number that can have that difference is $41=4^2+5^2$ and -4 and 5 differ by 9; but $39=1^2+2^2+3^2+5^2$, and the difference between -3 and 5 is 8; and the numbers 1, 2, 3, 5, either as positive or negative, give all the differences up to 8, but they do not give 2 equal roots: 39 is however divisible

into $1^2+1^2+1^2+6^2$, and then the equal roots are discovered. It is proved from the known properties of numbers that this property of having 2 roots whose difference will be 0, 1, 2, 3, &c., as far as is possible, belongs to all odd numbers. A new symbol is then suggested to represent the division of a number into 4 squares, such that 2 of the roots will have a given difference, and these are made the exterior roots; the number or figure denoting the difference is placed above on the left hand: thus $^{4}25$ denotes 0, 0, 3, 4 or —2, 1, 4, 2; $^{1}25$ denotes 1, 2, 4, 2.

The general theorem is this:—If any odd number of odd numbers be in arithmetical progression (4 being the common difference), as 9, 13, 17, 21, 25, then if the common difference be assumed as the index of the difference of roots to the middle term, and the higher terms in the series have as indices (4+1), (4+2), &c. in succession, and the lower terms have as indices (4—1), (4—2), &c., the series with its indices will be

$$\begin{array}{ccccc} 2 & 3 & 4 & 5 & 6 \\ 9 & 13 & 17 & 21 & 25 \end{array}$$

and if the terms less than the middle term be divided into 4 squares with exterior roots having the differences indicated by their respective indices thus,

$$\begin{array}{ccc} 2 & 3 & 4 \\ 9 & 13 & 17 \\ 0,1,2,2 & -1,2,2,2 & -2,0,3,2 \end{array}$$

then the terms greater than the middle term will have this relation to the terms less than the middle term; the terms equidistant from the middle term will have their middle roots the same, and the differences of the exterior roots will increase; those nearest the middle term will have a difference of 1, the next 2, and so on, thus:

$$\begin{array}{ccccc} 2 & 3 & 4 & 5 & 6 \\ 9 & 13 & 17 & 21 & 25 \\ 0,1,2,2 & -1,2,2,2 & -2,0,3,2 & -2,2,2,3 & -2,1,2,4 \end{array}$$

An algebraic proof is then given as to a series whose middle term is n and common difference p; and as n may be odd or even, and p also, and the index of differences may be *minus* as well as *plus*, the theorem applies frequently to even numbers, but not universally. The following example is given of the theorem applied to 17 terms of a series whose first term is 25, and common difference 1:—

−7	−6	−5	−4	−3	−2	−1	0	1
25	**26**	**27**	**28**	**29**	**30**	**31**	**32**	**33**
4,0,0,−3	5,0,0,−1	5,1,1,0	2,2,4−2	5,0,0,2	3,2,4,1	3,3,3,2	0,4,4,0	0,4,4,1
9	8	7	6	5	4	3	2	
41	**40**	**39**	**38**	**37**	**36**	**35**	**34**	
−4,0,0,5	−2,0,0,6	−1,1,1,6	−3,2,4,3	1,0,0,6	0,2,4,4	1,3,3,4	−1,4,4,1	

Comparing the terms above with the terms below, it is manifest the terms of the series are divisible into 4 squares whose roots conform to the law of the theorem. It is then shown that the odd squares, and also all the numbers mentioned in the beginning of the paper, can be made terms in an arithmetic series, and will therefore have the property stated. It is then suggested that the properties of numbers stated in the paper may have been in some form a portion of the mysterious properties of numbers by which Fermat announced he could prove his celebrated theorem of the polygonal numbers.

A Postscript was added, dated 20th May, which is here given entire.

Since this paper was sent to the Society, some other theorems of a similar kind have occurred to me, in which the terms of a series (*not* arithmetical of the 1st order) have a similar relation with regard to the roots of the 4 squares of which they may be composed, that is, those which are equidistant from the middle, or the middle term (according as the number of terms is even or odd), have the middle roots the same, and the exterior roots have an arithmetical relation to each other (varying with the distance from the centre), viz. the one being less and the other greater by the same quantity.

Thus, if any number of terms (exceeding 3) of either of the 2 series above-mentioned (viz. 1, 3, 9, 19, &c., or 1, 5, 13, 25, &c.), and, beginning with the first term, the differences be added "*inverso ordine,*" a new series will be obtained possessing the property in question; thus the first 7 terms of the 1st series are, $\overset{0}{1}$, $\overset{2}{3}$, $\overset{4}{9}$, $\overset{6}{19}$, $\overset{8}{33}$, $\overset{10}{51}$, $\overset{12}{73}$; the differences are, 2, 6, 10, 14, 18, 22; if the differences be added "*inverso ordine,*" the series becomes 1, 23, 41, 55, 65,

71, 73, each term of which may be divided into 4 squares, whose roots will be as follows :—

0	2	4	6	8	10	12
1	**23**	**41**	**55**	**65**	**71**	**73**
0,0,1,0	+1,2,3,3	0,3,4,4	−3,1,6,3	−2,3,4,6	−3,2,3,7	−6,0,1,6
		+2,0,1,6	−1,2,5,5	0,0,1,8		
			+1,1,2,7			

Here there is a middle term, all the terms equidistant from it have the same middle roots, the terms next to the middle term have the exterior roots, the one 2 less, the other 2 more, those next but one 4 less and 4 more, and the extreme terms 1 and 73 have their exterior roots one 6 less and the other 6 more than the corresponding roots of the other.

If 8 terms of the series be taken as 1, 3, 9, 19, 33, 51, 73, 99, and the differences be added "*inverso ordine,*" the series becomes
0 2 4 6
1, 27, 49, 67, &c., the terms of which divided into 4 squares, so that the differences of the exterior roots may correspond with the index, will be

0	2	4	6	8	10	12	14
1	**27**	**49**	**67**	**81**	**91**	**97**	**99**
0,0,1,0	−1,3,4,1	−2,4,5,2	−3,0,7,3	−4,0,7,4	−5,4,5,5	−6,3,4,6	−7,0,1,7
		+2,0,3,6	−1,4,5,5	−2,4,5,6	−1,0,3,9		
			+1,1,4,7	0,1,4,8			

Here there is no middle term ; the terms equidistant from the centre have the same middle roots, while the differences between the exterior roots increase as the numbers 1, 3, 5, 7.

The other series, 1, 5, 13, 25, &c., gives a similar result. If the differences of the first 7 terms be added "*inverso ordine,*" the new series, with its indices and the roots of the 4 squares which compose each term, will be as follows :—

1	3	5	7	9	11	13
1	**25**	**45**	**61**	**73**	**81**	**85**
0,0,0,1	0,0,4,3	−2,4,4,3	−3,0,6,4	−4,4,4,5	−4,0,4,7	−6,0,0,7
	−1,2,4,2	0,2,4,5	−2,4,4,5	−2,2,4,7	−5,2,4,6	
		+1,2,2,6		−1,2,2,8		

Here there is a middle term ; the equidistant terms have the same middle roots, the exterior roots are (next to the middle term) the

one 2 more, the other 2 less, and the differences increase by 2 as the terms are more distant from the middle term.

If the number of terms be 8, the resulting series with its indices and roots will be—

1	3	5	7	9	11	13	15
1	**29**	**53**	**73**	**89**	**101**	**109**	**113**
0,0,0,1	0,2,4,3	−2,2,6,3	−1,0,6,6	−2,0,6,7	−5,2,6,6	−5,2,4,8	−7,0,0,8
	+2,0,0,5	−1,0,6,4	+1,2,2,8	0,2,2,9	−4,0,6,7	−3,0,0,10	
		+1,0,4,6			−2,0,4,9		
		+2,0,0,7			−1,0,0,10		

and the differences of the exterior roots will be 1, 3, 5, 7. The reason of these results is, that the equidistant terms are always equal to the original corresponding term in the series increased by the same number.

Thus, in the first example, if to the terms

1, 3, 9, 19, 33, 51, 73 there be added
0, 20, 32, 36, 32, 20, 0, the result is
1, 23, 41, 55, 65, 71, 73, which is the

series with the differences added "*inverso ordine.*" And in the last example, if to

1, 5, 13, 25, 41, 61, 85, 113 there be added
0, 24, 40, 48, 48, 40, 24, 0, the result is
1, 29, 53, 73, 89, 101, 109, 113, that is,

the series arising from the differences being added "*inverso ordine.*"

It is worthy of observation that these numbers, 0, 24, 40, 48, 48, 40, 24, 0, which, added to the first 8 terms, produce a series identical with the result of the differences being added "*inverso ordine,*" have the same effect upon any other consecutive 8 terms of the series. Take the 2nd term as the 1st of 8 terms—

5,	**13,**	**25,**	**41,**	**61,**	**85,**	**113,**	**145,**	to these add
8	12	16	20	24	28	32		
0	**24**	**40**	**48**	**48**	**40**	**24**	**0,**	the result is
5,	**37,**	**65,**	**89,**	**109,**	**125,**	**137,**	**145,**	
32	28	24	20	16	12	8		

in which last series the differences are reversed or added "*inverso ordine.*" The appropriate roots of these numbers are—

3	5	7	9	11	13	15	17
5	**37**	**65**	**89**	**109**	**125**	**137**	**145**
−1,0,0,2	−1,2,4,4	−3,2,6,4	−2,0,6,7	−3,0,6,8	−6,2,6,7	−6,2,4,9	−8,0,0,9
	+1,0,0,6	−2,0,6,5	0,2,2,9	−1,2,2,10	−5,0,6,8	−4,0,0,11	
		0,0,4,7			−3,0,4,10		
		+1,0,0,8			−2,0,0,11		

which may be immediately obtained from the former series, the middle roots being the same; and the exterior roots, one of them one less, the other one more. In this way any consecutive 8 terms, with the differences reversed, may be each divided into 4 squares throughout the whole series.

And the same is true of 4 terms, 5 terms, or any number of terms. If 3 terms have the differences reversed, the numbers added are—

			0	4	0		
If 4 terms			0	8	8	0	
If 5 terms		0	12	16	12	0	
If 6 terms	0	16	24	24	16	0	
If 7 terms	0	20	32	36	32	20	0
&c.			&c.		&c.		

The law under which these numbers are formed is obvious enough. The same numbers exactly are to be added to the other series (1, 3, 9, 19, &c.) to produce the same result.

	0	1	2	3	4	5	6	7
If the 2 series be blended together, thus	1	1	3	5	9	13	19	25,

&c., the differences will be 2, 2, 4, 4, 6, 6, 8, 8, &c.; and if an odd number of terms be taken (so as to begin and end with a number from the same series), and the differences be added *inverso ordine*, a similar result occurs. Take 11 terms.

1 3 5 9 13 19 25 33 41 51 61, and add the differences "*inverso ordine,*" the series becomes, with its indices and roots,—

1	2	3	4	5		
1	**11**	**21**	**29**	**37**		
0,0,0,1	+1,0,1,3	−1,0,4,2	0,2,3,4	+1,0,0,6	6	
				−1,2,4,4	**43**	(middle term.)
11	10	9	8	7	−3,3,4,3	
61	**59**	**57**	**53**	**49**		
−5,0,0,6	−3,0,1,7	−4,0,4,5	−2,2,3,6	0,0,0,7		
				−2,2,4,5		

In this case the additions are 0, 8, 16, 20, 24, 24, 20, 16, 8, 0; and if these be added to any other consecutive 11 terms (the 1st term having an *odd* index), they produce the same effect as if the differences were reversed; and the resulting numbers have the property of the terms equidistant from the centre, being connected by their roots, having the relation so frequently mentioned. It may be further remarked, that the numbers produced by reversing the differences are the initial numbers from which, by adding 2, 2, 4, 4, &c., 61 may be formed of the squares, which make the differences of its exterior roots 10, 9, 8, &c.

1 2 3 4 5 6 7 8 9 10
11 13 15 19 23 29 35 43 51 61
2 2 4 4 6 6 8 8 10
0,0,3,1 0,0,3,2 −1,0,3,3 −2,0,3,4 −3,0,3,5 −4,0,36

1 2 3 4 5 6 7 8 9
21 23 25 29 33 39 45 53 61
2 2 4 4 6 6 8 8
0,2,4,1 −4,2,4,5

and so of all the others.

The matter referred to in this Postscript tends to strengthen the suggestion already made, that the properties of numbers referred to are connected with the mysterious and abstruse properties to which Fermat referred as enabling him to prove the theorem he announced of the Polygonal Numbers.

III. "Observations on the Mer de Glace."—Part I. By John Tyndall, Ph.D., F.R.S. &c. Received June 10, 1858.

(Abstract.)

In this paper the author communicates the first part of a series of observations upon the Mer de Glace, made during a residence of six weeks at the Montanvert last summer *. He corroborates the laws regarding the swifter flow of the central portions of the ice-stream, first established by Prof. Forbes, and shows how the velocity changes as the width of the glacier varies. The Mer de Glace moves through a valley which twice turns a convex curvature to the east, and once to the west. The points of swiftest motion at these curves are found to be not central, but thrown to that side

* During the whole of which period he was most ably assisted by his friend Mr. T. A. Hirst.

of the valley towards which the glacier turns its convex curvature. It has hitherto been believed that the portion of the Mer de Glace derived from the Glacier du Géant moved swiftest. The author shows that the tributaries which form the Mer de Glace lose their individuality in the trunk stream, the latter flowing as if it proceeded from a single source. The point of maximum motion is sometimes on the eastern, sometimes on the western side of a line drawn along the centre of the glacier, the change from side to side depending upon the curvature of the valley. The locus of the point of swiftest motion in a glacier which moves through a sinuous valley, is exactly similar to that of a river moving through a sinuous channel; it forms a curve more deeply sinuous than the valley itself, and crosses the centre of the valley at each point of contrary flexure.

A rare opportunity of determining the comparative velocities of a glacier at its surface and close to its bed, was furnished by a precipice of ice 140 feet in height, which was exposed near the Tacul. Three stakes were fixed in this precipice, one at the top, the other near the bottom, and a third in the face of the precipice at a height of nearly 40 feet above the bottom; the velocities of the three stakes were found to be 6 inches, 4·59 inches, and 2·56 inches per day; thus furnishing additional proof of the correctness of the law first predicted by Prof. Forbes, and confirmed subsequently by his own observations and those of M. Martins.

Attention is drawn to the immense exertion of force necessary to drive the glacier through the neck of the valley at Trelaporte. The sum of the width of the three tributaries of the Mer de Glace before they mutually act upon each other is 2597 yards. All these are squeezed through a gorge at Trelaporte, measuring 893 yards across. The Glacier de Léchaud, which, before its junction with the Talèfre, possesses a width of upwards of 37 chains, is squeezed at Trelaporte to a driblet less than 4 chains wide.

As a natural consequence of this obstacle in front, the Glacier du Géant is generally in a state of longitudinal compression. But the mechanical meaning of this term must be that the points behind are incessantly advancing upon those in front,—the distance between two points upon the axis of the glacier being thus gradually diminished. The daily motion of these points upon the axis of this glacier was determined, which led to the remarkable result, that a section of the glacier 1000 yards in length is shortened by the thrust to which it

is subjected, at the rate of 8 inches a day; which, if the action were uniform at all seasons, would amount to 240 feet a year.

The author also describes a system of remarkable bands of white ice which he observed to sweep across the Glacier du Géant in the same general direction as the "dirt-bands." He traced these bands to the following origin. The streams which flow upon the glacier in its upper portions, cut deep channels in the ice; these channels, during the winter, become gorged with snow, which is afterwards compressed to a highly resistant white ice during the descent of the glacier. Similar bands were also observed in a most remarkable position at the base of the Talèfre cascade; and here also he found the glacier crumpled into a series of protuberances, one consequence of the crumpling being the production of a "backward" as well as a forward "dip" of the veined structure. A similar system of protuberances occurs on the Glacier du Géant; and here also the author found the same structure dipping backward as well as forward. The cause of the crumpling is assigned in the paper.

The physical quality in virtue of which ice is able to change its form in the manner indicated by the observations is next inquired into; and it is shown by measurements carried out upon the glacier itself, that no quality which could with propriety be called viscosity is possessed by glacier ice. All the phenomena appear to be reconciled by reference to the fragility of ice at a temperature of 32° F., coupled with its power of regelation. The intestine motion of the parts is no doubt aided to some extent by the partial liquefaction of the ice by pressure; a fact first publicly pointed out by Mr. James Thomson, and proved experimentally by Prof. William Thomson and the author.

June 3, 1858.

The LORD WROTTESLEY, President, in the Chair.

The Annual General Meeting for the Election of Fellows was held this day,

The Statutes respecting the election of Fellows having been read, Sir George Back and Mr. Gwynn Jeffreys were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

Thomas Graham Balfour, M.D.	David Livingstone, LL.D.
Edward Mounier Boxer, Captain R.A.	John Lubbock, Esq.
Frederick Currey, Esq.	Henry Darwin Rogers, LL.D.
David Forbes, Esq.	William Scovell Savory, Esq., M.B.
Alfred Baring Garrod, M.D.	Warington Wilkinson Smyth, Esq.
William Henry Harvey, M.D.	Lieut.-Col. Andrew Scott Waugh, B.E.
The Rev. Samuel Haughton.	Thomas Williams, M.D.
Henry Hennessy, Esq.	

June 10, 1858.

The LORD WROTTESLEY, President, in the Chair.

The following Gentlemen were admitted into the Society:—

Thomas Graham Balfour, M.D.; Frederick Currey, Esq.; Alfred Baring Garrod, M.D.; John Lubbock, Esq.; William Scovell Savory, Esq., M.B.; Warington Wilkinson Smyth, Esq.; Thomas Williams, M.D.

The following communications were read:—

I. "On the formation of Continuous Tabular Masses of Stony Lava on steep slopes; with Remarks on the Mode of Origin of Mount Etna, and the Theory of 'Craters of Elevation.'" By Sir CHARLES LYELL, F.R.S., &c. Received June 10, 1858.

(Abstract.)

The question whether lava can consolidate on a steep slope, so as to form strata of stony and compact rock, inclined at angles of from 10° to more than 30°, has of late years acquired considerable importance, because geologists of high authority have affirmed that lavas which congeal on a declivity exceeding 5° or 6° are never continuous and solid, but are entirely composed of scoriaceous and fragmentary materials. From the law thus supposed to govern the consolidation of melted matter of volcanic origin, it has been logically inferred that all great volcanic mountains owe their conical form principally to upheaval or to a force acting from below and exerting an

upward and outward pressure on beds originally horizontal or nearly horizontal. For in all such mountains there are found to exist some stony layers dipping at 10°, 15°, 25°, or even higher angles; and according to the assumed law, such an inclined position of the beds must have been acquired subsequently to their origin.

After giving a brief sketch of the controversy respecting "Craters of Elevation," the author describes the results of his recent visit (October, 1857) to Mount Etna, in company with Signor Gaetano G. Gemmellaro, and his discovery there of modern lavas, some of known date, which have formed continuous beds of compact stone on slopes of 15°, 36°, 38°, and, in the case of the lava of 1852, more than 40°. The thickness of these tabular layers varies from 1½ foot to 26 feet; and their planes of stratification are parallel to those of the overlying and underlying scoriæ which form part of the same currents. The most striking examples of this phenomenon were met with—1st, at Aci Reale; 2ndly, in the ravine called the Cava Grande near Milo, where a section of the lava of 1689 is obtained; 3rdly, in the precipice at the head of the Val di Calanna, in the lava of 1852–53; and 4thly, at a great height above the sea near the base of the Montagnuola.

Sir C. Lyell then alludes to the extraordinary changes which had taken place in the scenery of the Valley of Calanna and the Val del Bove since his former visit to Mount Etna in 1828—changes effected by the eruption of 1852–53, one of the greatest recorded in history. A brief account is given, extracted from contemporary narratives and illustrated by a map, compiled with the assistance of Dr. Giuseppe Gemmellaro, of the course taken in 1852–53 by various streams of lava, some of them six miles in length, flowing during nine successive months from the head of the Val del Bove to the suburbs of Zafarana and Milo. The present aspect of this lava-field, parts of it still hot and emitting vapour, and the numerous longitudinal ridges and furrows on its surface are described. As to the origin of these superficial inequalities, the author inquires whether they may be due to the flowing of lava in subterranean tunnels, or whether they be anticlinal and synclinal folds caused by fresh streams pouring over preceding and half-consolidated ones, so that these last may be bent and crumpled by the newly superimposed weight, like soft yielding ground on which a railway embankment has been made. The cas-

cade of the lava of 1852, descending a precipitous declivity 500 feet high, called the Salto della Giumenta, and the stony character of the layers which encrust the steep slope at angles of more than 35° and even 45°, are commented upon. This lava has overflowed that of 1819, which congealed on the same precipice; and it is shown that in such cases the junction-lines separating two successive currents must be obliterated, the bottom scoriæ of the newer dovetailing into the upper scoriæ of the older current.

The structure of the nucleus of Etna, as exhibited in sections in the Val del Bove, is next treated of, and the doctrine of a double axis is deduced from the varying dip of the beds. The strata of trachyte and trachytic agglomerate in the Serra Giannicola seen at the base of the lofty precipice at the head of the Val del Bove are inclined at angles of 20° to 30° N.W., *i. e.* towards the present central axis of eruption. Other strata to the eastwards (as in the hill of Zoccolaro) dip in an opposite direction, or S.E., while, in a great part of the north and south escarpments of the Val del Bove, the beds dip N.E. or N., and S.E. or S. respectively. There is, therefore, a quâquâversal dip away from some point situated in the centre of the area called the Piano di Trifoglietto. Here a permanent axis of eruption may have existed for ages in the earlier history of Etna, for which the name of the axis of Trifoglietto is proposed, while the modern centre of eruption, that now in activity, may be called the axis of Mongibello. The two axes, which are three miles distant the one from the other, are illustrated by an ideal section through the whole of Etna, passing from west to east through the Val del Bove, or from Bronte to Zafarana. Touching the relative age of the two cones, it is suggested that a portion only of that of Mongibello may be newer than the cone of Trifoglietto. The latter, when it became dormant, was entirely overwhelmed and buried under the upper and more modern lavas of the greater cone. This doctrine of two centres, originally hinted at by the late Mario Gemmellaro, had been worked out (unknown to Sir C. Lyell at the time of his visit) by Baron Sartorius v. Waltershausen, and has been since supported in the fifth and sixth parts of his great work called "The Atlas of Etna" both by arguments founded on the quâquâversal dip of the beds as above explained, and by the convergence of a certain class of greenstone dikes towards the axis of Trifoglietto. Von Waltershausen has also

shown that the superior lavas and volcanic formations crowning the precipices at the head of the Val del Bove, from the Serra Giannicola to the Rocca del Corvo, inclusive, are unconformable to the highly inclined beds in the lower half of the same precipice, the superior beds being horizontal, or, when inclined, dipping in such directions as would imply that they slope away from the higher parts of Mongibello.

According to Sir C. Lyell, the alleged discontinuity between the older and modern products of Etna is, in truth, only partial, and almost confined to that flank of the mountain, where its physical geography has been altered by three causes: 1st, the interference of the two foci of eruption (Trifoglietto and Mongibello); 2ndly, the truncation of the cone of Mongibello; and 3rdly, the formation of the Val del Bove. The truncation of the mountain here alluded to is proved by the remains of the upper portion of a cone, traceable at intervals around the borders of an elevated platform between 9000 and 10,000 feet high. These remains bear the same relation to the highest and active cone, nearly in the centre of the platform, which Somma bears to Vesuvius. The manner in which the north and south escarpments of the Val del Bove diminish in altitude as they trend eastward from the high platform, is appealed to as showing that the great lateral valley had no existence till after the time when Mongibello had attained its fullest development and height.

The double axis of Etna is then compared to the twofold axis of the island of Madeira, as inferred from observations made in 1854 by M. Hartung and the author. In that island the principal chain of volcanic vents, running east and west, and 30 miles long, attains at one point a height of 6000 feet. Parallel to it, at the distance of two miles, a shorter and lower, secondary chain once existed, but was afterwards overflowed and buried to a great depth by lavas issuing from the higher and dominant chain. The space between the two axes, like the space which separated the two cones of Etna, has been filled up with lavas in part horizontal. On the north side of Madeira, as probably on the west side of Etna, where no secondary centre of eruption interfered with the slope of the volcanic formations, and where the order of their succession and superposition is uninterrupted, there occur, both in Madeira and Etna, deep crateriform valleys (the Curral and the Val del Bove) intersecting the products of the two axes of eruption.

In concluding this part of his memoir, Sir C. Lyell observes, that the admission of a double axis, as explained by him, is irreconcileable with the hypothesis of "craters of elevation;" for it implies that, in the cone-making process, the force of upheaval merely plays a subordinate part. One cone of eruption, he says, may envelope and bury an adjoining cone of eruption; but it is obviously impossible that one cone of upheaval should mantle round and overwhelm another cone of upheaval.

An attempt is then made to estimate the proportional amount of inclination which may be due to upheaval in those parts of the central nucleus of Etna where the dip is too great to be ascribed exclusively to the original steepness of the flanks of the cone. The highest dip seen by the author was in the rock of Musarra, where some of the strata, consisting of scoriæ with a few intercalated lavas, are inclined at 47°. Some masses of agglomerate and beds of lava in the Serra del Solfizio were also seen inclined at angles exceeding 40°. Some of these instances are believed to be exceptional and due to local disturbance; others may have an intimate connexion with the abundance of fissures, often of great width, filled with lava, for such *dikes* are much more frequent near the original centres of eruption than at points remote from them. The injection of so much liquid matter into countless rents may imply the gradual tumefaction and distension of the volcanic mass, and may have been attended by the tilting of the beds, causing them to slope away at steeper angles than before, from the axis of eruption. But instead of ascribing to this mechanical force, as many have done, nearly all, or about four-fifths of the whole dip, Sir C. Lyell considers that about one-fifth may, with more probability, be assigned as the effect of such movements.

The alleged parallelism and uniformity of thickness in the volcanic beds of the Val del Bove, when traced over wide areas, is next considered, and the author remarks that neither in the northern nor southern escarpments of the great valley, could he and his companion verify the existence of such parallelism. Examples of a marked deviation from it are given, both in cliffs seen from a distance, and in others which were closely inspected, even in cases where these last, when viewed from far off, appeared to contain regular and parallel strata.

The direction and position of the dikes in the Val del Bove is then spoken of, both in reference to the two ancient centres of eruption, and to the question of the altered inclination of the intersected beds. In regard to the arrangement also of the lateral cones of eruption, the question is entertained, whether they are disposed in linear zones, or are in some degree independent of the great centre of Mongibello.

The origin of the Val del Bove has been variously ascribed to engulfment, explosion, and aqueous erosion. Admitting the probable influence of the two first causes, the author calls attention to the positive evidence in favour of aqueous denudation afforded by the accumulation of alluvium in the low country at the eastern base of Etna between the Val del Bove and the sea. This rudely stratified deposit, 150 feet thick and several miles in length and breadth, contains at Giarre, Mangano, Riposto and other places, fragments, both rounded and angular, of all the rocks, ancient and modern, occurring in the escarpments of the Val del Bove, and it implies the continuance there for ages of powerful aqueous erosion. The alluvium of Giarre is therefore supposed to bear the same relation to the Val del Bove that the conglomerate of the Barranco de las Angustias bears to the Caldera of Palma in the Canaries; and those two crater-like valleys, as well as the Curral of Madeira, are believed to have been shaped out in great part by running water. But to render this possible, the suspension, for a long period, of the outpouring of lava on the eastern flank of Etna must be assumed.

The author fully coincides in the generally received opinion that the accessible parts of Etna are of subaërial origin, and refers to some fossil leaves presented to him by MM. Gravina and Tornabene, of Catania, as well as to others collected by himself *in situ*, from the volcanic tuffs of Fasano and Licatia, which have been determined by Prof. Heer to belong to terrestrial plants, of the genera Myrtle, Laurel, and Pistachio, now living in Sicily. These tuffs, together with the general mass of Etna, repose on marine strata of the newer Pliocene period, in which 150 species of shells, nearly nine-tenths of them identical with species now existing in the Mediterranean, have been found. A very modern marine breccia, with shells of living species extending to the height of thirty feet on the coast along the eastern base of Etna, was pointed out to the author by Signor G. G. Gemmellaro near Trezza, and in the Island of the Cyclops. The

same formation has been traced together with lithodomous perforations by Dr. Carlo Gemmellaro and Baron v. Waltershausen along the sea-shore as far north as Taormina, beyond the volcanic region of Etna. From these and other data enlarged upon in the memoir, Sir C. Lyell concludes, first, that a very high antiquity must be assigned to the successive eruptions of Etna, each phase of its volcanic energy, as well as the excavation of the Val del Bove, having occupied a lapse of ages compared to which the historical period is brief and insignificant; and secondly, that the growth of the whole mountain must nevertheless be referred, geologically, to the more modern part of the latest Tertiary epoch.

II. "On some Thermo-dynamic Properties of Solids." By J. P. Joule, LL.D., F.R.S. &c. Received April 22, 1858.

(Abstract.)

A *résumé* of the greater part of this paper has already appeared in the 'Proceedings' for January 29, June 18, and November 26, 1857. The author has since examined the expansion by heat of wood cut across the grain, which, as well as that cut in the direction of the fibre, he finds to be increased by tension and decreased by moisture. When a sufficient quantity of water has been absorbed the expansibility by heat ceases, and wood is *contracted* in each direction by rise of temperature. Nevertheless, when wood, saturated with water, is weighed in water of different temperatures, the result shows cubical expansion of the substance of the wood by heat. The inference drawn by the author from these facts is, that the contraction of the dimensions of wet wood is owing to the action of heat in diminishing the force of capillary attraction, and that thus the walls of the minute cells and tubes of the woody structure are partially relieved from a force which thrusts them asunder, a small quantity of water exuding at the same time. In the case of wet wood which contracts by heat, he finds, in accordance with Professor Thomson's formula, that a rise of temperature is produced by the application of tension. In conformity with the deductions of the same philosopher, the author has also been able to detect experimentally the minute quantity of heat absorbed, in bending or twisting an elastic spring, arising from the diminution of the elastic force of metals with a rise of temperature.

III. "On the Thermal Effect of drawing out a Film of Liquid." By Professor WILLIAM THOMSON, F.R.S., &c., being extract of two Letters to J. P. JOULE, LL.D., F.R.S., dated February 2 and 3, 1858. Received April 30, 1858.

A very novel application of Carnot's cycle has just occurred to me in consequence of looking this morning into Waterston's paper on Capillary Attraction, in the January Number of the Philosophical Magazine. Let T be the contractile force of the surface (by which in Dr. Thomas Young's theory the resultant effect of cohesion on a liquid mass of varying form is represented), so that, if Π be the atmospheric pressure, the pressure of air within a bubble of the liquid of radius r, shall be $\frac{4T}{r}+\Pi$. Then if a bubble be blown from the end of a tube (as in blowing soap-bubbles), the work spent, per unit of augmentation of the area of one side of the film, will be equal to 2T.

Now since liquids stand to different heights in capillary tubes at different temperatures, and generally to less heights at the higher temperatures, T must vary, and in general decrease, as the temperature rises, for one and the same liquid. If T and T′ denote the values of the capillary tension at temperatures t and t' of our absolute scale, we shall have $2(T-T')$ of mechanical work gained, in allowing a bubble on the end of a tube to collapse so as to lose a unit of area at the temperature t and blowing it up again to its original dimensions after having raised its temperature to t'. If $t'-t$ be infinitely small, and be denoted by $\mathfrak{T}$, the gain of work may be expressed by

$$-\frac{2dT}{dt}\times\mathfrak{T};$$

and by using Carnot's principle as modified for the Dynamical Theory, in the usual manner, we find that there must be an absorption of heat at the high temperature, and an evolution of heat at the low temperature; amounting to quantities differing from one another by

$$\frac{1}{J}\times\frac{-2dT}{dt}\times\mathfrak{T},$$

and each infinitely nearly equal to the mechanical equivalent of this

difference, divided by Carnot's function, which is $\frac{J}{t}$, if the temperature is measured on our absolute scale. Hence if a film such as a soap-bubble be enlarged, its area being augmented in the ratio of 1 to m, it experiences a cooling effect, to an amount calculable by finding the lowering of temperature produced by removing a quantity of heat equal to

$$m\frac{t}{J}\times\frac{-dT}{dt},$$

from an equal mass of liquid unchanged in form.

For water $T=2{\cdot}96$ gr. per lineal inch.

Work per square inch spent in drawing out a film $=5{\cdot}92$, say 6 grains, $\frac{dT}{dt}=\frac{1}{550}T$, or thereabouts.

Suppose $\frac{t'}{J}=\frac{300}{1390\times 12}$, then the quantity of heat to be removed, to produce the cooling effect, per square inch of surface of augmentation of film will be $\frac{1^{\circ}}{5100}$. Suppose, then, 1 grain of water to be drawn out to a film of 16 square inches, the cooling effect will be $\frac{16}{5100}$ of a degree Centigrade, or about $\frac{1^{\circ}}{320}$. The work spent in drawing it out is $16\times 6=96$ grains and is equivalent to a heating effect of $\frac{96}{12\times 1390}=\frac{1}{174}$. Hence the total energy (reckoned in heat) of the matter is increased $\frac{1}{174}+\frac{1}{320}$ of a degree Centigrade, when it is drawn out to 16 square inches.

IV. "On the Logocyclic curve, and the geometrical origin of Logarithms." By the Rev. J. Booth, LL.D., F.R.S. Received April 15, 1858.

In a paper read before the Mathematical Section of the British Association during its meeting at Cheltenham in 1856, and which was printed among the reports for that year, I developed at some length the geometrical origin of logarithms, and showed that a trigonometry exists as well for the parabola as for the circle, and that every formula in the latter may be translated into another which shall indicate some property of parabolic arcs analogous to that from which it has been derived. I showed, moreover, that the

whole theory of logarithms was founded on a basis as purely geometrical as the trigonometry of the circle, and I pointed out how the imaginary expressions in the latter—such as De Moivre's theorem—have real counterparts in the trigonometry of the parabola. As the principle of duality is of the widest application in geometrical investigation, and as every property of circumscribed space has its dual, it would be strange if the dual of circular trigonometry had no existence.

In the paper to which I have referred, I showed how, by the help of certain arbitrary lines drawn about the parabola, numbers and their logarithms might be exhibited. But as there was something conventional in this representation, I was not quite satisfied with the construction. I suspected that the geometrical theory of logarithms was just as little conventional as the trigonometry of the circle. With this view I have again lately considered the whole subject, and by the help of a new curve, which I have called the *Logocyclic Curve*, from the similarity of many of its properties to those of the circle, and from its use in representing numbers and *their logarithms*, I have succeeded in exhibiting the whole theory of logarithms in a geometrical form as complete as it is comprehensive, and simple as it is beautiful.

The properties of the Logocyclic curve, a curve which hitherto has escaped discussion, if not discovery, are many of them as remarkable as they are simple, and I will now proceed to mention some of the most obvious.

The equations of the Logocyclic curve.

Let r and θ be the polar coordinates of the curve, then its equation is

$$r=a(\sec\theta\pm\tan\theta). \qquad (1)$$

Since $$\sec\theta=\frac{r}{x},\ \tan\theta=\frac{y}{x},\ r=\sqrt{x^2+y^2},$$

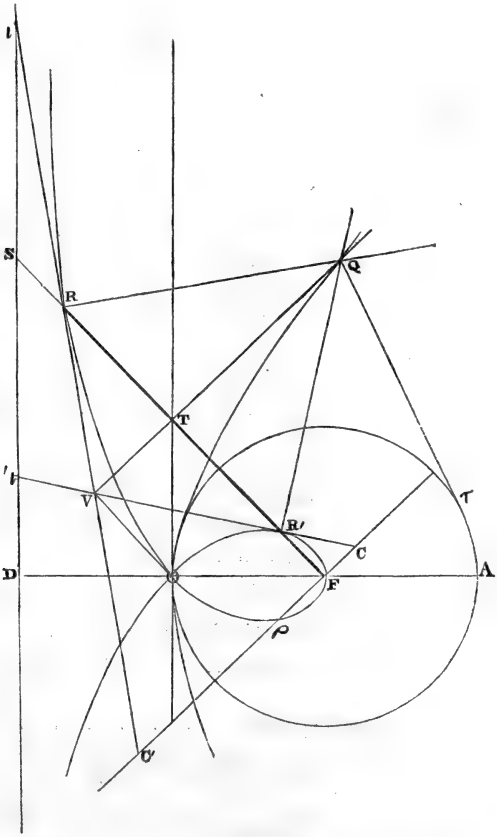
we get for the equation of the curve in rectangular coordinates,

$$y^2(2a-x)=x(a-x)^2. \qquad (2)$$

or $$(x^2+y^2)(2a-x)=a^2x.$$

Let the focus F of the parabola, whose vertical focal distance FO is a, be taken as the origin, the axis and parameter of the parabola being the axes of x and y; let the vertical tangent OT and the directrix of the parabola DS be drawn, then the following properties of this curve may easily be shown :—

I. The directrix DS of the parabola is an asymptote of the curve, which has two infinite branches along this line above and below the axis. These branches intersect at right angles on the axis of the parabola in its vertex, and form an oval or loop between this point and the focus of the parabola F.



II. The radius vector $r=a(\sec\theta\pm\tan\theta)$, drawn from the focus F of the parabola, cuts the loop and one of the infinite branches in the points R_1 and R, so that

$$FR=a(\sec\theta+\tan\theta),\text{ and } FR_1=a(\sec\theta-\tan\theta).$$

The product of the segments of this secant, as in the circle, is constant and equal to a^2.

These points may be named *Reciprocal Points.*

III. O being the vertex of the parabola, in which the two branches of the curve cut at right angles, the angle ROR′ is always a right angle.

IV. Through the points R and R_1 let normals be drawn to the Logocyclic curve, they will meet the parabola in the same point Q, and the parabolic arc OQ diminished by the protangent QT is the logarithm of the number FR or of its reciprocal FR_1.

Q, the intersection on the parabola of the normals, drawn to the Logocyclic curve at the *reciprocal* points R, R_1 may be called the Logarithmic point of the line FR.

V. The normals RQ and R_1Q are equal. This is also a property of the circle.

VI. At the points R and R_1, the *reciprocal points,* let tangents be drawn to the curve and meet in V. The tangents RV and R_1V are equal, and they are equally inclined to the chord RR_1. This is another property of the circle.

VII. The locus of V, the intersection of tangents drawn to the curve through the *reciprocal points,* is the Cissoid of Diocles, a curve discovered in the Schools of Alexandria.

VIII. Since the line VQ is at right angles to the line VO, we have this new property of that old curve, that a line drawn through any point of a Cissoid at right angles to the radius vector drawn to the cusp of the curve envelopes a parabola.

IX. The directrix of the parabola is the asymptote of the Cissoid, and its cusp is at O the vertex of the parabola.

X. The sum of the ordinates of the reciprocal points R and R_1 is equal to the ordinate of the logarithmic point Q on the parabola, and the sum of the distances of the points R and R_1 from the asymptote DS is constant and equal to $2a$.

XI. The sum of the products of the abscissæ and ordinates of the *reciprocal points* on the curve is constant, or

$$x_{\prime}x_{\prime\prime}+y_{\prime}y_{\prime\prime}=a^2.$$

XII. The distances of any point Q on a parabola from its focus and directrix are equal. We may generalize this well-known theorem, and say the distances are equal of any point on a parabola from its Logocyclic curve, measured along the two normals to this curve drawn through the point Q.

XIII. As the tangent to a parabola makes equal angles with the focal radius vector and a perpendicular on the directrix, so also this tangent makes equal angles with the normals to the Logocyclic curve drawn through this point of contact.

XIV. The two reciprocal points R and R_1 on the Logocyclic, the logarithmic point Q on the parabola, and the intersection V of the tangents to the Logocyclic which meet on the Cissoid lie on the circumference of a circle, or in other words, any two reciprocal points on the curve, the intersection of the normals at these points and the intersection of the tangents at the same points lie in the circumference of a circle.

XV. Through the two *reciprocal points* R and R_1 a circle may be drawn touching the axis at O, and having its centre at T the intersection of the vertical tangent OT, with the radius vector FR. This property leads to a simple construction of the curve by points.

XVI. The tangent to the curve at either of the *reciprocal points* makes with the radius vector through this point an angle whose tangent is equal to the cosine of the inclination of the radius vector to the axis.

XVII. The sum of the polar subtangents FC and $FC_{,}$ belonging to the reciprocal points R and $R_{,}$ is constant, and equal to $2a$.

XVIII. The sum of the reciprocals of the polar subnormals belonging to two reciprocal points is constant, and equal to $\frac{2}{a}$.

XIX. The lengths of the tangents to the curve between any two reciprocal points and the asymptote are equal, or $Rt = R_{,}t_{,}$, and $St = St_{,} = a \sec \theta$.

XX. The products of these two tangents t, and the perpendiculars $p, p_{,}$ from the focus F upon them, is constant, or $t^2 pp_{,} = a^4$.

XXI. Let ρ and $\rho_{,}$ be the reciprocals of the radii of curvature of the Logocyclic curve at the points R and $R_{,}$; and let $\rho_{,,}$ be the reciprocal of the radius of curvature of the parabola at a point through which the normal makes the angle ψ with the axis, then

$$\rho + \rho_{,} = 8\rho_{,,}$$

ψ and θ are connected by the condition $\tan \psi = \cos \theta$. See (XV.)

XXII. The Logocyclic curve is the envelope of all the circles whose centres range along the parabola, and whose radii are successively equal to $\sqrt{f^2 - a^2}$, f being the distance of the centre, Q, of the

circle from the focus of the parabola. This follows from an inspection of the figure; but it may easily be proved as an independent theorem.

XXIII. The vertical tangent OT bisects all the cords of the Logocyclic passing through F. The angles VRT or $VR_1T=\psi$ and θ or OFR are so connected that $\tan\psi=\cos\theta$. Hence the maximum ordinate of the loop is found by making $\psi=\theta$, or $\tan\theta=\cos\theta$, or $\sin\theta=\frac{\sqrt{5}-1}{2}$.

XXIV. If any point Q on the parabola be taken as centre, and through the two corresponding reciprocal points on the Logocyclic curve a circle be drawn, it will always cut at right angles the fixed circle whose centre is the focus F and radius $=a$.

XXV. The Logocyclic curve, like the circle, is its own *inverse* curve.

On the Area and rectification of the Logocyclic Curve.

XXVI. The area of the loop will be found equal to

$$2a^2-\frac{a^2\pi}{2},$$

and the area between the asymptote and infinite branch will be

$$2a^2+\frac{a^2\pi}{2}.$$

Hence the entire area of the curve is $4a^2$, or equal to the square of the semiparameter of the parabola. It is obvious that the area of the half-loop is equal to the difference between the square of a and the quadrant of a circle inscribed in it, while that of the infinite branch is equal to the square of a with the quadrant added to it. Hence also the difference of the two areas is equal to $a^2\pi$, that is to the area of a circle whose radius is a.

The length of the half-loop, together with that of the infinite branch of the curve, is equal to the infinite branch of an equilateral hyperbola whose transverse semiaxis is $\sqrt{2}a$, and to the half-loop of the lemniscate which belongs to that equilateral hyperbola. Hence the entire length of the Logocyclic is equal to the entire continuous arc of an equilateral hyperbola whose transverse axis is $\sqrt{2}a$, and to the loop of the lemniscate which belongs to that branch; while the difference between the lengths of the loop and the infinite branch is equal to an arc of the parabola together with a right line.

To represent Numbers and their Logarithms by the Logocyclic Curve and its Conjugate Parabola.

XXVII. A parabola whose focal vertical distance is a or 1 being drawn, and also its Logocyclic curve, let a radius vector be drawn to the latter equal to the given number n. Then $n = \sec\theta + \tan\theta$.

Let this line meet the vertical tangent in T, the parabolic arc OQ$-$QT is the logarithm of n.

It is clear that the infinite branch of the curve from $+\infty$ to O will give radii vectores of every magnitude from ∞ to 1, and parabolic arcs from ∞ to 0; hence, while the numbers range from ∞ to 1, the parabolic arcs range from ∞ to 0. When the number lies between 1 and 0, the radius vector representing it is drawn *below* the axis; its extremity will be found on the loop, and the corresponding arc of the parabola will be negative, hence the logarithm of a positive number is equal to the logarithm of its reciprocal, with the sign changed; for the magnitude of the parabolic arc depends on θ, and θ is the same in $\sec\theta + \tan\theta$, as in its reciprocal $\sec\theta - \tan\theta$.

Hence while the infinite branch of the Logocyclic curve from $+\infty$ through R, O, ρ, to F, may by its radii vectores represent all positive numbers from $+\infty$ to $+0$, the *two* infinite branches of the parabola will be used in representing the logarithms of positive numbers from $+\infty$ to $+0$; that is, the upper or positive branch of the parabola will be "used up" in representing the logarithms of positive numbers from $+\infty$ to $+1$, and the lower or negative branch of the parabola in representing the logarithms of positive fractional numbers from $+1$ to $+0$. Hence there is no construction by which we can represent negative numbers or their logarithms, therefore such numbers can have no logarithms.

Let radii vectores be drawn from F to the Logocyclic curve equal to $e, e^2, e^3, e^4 \ldots\ldots e^n$, then these lines will meet the tangent to the vertex of the parabola in the points T, $T_{,}$, $T_{,,} \ldots\ldots T_n$; and tangents being drawn from these points, touching the parabola in Q, $Q_{,}$, $Q_{,,}$, $Q_{,,,}$, Q_n, the logarithms of these numbers will be

$$OQ - QT = 1,\ \ OQ_{,} - Q_{,}T_{,} = 2,\ \ OQ_{,,} - Q_{,,}T_{,,} = 3, \ldots$$
$$OQ_n - Q_nT_n = (n+1);$$

hence the logarithms of e, e^2 e^3, e^n are 1, 2, 3, ... n.

In like manner we should find the logs of $e, e^{\frac{1}{2}}, e^{\frac{1}{3}}\ e^{\frac{1}{4}} \ldots\ldots e^{\frac{1}{n}}$ to be $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4} \ldots\ldots \frac{1}{n}$.

Let a series of radii vectores be drawn from the point F to the Logocyclic curve in geometrical progression, and let them be

$(\sec\theta+\tan\theta)$, $(\sec\theta+\tan\theta)^2$, $(\sec\theta+\tan\theta)^3 \ldots\ldots (\sec\theta+\tan\theta)^n$,

meeting the vertical tangent to the parabola in the points $T_{,}$, $T_{,,}$, $T_{,,,} \ldots\ldots T_n$, and let the tangents drawn from the points $T_{,}$, $T_{,,}$, &c. touch the parabola in the points $Q_{,}$, $Q_{,,}$, $Q_{,,,}$, $\ldots\ldots Q_n$; let the difference between the first parabolic arc and its *protangent* be δ, then we shall have $OQ_{,}-Q_{,}T_{,}=\delta$, $OQ_{,,}-Q_{,,}T_{,,}=2\delta$, $OQ_{,,,}-Q_{,,,}T_{,,,}=3\delta$ $OQ^n-Q_nT_n=n\delta$.

Or while numbers increase in geometrical progression, their logarithms increase in arithmetical progression.

As every number whose logarithm is to be exhibited must be put under the form $\sec\theta+\tan\theta$, which is of the form $1+x$, since the limiting value of $\sec\theta$ is 1, we discover the reason why in developing the logarithm of a number the number itself must be put under the form $1+x$, or some derivative from it, and not simply under that of x.

If we equate $\sec\theta+\tan\theta$ with $1+x$, we shall find

$$x=\frac{2\tan\frac{\theta}{2}}{1-\tan\frac{\theta}{2}}. \quad \text{Let } u=\tan\frac{\theta}{2},$$

then $n=\sec\theta+\tan\theta=1+x=\frac{1+u}{1-u}$, which is another familiar form under which a number is put, whose logarithm is to be developed in a series.

Let θ be the angle which the line $(\sec\theta+\tan\theta)$ makes with the axis, and let $\theta_{,}$ $\theta_{,,}$ $\theta_{,,,}$ be the angles which the lines $(\sec\theta+\tan\theta)^2$, $(\sec\theta+\tan\theta)^3 \ldots\ldots (\sec\theta+\tan\theta)^n$ make with the same axes, then

$$\begin{aligned}
\theta\ &=\theta \\
\theta_{,}\ &=\theta\perp\theta \\
\theta_{,,}&=\theta\perp\theta\perp\theta \\
&\ldots \\
\theta_n&=\theta\perp\theta\perp\theta\perp\theta \ldots\ldots \text{to } n \text{ terms.}
\end{aligned}$$

The *definitions* of the symbols $\perp$, $\top$, which I call logarithmic or parabolic plus and minus, are given in the paper referred to.

Hence

$$(\sec\theta+\tan\theta)^n=\sec(\theta\perp\theta\perp\theta\ldots.n)+\tan(\theta\perp\theta\perp\theta\perp\theta\ldots.\text{ to }n).$$

Change $\sec\theta$ into $\cos\phi$, $\tan\theta$ into $\sqrt{-1}\sin\phi$, and $\perp$ into $+$, then we shall have

$$(\cos\phi+\sqrt{-1}\sin\phi)^n=\cos n\phi+\sqrt{-1}\sin n\phi.$$

Hence De Moivre's theorem, which represents an imaginary property of the circle, has its counterpart in a real property of the parabola.

The base of the Neperian system e is that particular radius vector drawn to the Logocyclic which gives the difference between the corresponding parabolic arc and its *protangent* equal to the focal vertical distance of the parabola. Let ϵ be the angle which this radius vector makes with the axis. Then

$$e=\sec\epsilon+\tan\epsilon.$$
$$e=2\cdot718281828.$$

Hence also as the angles θ, $\theta+\theta$, $\theta+\theta+\theta$, ... $n\theta$ give circular arcs which increase in arithmetical progression, the angles θ, $\theta\perp\theta$, $\theta\perp\theta\perp\theta$, &c. give parabolic arcs, whose excesses over their *protangents* increase in arithmetical progression.

If the lines $(\sec\theta+\tan\theta)$, $(\sec\theta+\tan\theta)^2$, $(\sec\theta+\tan\theta)^3$, &c. were drawn, making *equal* angles with each other, and therefore multiples of θ with the axis, instead of the angles θ, $\theta\perp\theta$, $\theta\perp\theta\perp\theta$, the locus of the extremities would be the logarithmic spiral instead of the Logocyclic curve.

The spiral of Archimedes may also be used as a means of exhibiting logarithms or parabolic arcs. For the equation of the spiral being $r=a\theta$, the arc of the spiral is given by the equation $s=a\int\sqrt{1+\theta^2}d\theta$.

Let $a\theta=a\tan\phi$, then making the necessary substitutions, $s=a\int\frac{d\phi}{\cos^3\phi}$; but this is the expression for an arc of a parabola measured from the vertex to a point whose ordinate is $2a\tan\phi=2r$, that is to twice the radius vector of the spiral.

Hence, if a line be drawn along the vertical tangent of a parabola equal to twice the radius vector of the spiral, and a line be drawn parallel to the base, it will determine the parabolic arc corresponding to the radius vector of the spiral.

It is not difficult to construct a trammel which shall describe the

Logocyclic Curve by a continuous motion; and a very ingenious instrument has been contrived by Mr. Henry Johnson of Crutched Friars, to describe the spiral of Archimedes, which is as simple as it is effective.

June 17, 1858.

The LORD WROTTESLEY, President, in the Chair.

The Earl Granville, Professor Hennessy, and the Rev. Samuel Haughton were admitted into the Society.

In accordance with notice given at the last Meeting, the Earl of Rosse proposed the Right Hon. Sir John Pakington, Bart. for election and immediate ballot.

The Ballot having been taken, Sir John Pakington was declared duly elected.

The following communications were read:—

I. "On the Problem of Three Bodies." By CHARLES JAMES HARGREAVE, LL.D., F.R.S. Received May 3, 1858.

(Abstract.)

The author states that the principal object of this memoir is to set forth two new methods of treating the dynamical equations by processes of variation of elements, differing from the ordinary processes of this nature principally in this particular, that the variations are represented in explicit terms of the elements themselves and of the time, and not through the medium of partial differential coefficients. It has been his object to render the processes as elementary as possible; and to preserve them in a vigorous form, by postponing all attempts at approximation until the formulæ are actually applied to practical problems. The applications given in the paper comprise the circular and spherical pendulums, and the planetary and lunar theories, and a special theorem as to the movement of the plane of a planet's motion under the influence of several other planets.

The original normal problem which is taken as the basis, is that

of motion about a fixed centre of force, where the force is directly as the distance; or, in other words, the system of equations not exceeding three in number, of the form

$$x'' + n^2 x = 0;$$

whose solutions are represented under the form

$$x = \lambda_a\, a \cos(nt+\rho) + \mu_a\, b \sin(nt+\rho),$$
$$y = \lambda_b\, a \cos(nt+\rho) + \mu_b\, b \sin(nt+\rho),$$
$$z = \lambda_c\, a \cos(nt+\rho) + \mu_c\, b \sin(nt+\rho);$$

where

$$\lambda_a = \cos\phi\cos\psi - \sin\phi\sin\psi\cos l,$$
$$\lambda_b = \cos\phi\sin\psi + \sin\phi\cos\psi\cos l,$$
$$\lambda_c = \sin\phi\sin l;$$

$$\mu_a = -\sin\phi\cos\psi - \cos\phi\sin\psi\cos l,$$
$$\mu_b = -\sin\phi\sin\psi + \cos\phi\cos\psi\cos l,$$
$$\mu_c = \cos\phi\sin l;$$

to which are afterwards added,

$$\nu_a = \sin\psi\sin l,$$
$$\nu_b = -\cos\psi\sin l,$$
$$\nu_c = \cos l.$$

These are the equations of an ellipse whose centre is at the force, and situated in a plane inclined at the angle l to the plane of $x\,y$, and the longitude of whose node is ψ; and ϕ is the angular distance of the major axis of the ellipse from the node; a and b are the semi-axes of the ellipse; and ρ is the angular distance, from the major axis, of the zero-point of the motion, measured on the circle described on the major axis. A uniform motion around the circle represents the place of the body by the corresponding point on the ellipse, where it is cut by a perpendicular dropped on the major axis.

If the force be not situated at the origin, but at the point (X, Y, Z), we have merely to substitute $x-\mathrm{X}$ for x, &c. in the above equations of motion and solutions.

Applying the method of tangential variation to the system

$$x'' + \frac{\mu}{r^3}x = 0, \text{ \&c.},$$

we perceive that this system admits of complete solution in finite terms, leading in fact to the usual theory of elliptical motion. Taking this system, therefore, as a normal system, the author proceeds to

deduce the formulæ for the variation of the elements of this system, in order to arrive at the solution of the system

$$x'' + \frac{\mu}{r^3}x = P_x, \text{ \&c.}$$

The elements which have been selected, for reasons fully explained in the paper, are l and ψ, whose meanings are already known; A and $N\tau$ denoting respectively the mean distance, and the longitude of the epoch measured in the plane of the tangential ellipse as it exists at the time t, and measured from the node at that time; and e and ϖ denoting respectively the eccentricity of the tangential ellipse, and the longitude of its perihelion measured as above; and it is observed that these are strictly normal elements, according to Professor Donkin's definition of normal elements.

The variations of these elements are then rigorously found, and are expressed as follows:—Denote

$$\cos\psi\, P_x + \sin\psi\, P_y \text{ by the symbol } P_\xi,$$

and

$$\cos l\,(\cos\psi P_x - \sin\psi P_y) + P_z \sin l \text{ by the symbol } P_\eta;$$

and let

$$-P_\xi \sin\theta + P_\eta \cos\theta = P_{\xi,\theta}; \quad -P_\xi \sin\varpi + P_\eta \cos\varpi = P_{\xi,\omega};$$
$$P_\xi \cos\theta + P_\eta \sin\theta = P_{\eta,\theta}; \quad P_\xi \cos\varpi + P_\eta \sin\varpi = P_{\eta,\omega};$$

then

$$\delta l = \frac{r\cos\theta}{NA^2(1-e^2)^{\frac{1}{2}}}(P_z),$$

$$\sin l\,\delta\psi = \frac{r\sin\theta}{NA^2(1-e^2)^{\frac{1}{2}}}(P_z),$$

$$\delta e = \frac{r}{NA^2(1-e^2)^{\frac{1}{2}}}\left\{(\cos(\theta-\varpi)+e)\,P_{\xi,\theta} + (1+e\cos(\theta-\varpi))\,P_{\xi,\omega}\right\},$$

$$e(\delta\varpi + \cos l\,\delta\psi) = \frac{r}{NA^2(1-e^2)^{\frac{1}{2}}}\left\{\sin(\theta-\varpi)\,P_{\xi,\theta} - (1+e\cos(\theta-\varpi))\,P_{\eta,\omega}\right\},$$

$$\delta A = \frac{2}{N(1-e^2)^{\frac{1}{2}}}(P_{\xi,\theta} + e\,P_{\xi,\omega}),$$

$$\delta(N(t+\tau)) = \frac{r}{NA^2e}\left\{-\sin(\theta-\varpi)\,P_{\xi,\theta} + (1+e\cos(\theta-\varpi))\,P_{\eta,\omega} - 2eP_{\eta,\theta}\right\};$$

which are capable of being expanded in terms of the elements, and t by means of the ordinary expressions for r, θ, and $\theta-\varpi$ in terms of the same quantities. The values of the elements at the time t being supposed to be found, by the integration of these formulæ, in terms

of t, and their initial values, are to be substituted in the ordinary expressions for the coordinates, so as to obtain their values at the time t.

The author exhibits the application of the preceding formulæ to certain simple examples, and then proceeds to apply them to the planetary theory. For two planets (distinguished by the suffixes 2 and 3) supposed to move in the same plane, the following are the rigorous expressions for the variations. Let α_2 and α_3 be the ratio of the mass of each planet to that of the central body. Let P denote the cube of $r_3 \div r_{23}$, and let $(P-1) \sin(\theta_3-\theta_2)$ be called Q, and $(P-1)\cos(\theta_3-\theta_2)-\frac{r_2}{r_3}P$ be called R; then

$$\delta e_2=\alpha_3\frac{N_2A_2r_2}{r_3^2(1-e_2^2)^{\frac{1}{2}}}\Big\{(\cos(\theta_2-\varpi_2)\,(2+e_2\cos(\theta_2-\varpi_2))+e_2)\,Q+\sin(\theta_2-\varpi_2)\,(1+e_2\cos(\theta_2-\varpi_2))R\Big\},$$

$$e_2\,\delta\varpi_2=\alpha_3\frac{N_2A_2r_2}{r_3^2(1-e_2^2)^{\frac{1}{2}}}\Big\{\sin(\theta_2-\varpi_2)\,(2+e_2\cos(\theta_2-\varpi_2))\,Q-\cos(\theta_2-\varpi_2)\,(1+e_2\cos(\theta_2-\varpi_2))\,R\Big\},$$

$$\delta A_2=2\alpha_3\frac{N_2A_2^3}{r_3^2(1-e_2^2)^{\frac{1}{2}}}\Big\{(1+e_2\cos(\theta_2-\varpi_2))\,Q+e_2\sin(\theta_2-\varpi_2)R\Big\},$$

$$\delta(N(t+\tau))=-(1-e^2)^{\frac{1}{2}}\,\delta\varpi-2\alpha_3\frac{N_2A_2r_2}{r_3^2}R.$$

From these formulæ, the secular variations of the elements are obtained without difficulty; and a new method of integrating the equations for the variations of the eccentricity and longitude of perihelion is given.

The author then enters upon a minute examination of the mathematical character of secular variations, and their bearing upon the methods of approximation to which the problem of three bodies has given rise. It is pointed out that the disturbance finally effected through the medium of a secular variation is not of the order of the disturbing force, or rather of the ratio of the disturbing force to the central force; but that it may remain precisely the same, though this ratio should be diminished or increased without limit. The difference affects not the aggregate amount of deviation or disturbance caused, but the time in which this aggregate amount is produced. If we consider the undisturbed problem of two planets about a sun

as representing motion in two planes inclined to each other at the angle I, and in ellipses having eccentricities e_2 and e_3, it is shown that, no matter how small or how large may be the disturbing force produced on each orbit by the other planet, the aggregate amount of disturbance of the planet m_2 is of the order of the quantities I and e_2, and that of the planet m_3, of the order of I and e_3. From considerations of this nature, which are dwelt upon at length in the memoir, the author concludes that the ordinary direct methods of solution by approximation, being based upon the erroneous assumption that the variations of the coordinates are of the order of the disturbing force, are not, in a mathematical sense, legitimate processes; and that, in the planetary theory, they produce results practically true only on account of the minuteness of the disturbing forces, and the consequent great length of the secular periods; and that, in the lunar theory, their failure is made evident, in consequence of the comparatively large magnitude of the disturbing force, and the consequent rapidity with which the elements of the moon's orbit pass through their secular periods.

The formulæ for the variations of the elements are then applied to the lunar theory; and some of the integrations are effected by means of a lemma containing the solution of the differential equation

$$\frac{d\phi}{dt}=\mathrm{F}\cos(pt-q\phi)$$

(where F, p and q are numerical coefficients), in the form

$$\cos(pt-q\phi)=\frac{q\mathrm{F}+p\cos\mathrm{M}t}{p+q\mathrm{F}\cos\mathrm{M}t},\ \mathrm{M}\text{ being }\left(1-\frac{q^2\mathrm{F}^2}{p^2}\right)^{\frac{1}{2}}p.$$

By this method, the total motion of the moon's perigee, as well as the coefficients of the evection, are fully obtained in the first instance, without the necessity of any second approximation; and the usual difficulty as to the movement of the perigee does not present itself. The motion of the node, and the evection in latitude, are correctly obtained in a similar manner.

This part of the memoir is concluded by an extension of the general formulæ for the tangential variation of elements to the case in which we suppose the constant μ to become variable, the result being to add to each variation a term involving $\delta\mu$.

The third part of the Paper contains the development of the

method of osculating variation, before briefly described; from which are deduced the formulæ for the osculating variations of elliptic elements. This method is capable of being applied to the planetary and lunar theories, as well as that of tangential variation; but the advantages of this method did not appear to be such as to justify the actual expansion of the formulæ for these theories. The author, however, shows that with reference to any system of three bodies, the equations of motion for each body naturally assume the form

$$\chi'' + n^2(x - \mathrm{X}) = 0, \text{ \&c.}$$

(being the system solved by this method); and that the X, Y, and Z are absolutely the same for each of the three bodies. This is shown by demonstrating, that at any given moment the three lines which represent the direction of the force acting on each of the three bodies all pass through the same point, which is denominated the centre of force. The coordinates of this common centre of force are,

$$\mathrm{X} = \frac{(23)x, + (31)x_2 + (12)x_3}{(23) + (31) + 12};$$

with similar expressions for Y and Z; (12) being $r_{12} \div m\, m_2\, \chi r_{12}$, χ denoting the law of force, &c. Each body has its own value of n^2; their ratios being denoted by the proportion

$$n_1^2 : n_2^2 : n_3^2 :: \frac{r_{23}}{\chi r_{23}} : \frac{r_{31}}{\chi r_{31}} : \frac{r_{12}}{\chi r_{12}}.$$

The invariable plane of this system of three bodies is then found; and it is shown that the nodes of the three orbits upon this plane are always in a certain relative position, constituting a kind of triangle of equilibrium about the centre of force; resulting, in the limiting case where one of the three bodies is infinitely larger than the other two (or in what is denominated the undisturbed Problem of Three Bodies), in an exact opposition of the two nodes of the orbits of the latter two bodies upon the invariable plane of the system.

The formulæ for the osculating variation of elements are then applied to a system of three bodies, of which one possesses a predominating magnitude, so far as is necessary to determine the movement of the planes of the orbits; and it is readily shown that, if we consider only the first order of the disturbing force, the inclination

of the plane of each orbit to the invariable plane is absolutely constant; and that the two nodes are always in opposition to each other, and move with a uniform angular velocity round the invariable plane.

This theorem is then extended to a system of n bodies moving about a central predominant body; and it is shown that the aggregate effect of the disturbing forces of such a system upon the plane of any one of the bodies can always be represented by stating that its node upon a certain fixed plane revolves with a uniform angular velocity, the plane of the orbit always remaining at the same inclination to the fixed plane. The rate of this angular movement, and the coordinates of the fixed plane upon which the movement takes place, are found by means of formulæ of remarkable simplicity. These three quantities may be ascertained once for all for each planet (viz. the inclination of the fixed plane on which the node moves to any coordinate plane, the longitude of the node of the fixed plane in relation to any coordinate line, and the angular rate of movement of the node of the orbit upon this fixed plane), and, when once ascertained, may be regarded as fixed elements of the planet, from which the position of the plane of its orbit can always be determined without the use of tables.

It is then shown that a system of the form

$$x''+n^2x=P_x, \text{ \&c.},$$

where n^2 and P_x, P_y, and P_z are any variables, may be solved by the same set of final integrals, and the same values of x', y', and z', by supposing the elements a, b, φ, ψ, z, l, and ρ to become variable. These elements are those of an ellipse tangential to the actual curve of motion; and the following formulæ are obtained for their variation:—

Let

$$\begin{aligned}\lambda_a P_x+\lambda_b P_y+\lambda_c P_z&=(P_x),\\ \mu_a P_x+\mu_b P_y+\mu_c P_z&=(P_y),\\ \nu_a P_x+\nu_b P_y+\nu_c P_z&=(P_z);\end{aligned}$$

and let (putting T for $nt+\rho$)

$$\begin{aligned}a\cos\phi\cos T-b\sin\phi\sin T&=\xi,\\ a\sin\phi\cos T+b\cos\phi\sin T&=\eta;\end{aligned}$$

then

$$nab\,\delta l=\xi(P_z),$$

$$nab\sin l\,\delta\psi=\eta(P_z),$$

$$\delta(nab)=a\cos T(P_y)-b\sin T(P_x),$$

$$\tfrac{1}{2}\delta n^2(a^2+b^2)=-n(a\sin T(P_x)-b\cos T(P_y))+r^2nn',$$

$$(a^2-b^2)(\delta\phi+\cos l\,\delta\psi)=\frac{1}{n}(b\cos T(P_x)-a\sin T(P_y))+2ab\sin T\cos T\frac{n'}{n},$$

$$(a^2-b^2)\,\delta(nt+\rho)=-\frac{1}{n}(a\cos T(P_x)-b\sin T(P_y))-(a^2+b^2)\sin T\cos T\frac{n'}{n}.$$

It may be observed that ξ and η are coordinates of the body referred to the plane of the tangential ellipse, and to an axis of ξ coinciding with the node.

This method is denominated the method of Tangential Variation; and it is applied directly to the problem of the circular pendulum, that of the spherical pendulum, and that of the motion of a particle where the force is a function of the distance, and in particular that of elliptical motion, where the law of force is that of the inverse square.

In a subsequent part of the paper it is shown that a system of the form

$$x''+n^2(x-X)=0, \text{ \&c.},$$

where n^2, X, Y, and Z are any variables, may be solved by the same set of final integrals, and the same values of x', y', and z' as those which have been already given as the solutions of the same system when n, X, Y, and Z are constant, by supposing the elements to become variable. In such a case, the elements are those of an ellipse osculating with the actual curve of motion, always of course having its centre at the moveable point (X Y Z). The following formulæ are obtained for the variation of these elements:—

Let

$$\lambda_a X'+\lambda_b Y'+\lambda_c Z'=(X'),$$

$$\mu_a X'+\mu_b Y'+\mu_c Z'=(Y'),$$

$$\nu_a X'+\nu_b Y'+\nu_c Z'=(Z');$$

then

$$nab\,\delta l=(Z')\frac{d\xi}{dt},$$

$$nab\sin l\,\delta\psi=(Z')\frac{d\eta}{dt},$$

$$\delta(nab) = -n((X')\, b \cos T + (Y')\, a \sin T),$$

$$\tfrac{1}{2}\delta(n^2(a^2+b^2)) = -n^2((X')\, a \cos T + (Y')\, b \sin T) + r^2 nn',$$

$$(a^2-b^2)(\delta\phi + \cos l\, \delta\psi) = -((X')\, b \sin T + (Y')\, a \cos T) + 2a\, b \sin T \cos T \frac{n'}{n},$$

$$(a^2-b^2)\, \delta(nt+\rho) = (X')\, a \sin T + (Y')\, b \cos T - (a^2+b^2) \sin T \cos T \frac{n'}{n};$$

in which $\frac{d\xi}{dt}$ and $\frac{d\eta}{dt}$ are the differential coefficients of the expressions for ξ and η, taken explicitly with regard to t.

This method is denominated the method of Osculating Variation.

II. "Description of some Remains of a Gigantic Land-Lizard (*Megalania prisca*, Ow.) from Australia." By Prof. RICHARD OWEN, F.R.S. Received May 13, 1858.

(Abstract.)

The subject of this communication forms part of a collection of fossil remains from Australia, recently acquired by the British Museum, and demonstrates the former existence in that continent of a land-lizard considerably surpassing in bulk the largest species now known. The characters are chiefly derived from vertebræ, partially fossilized, equalling in size those of the largest existing Crocodiles; they are of the 'procœlian' type, but present lacertian modifications, and closely agree with those in the great existing 'Lace-lizard' of Australia (*Hydrosaurus giganteus*, Gray), of which individuals upwards of six feet long have been taken. A generic or subgeneric distinction is indicated by the comparatively contracted area of the neural canal, and by the inferior development of the neural spine, of the fossil vertebræ, which have belonged to an individual not less than twenty feet in length, calculated from the vertebræ and proportions of the body of the existing *Hydrosauri*. For this, probably extinct lizard, the name of *Megalania prisca* is proposed.

The results of an extended series of comparisons of its vertebræ with those of recent and extinct Sauria are given; and the paper is illustrated by drawings of the vertebræ of *Megalania* and those of *Hydrosaurus*.

III. "Notes of Researches on the Poly-Ammonias."—No. III. Contributions towards the History of the Diamides; Cyanate and Sulphocyanide of Phenyl. By A. W. Hofmann, Ph.D., F.R.S. &c. Received May 7, 1858.

About ten years* ago, when engaged in the study of aniline, I discovered two beautiful crystalline compounds, carbanilide and sulphocarbanilide, which can be produced by a variety of processes. The former is best prepared by the action of phosgene-gas on aniline, while the latter is most readily and most abundantly procured by the action of bisulphide of carbon on aniline. The composition and the constitution of these bodies is indicated by the formulæ—

$$\text{Carbanilide} \ldots\ldots\ C_{26}\,H_{12}\,N_2\,O_2 = \left.\begin{matrix}(C_2\ O_2)'' \\ (C_{12}\,H_5)_2 \\ H_2\end{matrix}\right\}N_2,$$

$$\text{Sulphocarbanilide} \ldots\ C_{26}\,H_{12}\,N_2\,S_2 = \left.\begin{matrix}(C_2\ S_2)'' \\ (C_{12}\,H_5)_2 \\ H_2\end{matrix}\right\}N_2.$$

They may be viewed as derived from two molecules of ammonia (diammonia) in which two equivalents of hydrogen are replaced by two molecules of phenyl, and two other equivalents by the biatomic molecules $C_2\,O_2$ and $C_2\,S_2$.

The two substances in question, as far as their formulæ are involved, obviously correspond to urea and sulphocyanide of ammonium:—

$$\text{Urea} \ldots\ldots\ldots\ldots\ C_2\,H_4\,N_2\,O_2 = \left.\begin{matrix}(C_2\,O_2)'' \\ H_2 \\ H_2\end{matrix}\right\}N_2,$$

$$\text{Sulphocyanide of ammonium} \ldots\ C_2\,H_4\,N_2\,S_2 = \left.\begin{matrix}(C_2\,S_2)'' \\ H_2 \\ H_2\end{matrix}\right\}N_2.$$

In their formation likewise a certain analogy with urea and sulphocyanide of ammonium may be recognized; for recent experiments have proved that urea is actually produced by the action of phosgene-gas on ammonia, while the formation of sulphocyanide of ammonium by means of ammonia and bisulphide of carbon is a long established fact. The analogy, however, seems to disappear altogether, if the

* Journal of the Chemical Society, vol. ii. 36.

chemical nature of the four bodies be compared, for while urea exhibits the deportment of a base, and the saline character of sulphocyanide of ammonium is so well defined, carbanilide and sulphocarbanilide are apparently perfectly indifferent substances.

Nevertheless, on considering the difference of the chemical properties of urea and sulphocyanide of ammonium, and on recollecting that the saline constitution of urea is much more hidden than that of sulphocyanide of ammonium, it appeared worth while to try whether the action of powerful agents would not reveal a similar, if I may use the term, saline construction in carbanilide and sulphocarbanilide. Experiment has realized this anticipation.

In the conception of the above view, I have endeavoured to split the two bodies in question according to the equations—

$$\underbrace{C_{26}H_{12}N_2O_2}_{\substack{\text{Carbanilide,}\\\text{Diphenyl-carbamide.}}} = \underbrace{C_{12}H_7N}_{\text{Phenylamine.}} + \underbrace{C_{14}H_5NO_2}_{\substack{\text{Cyanate of}\\\text{phenyl.}}},$$

and

$$\underbrace{C_{26}H_{12}N_2S_2}_{\substack{\text{Sulphocarbanilide,}\\\text{Diphenyl-sulphocarbamide.}}} = \underbrace{C_{12}H_7N}_{\text{Phenylamine}} + \underbrace{C_{14}H_5NS_2}_{\substack{\text{Sulphocyanide}\\\text{of phenyl.}}}$$

suggested by analogous changes of urea and sulphocyanide of ammonium :—

$$\underbrace{C_2H_4N_2O_2}_{\text{Urea.}} = H_3N + \underbrace{C_2HNO_2}_{\text{Cyanic acid.}},$$

$$\underbrace{C_2H_4N_2S_2}_{\substack{\text{Sulphocyanide}\\\text{of ammonium.}}} = H_3N + \underbrace{C_2HNS_2}_{\substack{\text{Hydrosulphocyanic}\\\text{acid.}}}$$

These reactions succeed without much difficulty. On submitting carbanilide and sulphocarbanilide to the action of agents capable of fixing aniline (anhydrous phosphoric acid, chloride of zinc, and even hydrochloric acid gas), the former yields *cyanate of phenyl*, a substance which I discovered many years ago among the products of decomposition of oxamelanile*, while the latter furnishes a remarkable body, *sulphocyanide of phenyl*, which had not been previously obtained.

The general features of cyanate of phenyl having been delineated

* Journal of the Chemical Society, vol. ii. 313.

in a former memoir, I have for the present been chiefly engaged with the examination of sulphocyanide of phenyl. This body, which is readily obtained in a state of absolute purity by rectification over anhydrous phosphoric acid, is a colourless transparent liquid of 1·135 density at 15°·5, and of a perfectly constant boiling-point at 222° C. under a pressure of $0^{m}\cdot762$. The odour is aromatic and pungent; it distantly resembles that of mustard; the body in question is in fact the mustard oil of the phenyl-series.

Mustard oil, sulphocyanide of allyl. . $C_8\ H_5\ N\ S_2 = C_6\ H_5,\ C_2\ N\ S_2$.
Sulphocyanide of phenyl . . . . $C_{14}\ H_5\ N\ S_2 = C_{12}\ H_5,\ C_2\ N\ S_2$.

Sulphocyanide of phenyl may be distilled with water, and even with hydrochloric acid, without undergoing any change. The alkalies, on the other hand, decompose it. Boiled with an alcoholic solution of potassa, it is first converted into sulphocarbanilide, and ultimately into carbanilide.

$$\underbrace{2C_{14}\ H_5\ N\ S_2}_{\text{Sulphocyanide of phenyl.}} + 4KO + 2HO = 2KS + K_2\ C_2\ O_6 + \underbrace{C_{26}\ H_{12}\ N_2\ S_2}_{\text{Sulphocarbanilide.}}.$$

$$\underbrace{2C_{14}\ H_5\ N\ S_2}_{\text{Sulphocyanide of phenyl.}} + 6KO + 2HO = 4KS + K_2\ C_2\ O_6 + \underbrace{C_{26}\ H_{12}\ N_2\ O_2}_{\text{Carbanilide.}}$$

When gently warmed with phenylamine, sulphocyanide of phenyl is instantaneously converted into sulphocarbanilide,—

$$\underbrace{C_{14}\ H_5\ N\ S_2}_{\text{Sulphocyanide of phenyl.}} + \underbrace{C_{12}\ H_7\ N}_{\text{Phenyl-amine.}} = \underbrace{C_{26}\ H_{12}\ N_2\ S_2}_{\text{Sulphocarb-anilide.}}$$

A similar reaction takes place with ammonia. An alcoholic solution of ammonia, when gently warmed with sulphocyanide of phenyl, readily solidifies into a crystalline compound, which may be obtained in beautiful needles by crystallization from boiling water.

The new body is formed according to the equation

$$\underbrace{C_{14}\ H_5\ N\ S_2}_{\text{Sulphocyanide of phenyl.}} + H_3\ N = \underbrace{C_{14}\ H_8\ N_2\ S_2}_{\text{Sulphophenyl-carbamide.}}$$

This substance is the thiosinamine of the phenyl-series; like the latter, it possesses the characters of a weak base. I have not been

able to obtain saline compounds with hydrochloric and sulphuric acid. It forms, however, compounds with nitrate of silver and bichloride of platinum. The latter has the usual composition, viz.—

$$C_{14}\,H_8\,N_2\,S_2,\,H\,Cl,\,Pt\,Cl_2.$$

Boiled with nitrate of silver, the new compound loses its sulphur, which is replaced by oxygen, phenylcarbamide, $C_{14}\,H_8\,N_2\,O_2$, being produced, a substance which I described many years ago. Sulphocyanide of phenyl is acted upon by a great number of ammonias with formation of bodies the composition of which is sufficiently pointed out by theory.

The mode of producing cyanate and sulphocyanide of phenyl, which I have described in the preceding paragraphs, deserves some notice, since the usual processes suggested by the experience in the methyl-, ethyl- and amyl-series, such as distillation of sulphophenylates with cyanates and sulphocyanides, have altogether failed in producing the desired result. The same reaction may be of course applied to tolylamine, cumylamine, naphthylamine, and all primary monamines.

IV. "Notes of Researches on the Poly-Ammonias."—No. IV. Action of Bibromide of Ethylene upon Aniline. By A. W. Hofmann, Ph.D., F.R.S. &c. Received May 13, 1858.

While engaged in some experiments on the action of bibromide of ethylene on ammonia, a short account of which I have lately communicated to the Royal Society*, I induced Mr. Henry Bassett, then working in my laboratory, to study the deportment of the same bromide with aniline, a characteristic representative of the class of primary monamines. In the following pages I propose to submit to the Society Mr. Bassett's observations, together with the results of a series of experiments which I carried out myself after Mr. Bassett by circumstances had been prevented from a further continuation of the inquiry.

A mixture of 1 volume of the bibromide of ethylene and 2 volumes

* Proceedings of the Royal Society, vol. ix. page 150.

of aniline, when exposed to the temperature of boiling water for an hour or two, solidifies into a crystalline mass of more or less solidity. This mass is chiefly hydrobromate of aniline; it contains, however, in addition, three new organic bases, partly free, partly in the form of hydrobromates. These substances are formed in very different quantities,—a beautiful crystalline body, difficultly soluble in alcohol, being invariably the chief product of the reaction, while the two other bases, the one solid but extremely soluble in alcohol, the other likewise solid but quite insoluble in this liquid, are found to be present in much smaller proportions.

The preparation, in a state of purity, of the principal product of the reaction presents no difficulty. The solid mass obtained by digesting bibromide of ethylene and aniline in the stated proportions is mixed with water, and submitted to distillation, when any bibromide left unchanged, together with some unaltered aniline, passes over. The residuary liquid is then mixed with a strong solution of potassa, which separates all the bases existing as hydrobromates in the form of a semi-solid resin. This is washed with water and then again submitted to distillation with water, when, together with more or less water, an additional quantity of aniline distils. The residuary mass, when treated with boiling (methylated) spirit, leaves the insoluble base as a white, flour-like powder, while the other two bases dissolve. On cooling, the solution deposits a beautiful crystallization of white needles, while the more soluble base remains dissolved in the spirit. The crystals are rather difficultly soluble in alcohol; two or three crystallizations from this solvent render them absolutely pure.

Thus obtained, the new base, for which, in accordance with the results of analysis, I propose the name *ethylene-phenylamine*, is a snow-white, inodorous and tasteless crystalline compound, of nacreous lustre, insoluble in water, soluble in boiling, less so in cold alcohol, soluble in ether. The solutions are without action on vegetable colours. The substance dissolves readily in hydrochloric, sulphuric and nitric acids, especially on gently heating the liquids, which on cooling deposit well crystallized saline compounds. The hydrochlorate yields yellow precipitates with bichloride of platinum and terchloride of gold. When exposed to the action of heat, ethylene-phenylamine fuses at 148° C.; at a temperature approaching 300° it begins to boil

and to distil, the larger portion undergoing decomposition. Among the products of decomposition which are not yet sufficiently examined, considerable quantities of aniline make their appearance. The results obtained in the analysis of ethylene-phenylamine lead to the formula

$$C_{16}\,H_9\,N$$

as the simplest molecular expression for this compound.

This formula is confirmed by the analysis of the hydrochlorate and of the platinum-salt, the preparation of which, on account of their instability, requires some management.

These salts contain respectively

Hydrochlorate.........$C_{16}\,H_9\,N, HCl$.
Platinum-salt ....$C_{16}\,H_9\,N, HCl, PtCl_2$.

The reaction which gives rise to ethylene-phenylamine is expressed by the following equation:—

$$\underbrace{2C_{12}\,H_7\,N}_{\text{Phenylamine.}} + \underbrace{C_4\,H_4\,Br_2}_{\substack{\text{Bibromide}\\ \text{of ethylene.}}} = \underbrace{C_{12}\,H_7\,N, HBr}_{\substack{\text{Hydrobromate}\\ \text{of phenylamine.}}} + \underbrace{C_{16}\,H_9\,N, HBr.}_{\substack{\text{Hydrobromate of}\\ \text{ethylene-phenylamine.}}}$$

What is the constitution of this new base? This question could not be answered without further experiments, on account of the twofold nature of bibromide of ethylene. In many cases this remarkable compound exhibits the character of the hydrobromic ether of a biacid ethylene-alcohol, of $(C_4\,H_4)''Br_2$, whilst in the majority of reactions it splits into hydrobromic acid and the bromide $C_4\,H_3\,Br$, which might be considered as the hydrobromic ether of a monacid alcohol, $C_4\,H_4\,O_2$, homologous to allylic alcohol. It remained therefore uncertain whether the new basic compound retained the original molecule $(C_4\,H_4)''$ replacing 2 equivs. of hydrogen, or the modified molecule $C_4\,H_3$ replacing 1 equiv. of hydrogen. In other words, it had to be established by further experiments, whether the base was

$$\left.\begin{matrix}(C_4\,H_4)'' \\ C_{12}\,H_5\end{matrix}\right\} N = C_{16}\,H_9\,N, \text{ or}$$

$$\left.\begin{matrix}C_4\,H_3 \\ C_{12}\,H_5 \\ H\end{matrix}\right\} N = C_{16}\,H_9\,N.$$

The deportment of the substance with iodide of methyl and ethyl, which immediately will be mentioned somewhat more in detail, has

decided in favour of the former view, and in accordance with it the name of the substance has been selected.

It deserves to be noticed, that there are already two other bases known which have exactly the same composition, the one obtained by M. Natanson in the reaction of bichloride of ethylene upon aniline, and described by him as *acetylaniline*, the other discovered by M. Dusart among the derivatives of nitronaphtaline and designated as phtalidine. It is only necessary superficially to glance at the description of these bodies in order to see that they are essentially different from ethylene-phenylamine. The constitution of acetylaniline and phtalidine has not been experimentally fixed. It is probable that Natanson's base contains the molecule $C_4 H_3$ formerly called acetyl, but for which the more appropriate term vinyl has lately been proposed, while phtalidine probably derives from the hydrocarbon styrol or an isomeric body, so that the difference in the constitution of the three bodies would be expressed in the following formulæ :—

$$\begin{matrix}\text{Phtalidine} \\ \text{Styrylamine (?)}\end{matrix} \quad \left.\begin{matrix} C_{16}H_7 \\ H \\ H \end{matrix}\right\} N.$$

$$\begin{matrix}\text{Acetylaniline} \\ \text{Vinyl-phenylamine}\end{matrix} \quad \left.\begin{matrix} C_4\,H_3 \\ C_{12}H_5 \\ H \end{matrix}\right\} N.$$

$$\begin{matrix}\text{Ethylene-phenyl-} \\ \text{amine}\end{matrix} \quad \left.\begin{matrix} (C_4\,H_4)'' \\ C_{12}H_5 \end{matrix}\right\} N.$$

I have already mentioned that the degree of substitution of ethylene-phenylamine was fixed by the deportment of this base with iodide of methyl and ethyl, bibromide of ethylene exerting no longer any influence upon it, even by protracted contact, at temperatures varying from 100° to 150° C.

A mixture of ethylene-phenylamine and iodide of methyl, on the other hand, when exposed for some hours to the temperature of boiling water, solidifies to a resinous mass, floating, together with a portion of unchanged base, in the excess of the iodide. Distillation with water separates the excess of iodide of methyl; and washing with cold water until the filtrate is no longer precipitated by an alkali removes any hydriodate of ethylene-phenylamine formed during the distillation. Lastly, by repeated crystallization of the resinous residue from boiling water, to which a small quantity of spirit may be added in the later stages (separation from ethylene-phenylamine), a perfectly cry-

stalline, slightly yellowish iodine-compound is obtained, which may be dried without decomposition at 100°.

On analysis, this iodine-compound was found to have the remarkable composition

$$C_{34}H_{21}N_2I = \left.\begin{matrix} C_{16}H_9N \\ C_{16}H_9N \end{matrix}\right\} C_2H_3I.$$

Treated with oxide of silver, the solution of the iodide yields a powerfully alkaline liquid, possessing all the characters of the class of bodies of which hydrated oxide of tetrethylammonium is the type. On adding hydrochloric acid and bichloride of platinum, this liquid furnishes a pale yellow amorphous platinum-salt containing

$$C_{34}H_{21}N_2Cl, PtCl_2 = \left.\begin{matrix} C_{16}H_9N \\ C_{16}H_9N \end{matrix}\right\} C_2H_3Cl, PtCl_2.$$

A repetition of this experiment in the ethyl-series has given perfectly similar results. On account of the less powerful action of iodide of ethyl, the reaction requires longer digestion. The iodide formed is less soluble in boiling water than the corresponding methyl-compound, and therefore more difficult to separate from any ethylene-phenylamine which may have remained unchanged. When pure, the new iodide is a yellowish white substance crystallizing in needles. It fuses in the water-bath without decomposition to a yellow oil, which solidifies on cooling into a brittle crystalline mass.

On analysis, numbers were obtained corroborating in every respect the results furnished by the methyl-series. The iodide contains

$$C_{36}H_{23}N_2I = \left.\begin{matrix} C_{16}H_9N \\ C_{16}H_9N \end{matrix}\right\} C_4H_5I.$$

Like the methyl-compound, it is readily decomposed by oxide of silver; and the powerfully alkaline solution yields, with hydrochloric acid and bichloride of platinum, a salt of exactly the same appearance as the salt of the methyl-series. This platinum-salt was found to contain

$$C_{36}H_{23}N_2Cl, PtCl_2 = \left.\begin{matrix} C_{16}H_9N \\ C_{16}H_9N \end{matrix}\right\} C_4H_5Cl, PtCl_2.$$

The action of iodide of methyl and ethyl upon ethylene-phenylamine, although different from what might have been anticipated, nevertheless appears to fix in an unequivocal manner the state of substitution of this base. It is obvious that ethylene-phenylamine no longer contains any replaceable hydrogen, and consequently that the

molecule $(C_4 H_4)''$, equivalent to H_2 as such, has been assimilated by the aniline.

But how is the composition of the bodies formed by the action of iodide of methyl and ethyl to be interpreted? Are they simply compounds of the alcohol-iodides with 2 equivalents of ethylene-phenylamine, analogous to the salts produced by the union of 1 equiv. iodide of mercury with 2 equivs. of ammonia?

Does not the existence of these bodies involve a further consideration of the formula which has been assigned to ethylene-phenylamine? Does the formula $C_{16} H_9 N$ actually represent the molecule of this body, or is it not more correct to double that expression and to consider the formula $C_{32} H_{18} N_2$ as a more appropriate representation of this molecule? Ethylene-phenylamine would then be derived from 2 equivalents of ammonia, it would be a diamine, and the hydrochlorate and the platinum-compounds would appear in the light of diammonium-compounds.

Diethylene-diphenyl-diamine $\left.\begin{matrix}(C_4 H_4)_2'' \\ (C_{12} H_5)_2\end{matrix}\right\} N_2.$

Bichloride........................ $\left.\begin{matrix}(C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_2\end{matrix}\right\} N_2\, Cl_2.$

Platinum-salt..................... $\left.\begin{matrix}(C_4 H_4)_2'' \\ (C_{12} H_5)_2 \\ H_2\end{matrix}\right\} N_2\, Cl_2, 2PtCl_2.$

At the first glance it certainly appears strange that a molecule capable of assimilating 2 equivalents of hydrochloric acid should unite only with 1 equiv. of iodide of methyl or ethyl, well established members of the hydrochloric type. But this deportment after all is not without parallelism.

The expression

$$C_{20} H_{12} NO_2,$$

originally established for quinine by Liebig, supported as it was by the analysis of numerous salts of the formula

$$C_{20} H_{12} NO_2, HX,$$

and especially by that of a platinum-compound,

$$C_{20} H_{12} NO_2, HCl, PtCl_2, aq,$$

was universally adopted by chemists.

A few quinine-salts of the formula

$$2(C_{20}H_{12}NO_2), HX$$

were considered as anomalous, as basic compounds; and it was not until the methylic and ethylic derivatives of quinine,

$$2C_{20}H_{12}NO_2, C_2H_3I \quad \text{and}$$
$$2C_{20}H_{12}NO_2, C_4H_5I,$$

had been discovered that chemists began to consider the formula

$$C_{40}H_{24}N_2O_4$$

as a more appropriate expression for the molecule of quinine.

Probably further examination of the salts of ethylene-phenylamine —I retain this name for the present—will furnish saline compounds corresponding to the methyl- and ethyl-derivatives, showing that this base, like quinine, is capable of forming two groups of salts.

It deserves to be noticed that the diammonic nature of ethylene-phenylamine is also strongly marked by its deportment under the influence of heat; for while all the monammonic basic derivatives of aniline are volatile without decomposition, ethylene-phenylamine, when submitted to distillation, is destroyed with reproduction of aniline, like the well-established diamines belonging to this group, melaniline, formyl-diphenylamine, &c.

In describing the preparation of ethylene-phenylamine, it has been mentioned that the action of bibromide of ethylene on aniline gives rise at the same time to two other basic compounds. These substances, which are formed in smaller quantity, differ in a very marked manner from the principal product of the reactions. Their study is not yet completed, but it may even now be stated, that they have the same composition as ethylene-phenylamine itself. One of these substances, remarkable for its solubility in spirit, is capable of being converted into ethylene-phenylamine by a simple molecular change. The relation in which these three isomeric bodies stand to each other is not yet finally fixed by experiment. The idea suggests itself that it may possibly be represented by the formulæ—

Soluble base............	$C_{16}H_9N$.
Ethylene-phenylamine	$C_{32}H_{18}N_2$.
Insoluble base.........	$C_{48}H_{27}N_3$.

V. "Notes of Researches on the Poly-Ammonias."—No. V. Action of Bichloride of Carbon on Aniline. By A. W. Hofmann, Ph.D., F.R.S. &c. Received June 17, 1858.

In two former notes I have described the deportment of aniline as the prototype of primary monamines with the bromine- and chlorine-compounds of biatomic and triatomic radicals. It was found that under the influence of these agents, two equivalents of aniline coalesce to a more complex molecule, retaining the chemical character of the constituents; the action of bibromide of ethylene and chloroform producing respectively—

$$\text{Diethylene-diphenyl-diamine } C_{32}H_{18}N_2 = \left\{ \begin{matrix} (C_4\,H_4)_2{}'' \\ (C_{12}\,H_5)_2 \end{matrix} \right\} N_2.$$

$$\text{Formyl-diphenyl-diamine} \;\ldots\; C_{26}H_{12}N_2 = \left\{ \begin{matrix} (C_2\,H)''' \\ (C_{12}H_5)_2 \\ H \end{matrix} \right\} N_2.$$

The result of these experiments led me to examine the behaviour of aniline under the influence of organic chlorides containing even a larger number of chlorine equivalents. The agent selected was the body well known by the name of bichloride of carbon, *i. e.* tetrachlorinetted marsh-gas, or chloroform, in which the residuary equivalent of hydrogen is replaced by chlorine.

Aniline and bichloride of carbon do not act upon each other at the common temperature; at the temperature of boiling water a change is perceptible, but even after several days' exposure the reaction is far from being complete. On submitting, however, a mixture of $3\frac{1}{2}$ parts by weight of aniline and 1 part of bichloride of carbon, both in the anhydrous state, for about thirty hours to a temperature of 170° C., the liquid will be found to be converted into a black mass, either soft and viscid, or hard and brittle, according to time and temperature.

This black mass, which adheres firmly to the tubes in which the reaction has been accomplished, is a mixture of several bodies. On exhausting with water, a portion dissolves, while a more or less solid resin remains behind.

The aqueous solution yields, on addition of potassa, an oily precipitate containing a considerable portion of unchanged aniline; on boiling this precipitate with dilute potassa in a retort, the aniline distils over, whilst a viscid oil remains behind, which gradually

solidifies with a crystalline structure. Washing with cold alcohol and two or three crystallizations from boiling alcohol render this body perfectly white and pure, a very soluble substance of a magnificent crimson colour remaining in solution.

The portion of the black mass which is insoluble in water dissolves almost entirely in dilute hydrochloric acid, from which solution it is reprecipitated by the alkalies in the form of an amorphous pink or dingy precipitate soluble in alcohol with a rich crimson colour. The greater portion of this body consists of the same colouring principle which accompanies the white crystalline substance. On the other hand, considerable quantities of this crystalline body are occasionally present in the product insoluble in water.

The crystalline body is insoluble in water, difficultly soluble in boiling alcohol, soluble in ether. From the hot alcoholic solution it crystallizes slowly on cooling in elongated four-sided plates, often grouped round a common centre; this substance is a well defined base; it freely dissolves in acids, from which, on the addition of the alkalies, it is thrown down as a dazzling white precipitate.

The analysis of this new base has led to the expression

$$C_{38}\,H_{17}\,N_{3},$$

a formula corroborated by the analysis of a fine, somewhat difficultly soluble hydrochlorate,

$$C_{38}\,H_{17}\,N_{3},\ HCl,$$

which is obtained by dissolving an excess of the new base in hot diluted hydrochloric acid, when it crystallizes on cooling.

A further confirmation was furnished by the examination of a bright yellow platinum-salt,

$$C_{38}\,H_{17}\,N_{3},\ HCl,\ PtCl_{2}.$$

Both the hydrochlorate and the platinum-salt are extremely soluble in an excess of hydrochloric acid, which has therefore to be carefully avoided in their preparation.

The phase of the action of bichloride of carbon on aniline, which gives rise to the formation of the new base, is expressed by the equation

$$6C_{12}\,H_{7}\,N+C_{2}\,Cl_{4}=C_{38}\,H_{17}\,N_{3},\ HCl+3(C_{12}\,H_{7}\,N,\ HCl).$$

What is the constitution of the new body? It is obviously derived from 3 molecules of aniline from which 4 equivalents of hydrogen have been eliminated by the 4 equivalents of chlorine in the bi-

chloride, the carbon entering as a biatomic molecule into the complex atom. The new body would thus become a triamine,

$$\left.\begin{matrix} C_2{}'' \\ (C_{12}\,H_5)_3 \\ H_2 \end{matrix}\right\} N_3.$$

It is however more probable that the carbon replaces in the form of cyanogen, when the new compound appears in the light of a diamine, as

$$\text{Cyan-triphenyl-diamine } \left.\begin{matrix} C_2\,N \\ (C_{12}\,H_5)_3 \\ H_2 \end{matrix}\right\} N_2.$$

The new compound thus becomes closely allied to melaniline, which may be viewed as diphenyl-cyan-diamine,

$$\text{Melaniline } C_{26}\,H_{13}\,N_3 = \left.\begin{matrix} C_2\,N \\ (C_{12}\,H_5)_2 \\ H_3 \end{matrix}\right\} N_2.$$

It deserves to be noticed, that in its appearance, and in its general characters, cyan-triphenyl-diamine resembles melaniline in a remarkable manner.

If we are entitled to view the new body which forms the subject of this note as a cyanogen-substitute, we have not less than four well-defined diamines of the phenyl-series.

$$\text{Diethylene-diphenyl-diamine } \left.\begin{matrix} (C_4\,H_4)''_2 \\ (C_{12}\,H_5)_2 \end{matrix}\right\} N_2.$$

$$\text{Formyl-diphenyl-diamine} \ldots\ldots \left.\begin{matrix} (C_2\,H)''' \\ (C_{12}\,H_5)_2 \\ H \end{matrix}\right\} N_2.$$

$$\text{Cyan-diphenyl-diamine} \ldots\ldots \left.\begin{matrix} C_2\,N \\ (C_{12}\,H_5)_2 \\ H_3 \end{matrix}\right\} N_2.$$

$$\text{Cyan-triphenyl-diamine} \ldots\ldots \left.\begin{matrix} C_2\,N \\ (C_{12}\,H_5)_3 \\ H_2 \end{matrix}\right\} N_2.$$

I intend to continue the inquiry still further in this direction, and propose next to examine the deportment of aniline with the so-called protochloride ($C_4\,Cl_4$) and sesquichloride of carbon ($C_4\,Cl_6$).

VI. "Researches on the Phosphorus-Bases." By A. W. Hofmann, Ph.D., F.R.S. &c. Received May 28, 1858.

In a paper published in the Transactions of the Royal Society, we (M. Cahours and myself) have given a detailed account of the preparation of the phosphorus-bases, and also an accurate description of triethylphosphine, the most characteristic and accessible representative of this class of compounds.

The object of our joint inquiry was chiefly to examine the phosphorus-bases as a class, and to establish their analogy with the corresponding terms of the nitrogen-series. The deportment of the phosphorus-bodies in their relation to other compounds has as yet been scarcely investigated. For several months I have been engaged in this study, which promises a rich harvest of results. Most of the experiments were made with triethylphosphine, a substance which, in consequence of its convenient position in the system of organic compounds, in consequence of the variety of its attachments, the energy and precision of its action, and, lastly, the well-defined character of its compounds, will probably become an agent of predilection in the hands of the chemist.

It is my intention to trace the history of this remarkable body in its several directions; and for this purpose, in fact, a considerable amount of material has been already accumulated. But since necessarily some time must elapse before such an inquiry, which from the peculiar character of the compound is often obstructed by unusual difficulties, can be completed, I beg leave to present my results in the same measure as the inquiry advances, hoping that at a later period I may be allowed to collect the scattered observations, and to lay them in a more elaborated and digested form before the Society.

Among the numerous reactions of triethylphosphine, my attention has been chiefly directed to the compounds which this body furnishes when submitted to the action of organic chlorides, bromides, and iodides.

I. *Action of Bibromide of Ethylene upon Triethylphosphine.*

In the anhydrous condition the two bodies act even at the common temperature with considerable power upon each other, a white cry-

stalline substance being immediately precipitated. If the reaction be allowed to go on in the presence of a large volume of anhydrous ether, the deposition of the crystalline body is considerably retarded, unless the mixture, in an appropriate apparatus, be exposed to the temperature of boiling water. After a short digestion, on distilling off the ether and the excess of bibromide, a crystalline cake is left in the retort, consisting of several bromides, the nature and the relative proportions of which appear in a great measure to depend upon the rapidity of the reaction. I have found it most convenient to work with ethereal solutions at the common temperature.

The determination of the bromine in the crystalline body revealed at once the compound character of this substance, for it steadily diminished by dissolving the bromide in absolute alcohol, and reprecipitating it partially by ether. By repeating this process four or five times, a body of constant composition was obtained.

The compound thus prepared is a crystalline mass, without odour, extremely soluble in water, and even in absolute alcohol, but insoluble in anhydrous ether. It exhibited a rather unexpected composition, for on analysis it was found to contain

$$C_{16}\,H_{19}\,PBr_2,$$

and consequently to have been formed by the simple union of 1 equivalent of triethylphosphine and 1 equivalent of bibromide of ethylene,

$$C_{12}\,H_{15}\,P+C_4\,H_4\,Br_2=C_{16}\,H_{19}\,PBr_2.$$

The bromine in this compound exists in two perfectly different forms; addition of nitrate of silver precipitated only one-half of this element as bromide of silver, while even by protracted ebullition the second half remained untouched. The result changed, however, on digestion with freshly precipitated oxide of silver, when the whole of the bromine separated at once in the form of bromide of silver.

On adding to the solution of the bromide an excess of nitrate of silver, filtering off the bromide, and removing the excess of silver by hydrochloric acid, a corresponding chloride was obtained, from which bichloride of platinum precipitated a beautiful orange-yellow platinum-salt. In a moderately diluted solution which had been previously gently heated, no immediate precipitate was produced; but on cooling, the same salt was deposited in magnificent needles, which

could be recrystallized from boiling water, or better from hydrochloric acid.

This compound contained

$$C_{16}H_{19}BrPCl, PtCl_2.$$

A difficultly soluble gold-salt, crystallizing from boiling water in small scales, was found to have the corresponding composition,

$$C_{16}H_{19}BrPCl, AuCl_3.$$

Very different results were observed when the whole of the bromine was removed by means of oxide of silver. A powerfully alkaline solution was thus obtained, which, converted into hydrochlorate, gave, with bichloride of platinum, a precipitate only after very considerable evaporation. The precipitate was likewise of a deep orange-red colour; it readily dissolved in boiling water, from which it separated on cooling in the form of well-defined octahedra having the composition

$$C_{16}H_{18}PCl, PtCl_2.$$

Terchloride of gold furnished likewise a crystalline precipitate very similar in appearance to the gold-salt previously mentioned, but containing

$$C_{16}H_{18}PCl, AuCl_3.$$

The action of bibromide of ethylene on triethylphosphine, and the subsequent transformation of the compound produced, is readily explained. The two substances unite in equal equivalents, the product of the reaction being the bromide of a phosphonium, in which the fourth equivalent of hydrogen is replaced by a compound molecule, C_4H_4Br (brominetted ethyl?), of monatomic substitution-power,

Bromide of triethyl-bromethylene-phosphonium $\left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ (C_4H_4Br)' \end{matrix}\right\} PBr.$

The compound phosphonium of this bromide possesses very considerable stability, as is sufficiently evinced by its deportment with nitrate of silver, and by the formation of the platinum- and of the gold-salt. All my attempts, however, to separate the base itself have entirely failed. Under the influence of oxide of silver, the bromide yields an alkaline solution possessing all the characters of the -onium-bases. The body in solution, however, no longer belongs

to the same series, the elements of hydrobromic acid having separated from the original compound metal.

$$\left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ (C_4H_4Br)' \end{matrix}\right\} PBr + 2AgO = 2AgBr + \left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ C_4H_3 \end{matrix}\right\} PO, HO.$$

The compound thus obtained may be designated as the hydrated oxide of triethyl-vinyl-phosphonium.

I have ascertained by experiment that the brominetted bromide is by no means the only result of the action of bibromide of ethylene on triethylphosphine, although under favourable circumstances it appears to be the chief product. Invariably a portion of the bibromide, faithful to its traditions, splits into hydrobromic acid and bromide of vinyl; and we find therefore in the white crystalline mass always, together with hydrobromate of triethylphosphine, a certain quantity of the very bromide of triethyl-vinyl-phosphonium, which, as has been stated, results from the action of oxide of silver on the brominetted bromide.

$$2\left[\left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} P\right] + C_4H_4Br_2 = \left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ H \end{matrix}\right\} PBr + \left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ C_4H_3 \end{matrix}\right\} PBr.$$

The action of bibromide of ethylene on triethylphosphine, complex as it is, receives an additional element of complication by the influence of heat. Ebullition appears to facilitate the formation of a fourth bromide, which, although less prominently, is also produced in the cold. The study of this compound is not yet completed.

VII. "Researches on the Phosphorus-Bases."—No. II. Action of Bisulphide of Carbon on Triethylphosphine. By A. W. Hofmann, Ph.D., F.R.S. &c. Received June 5, 1858.

Among the many characteristic reactions of the phosphorus-bases, their deportment with sulphur is so conspicuous that it has served frequently as a test for the presence of these substances. In continuing the study of the phosphorus-bases, I have found that this remarkable attraction for sulphur is by no means limited to this element in the free state. Many sulphur-compounds, when coming

into contact with triethylphosphine, are instantaneously decomposed, their sulphur being appropriated in the formation of the beautiful bisulphide

$$E_3PS_2,$$

which, as has been pointed out on a former occasion, is generated by the action of free sulphur. As an illustration, the deportment of bisulphide of nitrogen may be quoted. This substance, obtained by the action of ammonia on chloride of sulphur, and as yet scarcely touched upon as an agent of research, is instantaneously decomposed into its constituents when acted upon by triethylphosphine,

$$\underbrace{E_3P}_{\substack{\text{Triethyl-}\\\text{phosphine.}}} + \underbrace{NS_2}_{\substack{\text{Bisul-}\\\text{phide of}\\\text{nitrogen.}}} = \underbrace{E_3PS_2}_{\substack{\text{Bisulphide}\\\text{of triethyl-}\\\text{phosphine.}}} + N.$$

The reaction is so violent that care must be taken to prevent the phosphorus-base from being inflamed.

Triethylphosphine is not less powerfully attacked by bisulphide of carbon; but the result is different. On mixing the two bodies in the anhydrous condition, they are found to combine with explosive violence, a deep crimson-coloured crystalline compound being produced. This substance is obtained in better crystals if ethereal solutions, instead of the anhydrous compounds, be employed. The new body separates in beautiful crimson leaflets the moment the two solutions are mixed. This phenomenon is so characteristic, that ever since it was first noticed, it has served me as a valuable test for the detection of even minute traces of triethylphosphine. A watch-glass, moistened with the liquid in which the phosphorus-base is suspected, is held over a vessel containing bisulphide of carbon: the vapour of this compound immediately causes the formation of a crimson network of crystals, if the smallest quantity of triethylphosphine be present. It is necessary that the base should be free; its saline solutions are not affected by bisulphide of carbon; the reaction, however, immediately appears when the base is liberated by the addition of an alkali.

The new body produced by the action of bisulphide of carbon upon triethylphosphine is insoluble in water, nearly insoluble in ether, but soluble in alcohol. From boiling alcohol it is deposited on cooling in crimson needles, somewhat similar to the crystals of

chromic acid as obtained by the action of sulphuric acid upon bichromate of potassium. The presence of bisulphide of carbon in the alcohol considerably increases its solvent power for the crimson body. The new substance fuses at about 95° C; it is volatile even at the common temperature, and is easily volatilized at the temperature of boiling water. When rapidly heated it sublimes with partial decomposition.

The crimson crystals appear to have the character of a weak base; they easily dissolve in concentrated hydrochloric acid, a colourless liquid being formed; from this solution potassa or ammonia reprecipitate the body, apparently unchanged, although, in consequence of the finely divided state, of a somewhat lighter colour. The hydrochloric solution gives with bichloride of platinum a bright yellow amorphous salt insoluble in alcohol and ether, which on drying becomes dingy, with indications of decomposition. A gold-salt similarly obtained exhibits a like deportment. Both salts appeared but very little adapted for analysis. The alcoholic solution of the body is decomposed by nitrate of silver with formation of sulphide of silver.

The analysis of the crimson crystals has shown that they contain

$$C_{14}\,H_{15}\,PS_4=C_{\,2}\,H_{15}\,P+C_2\,S_4=E_3\,P+C_2\,S_4.$$

They are therefore formed by the direct union of 1 equivalent of triethylphosphine with 2 equivalents of bisulphide of carbon.

In the dry state the bisulphide of carbon compound may be preserved without being altered. In the presence of moisture, however, it is decomposed, especially during hot weather. On examining some specimens which had been kept during several months, the crimson colour was found to have disappeared, the substance had assumed a light yellow colour, and on opening the bottles the odour of sulphuretted hydrogen became at once apparent. The yellowish substance on recrystallization proved to be pure bisulphide of triethylphosphine. I leave it undecided whether this transposition had taken place according to the equation

$$E_3\,P\,C_2\,S_4+2HO=E_3\,P\,S_2+2HS+C_2\,O_2,$$

or

$$E_3\,P\,C_2\,S_4+2HO+2O=E_3\,P\,S_2+2HS+C_2\,O_4.$$

What is the constitution of the crimson body? In mineral

chemistry we are acquainted with a compound closely allied in composition and formation to the new compound. Bisulphide of carbon, when treated with an alcoholic solution of ammonia, furnishes, together with other products, a salt crystallized in long lemon-yellow needles, which is known by the name of sulphocarbamate of ammonium.

This compound,

$$(H_4 N) H_2 N, C_2 S_4,$$

when treated with diluted acids, is converted into an oily acid of but little stability, sulphocarbamic acid:

$$H H_2 N, C_2 S_4.$$

If we replace in this compound the hydrogen by ethyl, the nitrogen by phosphorus, in other words, if we replace the ammonia by triethylphosphine, we arrive at the composition of the body which forms the subject of this note.

I have convinced myself experimentally that trimethylphosphine exhibits with bisulphide of carbon a perfectly similar deportment. The compound formed is likewise of a crimson colour, but of a somewhat lighter tint; it is more volatile and more readily soluble in alcohol than the corresponding ethyl-compound: it is also somewhat soluble in water.

Triethylarsin is not altered by the addition of bisulphide of carbon; after some time, however, long needles are formed in the mixture of the two bodies. These needles are probably an analogous arsenic-compound: I have not however examined them. A mixture of triethylstibin and bisulphide of carbon was preserved for several months, without undergoing any change.

VIII. "Contributions towards the History of the Monamines." By A. W. Hofmann, Ph.D., F.R.S. &c. Received May 28, 1858.

The progress of my experiments on the poly-ammonias and on the phosphorus-bases, now and then involves the study of reactions which are scarcely comprised between the boundary lines of the principal inquiries. For the sake of perspicuity, I beg leave to submit the results of these studies separately to the Society.

1. *Action of Bibromide of Ethylene on Trimethylamine.*

The unexpected result obtained in the action of bibromide of ethyl-

ene on triethylphosphine, induced me to examine the deportment of the tertiary amine-bases under the influence of the same agent. As a characteristic representative of this class I have selected trimethylamine, which may be readily procured in tolerable quantity and in a state of purity.

On submitting trimethylamine to the action of bibromide of ethylene, phenomena are observed which are perfectly similar to those which occur in the analogous experiment with triethylphosphine. On account of the volatility of the trimethylamine, I have never worked with the anhydrous base, but invariably either with aqueous or alcoholic solutions. At the common temperature bibromide of ethylene is only gradually acted on by an aqueous solution of trimethylamine. Frequent agitation and contact for several days are necessary to complete the reaction; addition of alcohol accelerates the process; which may be still very considerably shortened by exposure of the mixture in sealed vessels to a temperature of from 40° to 50°. To exclude complication, it is desirable to avoid a higher temperature and to keep always the bromide in considerable excess.

By adopting these precautions, the mixture of the two bodies is soon found to deposit a white crystalline salt, the formation of which continues until the liquid has assumed an acid reaction. A considerable quantity of this salt is dissolved in the water; it is therefore most convenient to distil off the excess of bibromide of ethylene and to evaporate the residuary liquid to dryness. The dry saline mass, separated from a slightly yellowish deliquescent substance by washing with absolute alcohol and once or twice recrystallized from the same solvent, furnishes magnificent white needles, extremely soluble in water, readily soluble in boiling alcohol, much less so in cold alcohol, and insoluble in ether. This salt can be boiled with the fixed alkalies without disengaging a trace of an alkaline vapour. This deportment renders it easy to recognize the absence of impurities.

The composition of this substance, established by many determinations, is represented by the formula

$$C_{10}H_{13}NBr_2 = \left.\begin{array}{l} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ (C_4H_4Br)' \end{array}\right\} N, Br.$$

This substance, which presents itself as bromide of trimethyl-bromethylene-ammonium, is formed by the simple union of 1 equivalent

of bibromide of ethylene with 1 equivalent of the tertiary monamine. A glance at the formula exhibits the perfect analogy of the composition of this compound with that of the bromide formed by the action of bibromide of ethylene on triethylphosphine. The deportment of the two salts with nitrate and with oxide of silver is also similar in every respect.

By treatment with nitrate of silver, the bromine not belonging to the ammonium may be removed without affecting the bromine of the radical. The nitrate thus obtained, after separation of the excess of silver, furnishes with bichloride of platinum a difficultly soluble octahedral salt, crystallizable from a large quantity of boiling water, and containing

$$C_{10}H_{13}BrNCl, PtCl_2 = \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ (C_4H_4Br)' \end{matrix}\right\} NCl, PtCl_2;$$

and with terchloride of gold an analogous compound crystallizing from boiling water in splendid golden-yellow needles,

$$C_{10}H_{13}BrNCl, AuCl_3 = \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ (C_4H_4Br)' \end{matrix}\right\} NCl, AuCl_3.$$

Treatment with oxide of silver converts the bromide of trimethyl-bromethylene-ammonium into the oxide of trimethyl-vinyl-ammonium:

$$\left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ (C_4H_4Br)' \end{matrix}\right\} NBr + 2AgO = 2AgBr + \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ C_4H_3 \end{matrix}\right\} NO, HO.$$

The solution of this substance is a powerfully alkaline liquid, which, on saturation with hydrobromic acid, furnishes a deliquescent bromide of extreme solubility, entirely differing from the original bromide. The corresponding chloride forms with bichloride of platinum an octahedral salt, likewise extremely soluble in water, but insoluble in alcohol; with terchloride of gold, beautiful yellow needles recrystallizable from boiling water.

$$\text{Platinum-salt } C_{10}H_{12}NCl, PtCl_2 = \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ C_4H_3 \end{matrix}\right\} NCl, PtCl_2.$$

$$\text{Gold-salt.... } C_{10}H_{12}NCl, AuCl_3 = \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ C_4H_3 \end{matrix}\right\} NCl, AuCl_3.$$

As might have been expected from the experience gathered in the phosphorus-series, the formation of the brominetted bromide is invariably accompanied by the simultaneous production of the vinyl-compound, and of a corresponding quantity of hydrobromate of trimethylamine.

$$2\left[\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\end{matrix}\right\}N\right]+C_4\,H_4\,Br_2=\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\H\end{matrix}\right\}N\,Br+\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\C_4\,H_3\end{matrix}\right\}N\,Br.$$

Indeed it would appear that at a high temperature and with an excess of trimethylamine, the equation just given represents the principal phase of the reaction. In an experiment made under the stated conditions, the liquid in the digester had assumed a deep yellowish colour; and on evaporation and appropriate treatment a crystalline salt was obtained, which on analysis was found to consist exclusively of

$$C_{10}\,H_{12}\,N\,Br=\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\C_4\,H_3\end{matrix}\right\}N\,Br,$$

the mother-liquor containing a large quantity of hydrobromate of trimethylamine. It is possible that even in this reaction the vinyl-compound was only a secondary product, formed by the decomposition of the brominetted bromide under the influence of an excess of trimethylamine.

$$\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\(C_4\,H_4\,Br)'\end{matrix}\right\}N\,Br+\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\end{matrix}\right\}N=\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\H\end{matrix}\right\}N\,Br+\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\C_4\,H_3\end{matrix}\right\}N\,Br.$$

Exactly as in the phosphorus-series, together with the compounds described, some other substances are formed, particularly when the process is supported by the action of heat. As yet I do not sufficiently understand these additional reactions.

I have established experimentally that triethylamine and triamylamine, when treated with bibromide of ethylene, give rise to similar reactions. I have not, however, minutely examined the substances which are formed. They are sufficiently characterized by theory.

The unexpected deportment of bibromide of ethylene with the tertiary monamines and monophosphines, furnishes a new proof of the fact, that all our rational formulæ are, after all, the expressions of

special reactions. With the alkalies, the brominetted Dutch liquid behaves as a double salt of two monatomic compounds,

$$(C_4 H_3)' Br + H Br.$$

With silver-salts, with aniline, &c., it exhibits the deportment of a true biatomic compound,

$$(C_4 H_4)'' Br_2.$$

With the tertiary amines and phosphines, lastly, we find that the elements of the same body, in accordance with the requirement of the case, arrange themselves into one monatomic compound, the constitution of which, if we simply consider the function which it performs under these special circumstances, might be represented by the formula

$$(C_4 H_4 Br)' Br.$$

It is obvious that the three formulæ,

$$(C_4 H_3)' Br, H Br,$$
$$(C_4 H_4)'' Br_2 \quad \text{and}$$
$$(C_4 H_4 Br)' Br,$$

represent the constitution of this body with reference to certain special conditions; the absolute arrangement of the molecules we ignore altogether, and it is doubtful whether it will ever be accessible to experiment.

IX. "Researches on the Action of Ammonia on Glyoxal." By Dr. H. Debus. Communicated by Dr. Tyndall. Received May 21, 1858.

(Abstract.)

If alcohol be slowly oxidized by nitric acid at ordinary temperatures, besides other substances, glyoxal, $C_2 H_2 O_2$, and glyoxylic acid, $C_2 H_4 O_4$*, are formed.

I have continued the investigation of these substances, and beg to lay before the Royal Society some of the more interesting results.

Glyoxal, of the consistency of ordinary syrup, is mixed with about three times its bulk of strong ammonia, and the mixture kept for twenty minutes at a temperature from 60° to 80° C. The liquid

* C=12, H=1, O=16.—Phil. Mag. Nov. 1856, and Jan. 1857.

now contains two organic bases—one in the shape of a crystalline precipitate, which I propose to call glycosine, and the other in solution, to which in this paper the name of glyoxaline will be applied. Besides these two substances, only a little formic acid and the excess of ammonia can be recognized in the liquid.

Glycosine $=C_6 H_6 N_4$. The crystals contained in the ammoniacal liquid are collected on a filter and washed with cold water. By dissolving them in diluted hydrochloric acid, treating with charcoal and adding ammonia to the decolorized solution, the glycosine is obtained as a colourless, crystalline precipitate. The crystals are little prisms, tasteless, inodorous, and only soluble in a great quantity of boiling water. They become very electric when rubbed in a mortar. A little glycosine placed between two watch-glasses and heated on a sand-bath, sublimes without leaving a residue, and produces magnificent prismatic needles, sometimes of half an inch in length. It forms salts with acids, which generally crystallize well. The chloride has a great tendency to form double salts with the chlorides of copper, mercury, and platinum.

Chloroplatinate, $C_6 H_6 N_4 + 2(H\,Cl\,Pt\,Cl_2)$, forms a fine yellow crystalline powder, soluble with difficulty in water. An excess of water seems to abstract bichloride of platinum and hydrochloric acid. Glycosine is formed from ammonia and glyoxal according to the equation—

$$\underbrace{3(C_2 H_2 O_2)}_{\text{Glyoxal.}} + 4NH_3 = \underbrace{C_6 H_6 N_4}_{\text{Glycosine.}} + \underbrace{6H_2 O}_{\text{Water.}}$$

I showed on another occasion, that glyoxal has the properties of an aldehyde. Its behaviour towards ammonia confirms my former conclusions. The formation of amarine, from oil of bitter almonds, of acetonine from acetone and ammonia, takes place in a similar manner:—

$$3(C_7 H_6 O) + 2NH_3 = \underbrace{C_{21} H_{18} N_2}_{\text{Amarine.}} + 3H_2 O$$

$$3(C_3 H_6 O) + 2NH_3 = \underbrace{C_9 H_{18} N_2}_{\text{Acetonine.}} + 3H_2 O$$

In all other known cases, when from an aldehyde or the chloride of an alcohol radical and ammonia, a basic substance is formed, one or two equivalents of ammonia participate in the reaction.

If ammonia and glyoxal decompose each other, four equivalents of the first transfer their nitrogen to one equivalent of the base produced. The direct derivation from ammonia of a base which contains four equivalents of nitrogen, seems to me to be very interesting.

The rational formula of glycosine is probably

$$N_4 \left\{ \begin{array}{l} C_2 H_2 \\ C_2 H_2 \\ C_2 H_2 \end{array} \right.$$

$C_2 H_2$ being equivalent to H_4.

It is worthy of notice, that in chemical decompositions very often three equivalents of an aldehyde unite and act like one molecule. I will only mention, as examples, mesitylene, acetonia, thialdine, hydrosalicylamide, and amarine.

Glyoxaline $= C_3 H_4 N_2$ is obtained as binoxalate from the mother-liquor of glycosine, if, after expelling the ammonia by gentle heating, an excess of oxalic acid is added. The binoxalate crystallizes very well and may be purified easily. The composition of it is expressed by the formula $C_3 H_4 N_2 + C_2 H_2 O_4$. The base is obtained from this salt by treating it with carbonate of lime, and evaporating the filtrate from the oxalate of lime to the consistency of a strong syrup.

Glyoxaline crystallizes with difficulty in prismatic crystals, radiating from one centre. It is easily soluble in water, has a strong alkaline reaction, neutralizes acids perfectly, but does not appear to form a compound with carbonic acid. It melts easily, smells like fish, and evaporates at a higher temperature in dense white fumes. Chloride of copper forms a precipitate with glyoxaline, which is soluble in an excess of the base.

The chloroplatinate, $C_3 H_4 N_2 + HCl\ PtCl_2$, crystallizes in large red prisms, and is easily soluble in hot water.

The formation of glyoxaline takes place according to the following equation :—

$$\underbrace{2(C_2 H_2 O_2)}_{\text{Glyoxal.}} + 2N H_3 = \underbrace{C_3 H_4 N_2}_{\text{Glyoxaline.}} + \underbrace{C H_2 O_2}_{\text{Formic acid.}} + \underbrace{2H_2 O}_{\text{Water.}}$$

Glyoxaline is homologous with sinnamine.

X. "An Experimental Inquiry into the alleged Sugar-forming Function of the Liver." By F. W. Pavy, M.D. Communicated by Dr. Owen Rees. Received May 26, 1858.

(Abstract.)

The author commenced by stating that the question to be discussed in his communication was, not whether sugar was to be found in the animal system independently of a saccharine alimentation, for that he considered to stand upon irrefutable ground; but whether the sugar encountered in the liver *after death* was a natural representation of the condition during life, or was only the result of a *post mortem* occurrence. He had noticed as early as February 1854, that the blood removed by catheterism of the right ventricle during life, was almost completely destitute of saccharine impregnation. The observation did not then, however, receive the attention it deserved; but on repeating the experiment at a later period, and meeting with a similar result, an investigation was made which has led to the conclusions advanced in his communication.

From upwards of sixty observations, it is asserted that the condition of the blood after death can no longer be taken as indicating its state during life. For, if blood be withdrawn from the right ventricle of the living animal in a natural or tranquil state, there is scarcely an appreciable amount of sugar to be discovered, whilst, if the animal be afterwards sacrificed and blood collected from a fine incision of the ventricle, it will be found to present a strong indication of the presence of sugar. In one of the experiments quoted, there was a barely appreciable reaction in the blood removed during life, and nearly 1 per cent of sugar in the blood collected after death, the animal having been sacrificed immediately after catheterism has been performed.

Observing this striking contrast in the blood abstracted from the right ventricle *before* and *after* death, the possibility occurred that there might be a corresponding contrast in the organ that was considered to be specially endowed with a sugar-forming function. The recent researches of Bernard had taught us that a material naturally existed in the liver which was extremely susceptible of conversion into sugar. It was this material, in fact, which was looked upon as

giving rise to the sugar thought to be largely present in the liver during life. At the outset of the inquiry, an agent was sought for which would check the transformation of the sugar-forming material after death, and thus present the liver in a condition as near as possible to that which existed during life. Potash was found to possess this effect without destroying the principles concerned. A strong solution of it was then injected, as instantly after death as practicable, through the portal vein into the liver; and, as the result, the organ presented scarcely any appreciable trace of the presence of sugar. A liver similarly treated when it had been allowed to remain a short period after death, gave the usual strong reaction of sugar that has been hitherto noticed. By injecting only a part of the organ with the alkali, it is most strikingly susceptible of demonstration, that the presence of sugar is in reality due to a *post mortem* occurrence, and can therefore be no longer looked upon as a representation of the natural *ante mortem* condition.

The sudden abstraction of heat from the liver instantly after death, leads to a similar arrest of the production of sugar, and thus enables us likewise to represent the real condition of the organ belonging to life. In one of the experiments mentioned, where a dog was sacrificed, and a piece of the liver instantly sliced off and thrown into a freezing mixture of ice and salt, the absence of sugar was almost complete; the amount at least was so small, that it was found impossible to arrive at a quantitative determination with a concentrated spirituous extract, notwithstanding the process is susceptible of so great a delicacy. The portion of the liver which was not submitted to the action of cold, and which was allowed to remain a short time in the animal, yielded on analysis an indication of 2·96 per cent. of sugar.

Division of the spinal cord in the lower part of the cervical region, the effects of which have been noticed by Bernard, but differently interpreted, leads to a corroboration of the deductions drawn from the preceding experiments. When the weather is cold or moderate, the operation is followed by a gradual reduction of temperature; and if the animal be sacrificed when its body has cooled down to about 70°, the liver is found free from sugar, upon an ordinary immediate examination, because at such a degree the *post mortem* transformation is not effected with sufficient rapidity to lead to our deception.

Placed aside, however, it soon becomes strongly saccharine. Should the operation of division of the cord be performed, and the temperature of the animal be afterwards maintained at about the ordinary height by exposure to external warmth, then the liver is as strongly saccharine upon ordinary examination after death, as if the animal had been taken and simply sacrificed.

By oiling the coats of rabbits and exposing them to cold, the temperature of the body falls, and precisely the same phenomena are noticed as after division of the cord.

With frogs in a vigorous condition, the presence or absence of sugar in the liver submitted to the ordinary process of examination after death, is dependent upon the temperature of the animal at the time of the destruction of life. This fact was independently noticed by myself about the time that it was mentioned by Bernard in a communication to the Parisian Academy of Sciences. Bernard's interpretation of it is connected with the relative activity of the abdominal circulation; but, for myself, I bring it forward as strongly supporting the views that have been advanced, and consider it to be explained by the influence of temperature on the *post mortem* production of sugar.

The material which occasions the presence of sugar in the dead liver, has been called by Bernard "Glucogenic matter,"—a term which, being only specially applicable after death, it is suggested should be abandoned, and replaced by Hepatine.

The amount of hepatine in the liver of the dog is much greater under a vegetable than an animal diet. The amount is also increased by mixing sugar with animal food. From the examples given, it is shown likewise that the relative weight of the liver presents a proportionate variation, according to the quantity of hepatine present. In eleven dogs taken indiscriminately, that had been restricted to an animal diet, the weight of the liver was one-thirtieth that of the animal. The average per-centage of hepatine yielded by eight livers, also taken indiscriminately after an animal diet, was 6·97. Five instances have been collected of dogs restricted to a vegetable diet for some days prior to death. The average weight of the liver was one-fifteenth that of the animal. In only three of the examples was the actual amount of hepatine determined, but in the other two it was noticed to be exceedingly large. The average given by the

three was 17·23 per cent. Four dogs were placed upon an animal diet, and about a quarter of a pound of ordinary cane-sugar administered daily for a short period. The average weight given by the four livers was one-sixteenth and a half that of the animal, and the average amount of hepatine yielded was 14·5 per cent.

The natural destination of hepatine in the living body remains to be determined. It has also to be shown how it resists transformation into sugar during life, when it is so rapidly changed at an elevated temperature immediately after death. A possible analogy may be presented by the following occurrence:—When a solution of hepatine, *in a neutral state*, is placed in contact with saliva, an almost instantaneous transformation into sugar takes place; but if a little acid alkali or carbonated alkali be added, scarcely a trace of change is for some time discoverable.

Under normal circumstances, rarely an appreciable amount of sugar is encountered in the circulatory system—only, according to my analyses, from about ·047 to ·073 of a grain in 100 grains of defibrinated right-ventricular blood; and this would appear to result rather from a simple escape of a small amount of hepatine from the tissue of the liver into the blood whilst circulating through the capillaries, than from a special functional operation of the organ; for when a disturbance of the circulation, whether by congestion or the opposite, is occasioned, sugar makes its appearance to a considerable extent in the system, because the admixture of hepatine with the blood is favoured. It can be easily shown by experiment, that on introducing hepatine into the circulatory system, a saccharine state of the blood is induced, and if enough have been employed, a strongly marked diabetic condition of urine is established.

Sacrificing an animal and maintaining the circulation by performing artificial respiration, occasions a well-marked diabetes. With the destruction of life, the transformation of hepatine into sugar takes place, and this, being carried away by the blood, is eliminated by the kidneys, and thus renders the urine strongly saccharine.

Many phenomena which were before obscurely explained, receive a lucid interpretation from the new facts which have now been brought to light.

XI. "On the Properties of Electro-deposited Antimony" (continued). By GEORGE GORE, Esq. Communicated by Dr. TYNDALL. Received June 1, 1858.

(Abstract.)

In this paper the following additional information is given respecting this singular substance.

The change observed in it is shown not to be an exercise of the force of cohesion, because the amount of heat evolved by the powdered metal is not sensibly different from that set free by the substance in its coherent massive state.

The thermic discharge is not limited to a particular temperature, but commences between 170° and 190° Fahr., and increases in rapidity to some point above 212° Fahr., when it becomes sudden.

The heat may be discharged either suddenly or gradually, according to the amount to be discharged in relation to the amount of cooling influences present.

The specific heat of the unchanged metal was found to be= 0·06312; and of the same specimens, after being *gradually* discharged, the specific heat was not sensibly different. But the specific heat of the substance, after *sudden* discharge, was found to be=0·0543.

The total amount of heat evolved by the substance during its change was sufficient to raise the temperature of its own weight of ordinary antimony (sp. heat=0·0508) about 650° Fahr.

The evolution of vapour which generally occurs during the change is a result of the molecular heat acting upon the terchloride of antimony contained in the substance. It occurs when a sufficient temperature is produced either by internal or external causes, and does not occur when the molecular discharge is gradual and the temperature is not sufficiently raised; in such cases the weight of the substance remains unaltered.

The substance, as usually produced from ordinary muriate of antimony, or from a mixture of that substance and tartar-emetic, contains small quantities of nearly all the ingredients and impurities of the depositing liquid.

The pure substance deposited upon sheets of platinum, in a solution of pure hydrochloric acid three-fourths saturated with pure

oxide of antimony, with an anode of pure antimony, exhibited no material difference in properties from the less pure variety.

Two analyses of the pure unchanged substance gave the following per-centages:—

	No. 1.			No. 2.	
Sb	93·36		Sb	93·51	
$SbCl^3$	5·98	$\}=6·44$	$SbCl^3$	6·03	$\}=6·24$
HCl	0·46		HCl	0·21	
	99·80			99·75	

A trace of water contained in them was not estimated.

Solvents removed the chloride of antimony from the powdered substance much more readily *after* the thermic discharge than *before* it.

Differences of physical appearance were detected in the changed and unchanged substance in the state of powder under a microscope; the surfaces of the latter were *smooth* and brilliant, whilst those of the former were *granular* and less bright. No mechanical mixture could be detected in the changed powder.

From the various experiments detailed in the paper, it appears that the substance in question is a feeble chemical compound of antimony and acid hydrochlorate of terchloride of antimony, apparently in variable proportions, decomposable by heat, and that the change observed in it, in cases of *gradual* discharge, consists of a molecular alteration, attended by weakened chemical affinity, and by evolution of heat; but in cases of *sudden* discharge the evolved heat produces a partial chemical decomposition, which is of greater or less extent, according to the temperature acquired.

A portion of the powdered unchanged substance, digested sixty-three days with an aqueous solution of caustic potash, lost 2·95 per cent. in weight, but still retained about $\frac{3}{4}$ths of its heating power. A second portion, digested fifty-six days with strong hydrochloric acid, lost 6·66 per cent. and all its heating power.

Exposure to light did not destroy the heating power of the powdered substance.

By depositing the grey variety of antimony into mercury, a pasty compound of the two metals was formed. The amorphous variety did not combine with mercury under similar circumstances.

An acid solution of fluoride of antimony yielded by electro-deposition grey crystalline antimony not possessing the heating power.

XII. "On the Action of Bile upon Fats; with Additional Observations on Excretine." By W. MARCET, M.D., F.R.S., Assistant Physician and Lecturer on Chemistry to the Westminster Hospital. Received June 10, 1858.

(Abstract.)

Having formerly observed and communicated to the Société de Biologie of Paris, that by heating a solution of neutral tribasic phosphate of soda ($2NaO . HO . PO_5$) mixed with animal fatty acids, an emulsion was obtained attended with the formation of a small quantity of soap, while no such action occurred if neutral fats were used instead of fatty acids, I was induced to inquire into the nature of the action of bile on neutral fats and fatty acids (sheep's bile being used), with the final object of throwing, if possible, some additional light on the digestion of fats. These investigations led to the following results:—

1. A mixture of bile and neutral fats (stearine, oleine and margarine), heated to a temperature above the fusing-point of the fat, undergoes no change, and no chemical action takes place.

2. A mixture of bile and fatty acids (stearic, oleic, and margaric acids), heated to a temperature above the fusing-point of the fatty acids, is transformed into a solution, a very few and minute globules only of fat remaining unacted upon from the presence of oleic acid. This solution becomes a perfect emulsion on cooling, and is attended with a chemical decomposition of the bile; and further, if the emulsion of bile and fatty acids be filtered when quite cold, and the residue on the filter thoroughly washed with distilled water, the filtrate and washings mixed together again possess the property of forming an emulsion with another quantity of fatty acids, being also at the same time partly decomposed, although in the previous operation the bile appeared to have exhausted its power on the fatty acids. The filtrate and washings from this second operation again act upon a fresh quantity of fatty acids, and so on; only in every subsequent operation the proportion of emulsion obtained appears to diminish, and the induced chemical decomposition to be lessened.

3. Pure oleic acid, when agitated with bile, cold or hot, produces no emulsion or chemical action whatever.

4. The stomach during digestion has the power of decomposing

the fats contained in the food into fatty acids, fats acquiring thereby the property of being acted upon chemically by the bile, and of being transformed into an emulsion.

The chemical action, or saponification, induced by the fatty acids under the above circumstances, was proved by the mixture acquiring a strong acid reaction; and it was further observed that the acid filtrate from the cold emulsion was not precipitated by hydrochloric acid, showing apparently that fatty acids exert on bile a chemical decomposition at least as extensive as hydrochloric acid. With the view of determining precisely the amount of soap formed, a method of analysis was adopted calculated to indicate the proportion of fatty acid remaining unacted upon by the bile: the difference between the fatty acids used and the result of the above operation was equal to the weight of the fatty acids saponified. It was found, in three analyses, that the mixture of bile and fatty acids being exposed for three hours (in Analysis II. for $3\frac{1}{2}$ hours) to the heat of an open water-bath, contained an amount of soap in which the proportion of fatty acids was 30·21 per cent., 20·5 per cent., 11·5 per cent. of that employed in the analysis. The filtrate from the emulsion in analysis No. II., mixed with the solution obtained by washing the emulsion with distilled water, was treated for three hours on the water-bath with a fresh quantity of fatty acids, which operation yielded a proportion of fatty acid saponified equal to 12·7 per cent. of that used in the analysis. Finally, the filtrate and washings obtained in this last group were mixed with another quantity of fatty acids, and exposed for three hours to the heat of the water-bath, in which case the proportion of fatty acid saponified was equal to 3·8 per cent. of that used in the analysis. The various operations had been attended with the formation of an emulsion.

In order to be certain that, after exposing a mixture of bile and fatty acids to the heat of a water-bath for three hours, the chemical action thus induced was completely exhausted, two analyses were undertaken according to the process just mentioned, and with bile from the same gall-bladder; but in one operation the mixture was heated for three hours, and in the other for six hours: the proportion of fatty acid saponified was the same in both cases, showing that after three hours the bile had ceased to act on the fatty acids.

Having obtained the above results, an inquiry was next undertaken

respecting the state of the fats of food in the stomach during digestion. For this purpose the contents of the stomach of several dogs, fed with cooked meat and neutral sheep's fat, were examined at different stages of digestion; the acids of the stomach soluble in water were removed by protracted washings with distilled water, and the residue being treated with alcohol and ether, yielded solutions found to contain fatty acids. In some cases the contents of the stomach were first treated with alcohol, and the fatty matters thus obtained subsequently washed with distilled water, and finally again dissolved in alcohol and ether. These analyses constantly yielded fatty acids, which, when heated with fresh sheep's bile, were found to dissolve and produce an emulsion.

In order to determine whether the cooking of the meat with which the dogs had been fed had transformed any of the neutral fats into fatty acids, a sample of roast meat was mixed and washed with distilled water until the washings had completely lost their acid reaction; the meat was then mixed with alcohol and allowed to stand for more than a week. After that time the fluid was found to be perfectly neutral, showing that no fatty acids had been formed.

From these researches it appears that the presence of bile in the intestines is closely connected with the digestion of fats.

The results of recent investigations on excretine show that this substance exists on an average in the proportion of 0·460 grm. for one evacuation when the excretine is impure, and of 0·184 grm. when it is pure. From the careful examination of the fæces of a child one year old, I have ascertained that they invariably contained no excretine, but cholesterine; the proportion of the latter, purified by repeated crystallizations, being equal to 0·036 grm. in one evacuation, which number is, however, a very low estimate. Nothing in the food could account for this singular result. It is therefore most probable that excretine is only present in the evacuations of the full-grown or adult individual.

I have been most ably aided in these investigations by my assistant, Mr. Frederick Dupré, Ph.D.

XIII. "Further Remarks on the Organo-metallic Radicals, and Observations more particularly directed to the isolation of Mercuric, Plumbic, and Stannic Ethyl." By GEORGE BOWDLER BUCKTON, Esq., F.R.S. Received June 17, 1858.

Before again entering on the subject of the organo-metals, the author wishes to call attention to the remarks he has previously made* on the difficulties which presented themselves at that time in the preparation of mercuric ethyl. Secondary decompositions, induced by the nature of the materials employed and the high temperature necessary to the reaction, showed themselves even in the more easily prepared mercuric methyl, and reduced the quantity obtained considerably below that pointed out by theory.

The loss sustained in the similar operation of distilling together cyanide of potassium and iodide of mercurous ethyl, $C_4 H_5 Hg_2 I$, is yet more marked; and it may be remembered that the portion obtained did no more than suffice for a cursory examination of its most marked characters. A new mode of operating was therefore desirable, and it was not long before the following considerations presented themselves.

The powerful and well-defined affinities of zinc-ethyl have already furnished a valuable key to the explanation of several chemical problems, and seem to be well suited for experiment in the present case. Bearing in mind its well-known reactions on water and hydrochloric acid, there appeared to be well-grounded reasons for supposing that interesting decompositions might be effected with various oxides, chlorides, and iodides.

Through the instrumentality of zinc-ethyl the author has succeeded in isolating, in a neat and efficient manner, several of the organo-metals, and he indulges a hope that they may, when taken as starting-points of investigation, prove of service in fixing exact formulæ to some of those bodies, the composition of which, at present, appear doubtful from their complexity.

Action of Zinc-ethyl on Mercuric Chloride.

Corrosive sublimate acts with great energy on zinc-ethyl; so much

* Phil. Trans. Roy. Soc.; Proc. Roy. Soc. vol. ix. p. 91.

so, as to render it necessary to cool the apparatus in water, and add the well-dried salt by degrees. An excess of the latter must be avoided, since chloride of mercurous ethyl would be formed, as was formerly shown to be the case in the methyl series.

After the two bodies have been brought together in their proper proportions, heat is applied, and the radical passes over by distillation as a heavy, colourless, and nearly inodorous liquid; the slight excess of zinc-ethyl is then decomposed by the addition of water, and just sufficient dilute hydrochloric acid added as will dissolve the precipitated oxide of zinc.

The two transformations may be seen in the equations,

$$C_4 H_5 Zn + Hg Cl = C_4 H_5 Hg + Zn Cl,$$

and again,

$$C_4 H_5 Hg + Hg Cl = C_4 H_5 Hg_2 Cl.$$

The pure radical boils at a temperature between 158° and 160° C. It burns readily, with a luminous and somewhat smoky flame, with disengagement of mercurial vapour. It is almost wholly insoluble in water. Alcohol dissolves it rather sparingly, but it mixes freely with ether.

The behaviour of acids towards mercuric ethyl is strictly analogous to that shown by mercuric methyl. With dilute acid there is but little change, but warm concentrated hydrochloric or sulphuric acid liberates hydride of ethyl in sufficient quantity to permit of its inflammation through a gas jet. The salts of mercurous ethyl remain in solution.

The specific gravity of a specimen boiling between 158° and 160° C. was found to be 2·444, and the same sample when submitted to analysis, gave numbers agreeing accurately with the formula

$$C_4 H_5 Hg.$$

The correctness of this formula was further confirmed by an appeal to the vapour-density.

The first experiment failed, from the circumstance that the vapour decomposes with a slight explosion, when heated a few degrees above 205° C. In this experiment metallic mercury was deposited on the walls of the glass balloon as a grey film, and the other contents consisted of an inflammable gas. Mercuric methyl appears therefore to be resolved at this temperature into ethyl gas and mercury.

Another experiment was more successful, and gave the number 9·97 for the vapour-density.

The equivalent weight of mercuric ethyl is 129, which, being divided by the former figures, gives $\frac{129}{9 \cdot 97} = 12 \cdot 94$. If the constituents of this radical be condensed into two volumes of vapour, the more accurate number 14·86 should have been obtained.

The theoretical density of mercuric ethyl, thus calculated, is equal to $\frac{129}{14 \cdot 86} = 8 \cdot 68$*.

This portion of the subject would be incomplete unless a few words were added on the behaviour of zinc-ethyl towards mercurous chloride.

It has been mentioned, that all attempts to reduce iodide of mercurous methyl to the form of a radical containing one equivalent of methyl and two equivalents of mercury have hitherto failed.

Reasoning *a priori*, we should not expect to find a departure in the present case, neither does such appear. Mercurous chloride reacts with vigour on zinc-ethyl, but metallic mercury is formed simultaneously with chloride of zinc and mercuric ethyl.

The decompositions of mercurous and mercuric chlorides or iodides, are thus shown:—

$$C_4 H_5 Zn + Hg_2 Cl = C_4 H_5 Hg + Zn Cl + Hg,$$

and

$$C_4 H_5 Zn + Hg Cl = C_4 H_5 Hg + Zn Cl.$$

Having succeeded, by these simple means, in effecting a replacement in zinc-ethyl through the ordinary metallic chlorides, there remained yet one point untouched, viz. the behaviour of various organo-metallic salts, under similar treatment.

First in order was tried

* Here it is fitting to mention an error that has crept into the calculation of the vapour-density of mercuric methyl as it appears printed in the 'Proceedings of the Royal Society.' A false figure in the denominator of one of the fractions, causes the experimental density to appear as 14·86, whereas the true experimental density observed was 8·29. The theoretical density of mercuric methyl calculated for two volumes, equals $\frac{115}{14 \cdot 46} = 7 \cdot 95$.

The Action of Zinc-ethyl on Iodide of Mercurous Ethyl.

Carbonic acid, or ordinary coal-gas, was slowly passed through the neck of a retort; and when the atmospheric air was displaced, about two ounces of zinc-ethyl, nearly free from ether, and wholly so from iodide of ethyl, was introduced. Iodide of mercurous ethyl was then added, by degrees, through the tubulure, and the whole mixed by agitation. The zinc-ethyl at first dissolves the iodide, but subsequently a cake of iodide of zinc is formed. Distillation was then commenced, the heat being raised by degrees until gaseous products appeared. The distillate, after being well washed, was rectified by the thermometer, and in this manner the radical was obtained in a state of purity. Iodide of mercurous ethyl may be formed so easily by diffused daylight, and its action is so gentle on zinc-ethyl, that its use offers greater conveniences to the operator than are afforded by any of the substances previously mentioned.

For obvious reasons, a similar choice of materials is recommended for preparing mercuric methyl.

Action of Zinc-ethyl on Chloride of Lead.

The close relations which exist between the three metals, lead, mercury, and silver, in their equivalent weights, salts, and other characters, lead the author to anticipate success in forming their ethyl bases.

The existence of the lead radical might indeed be considered as certain, since various salts of complicated structure have been made known to chemists through the experiments of M. Löwig, on the alloy of lead and sodium, under treatment with iodide of ethyl.

The principal product obtained by him, and the only one apparently analysed, had a grouping similar to a sesquichloride. The formula ascribed by him to the radical plumbethylium is $Pb_2\,(C_4H_5)_3$. I have attempted to form the iodide of this radical by exposing sealed tubes, containing granulated lead and iodide of ethyl, to the sun's rays, but without success. No better result was obtained by substituting bromide of ethyl for the iodide, and no change could be induced even when these tubes were heated strongly with high-pressure steam.

M. Löwig's method was not resorted to, from the supposition that the action of zinc-ethyl on a mixture would only give rise to radicals of

various constitution, which it might be impossible afterwards to separate, except by working on a large scale, which, considering the costliness of the materials, had its disadvantages. Perhaps success might attend the use of one of Dr. Frankland's mirrors for concentrating the sun's rays.

For obtaining the lead-radical, recourse was had to well-dried chloride of lead, which was introduced into a flask containing zinc-ethyl. The chloride immediately turned black, from the deposit of metallic lead, whilst moderate heat was disengaged. An excess of chloride was used, and the mass incorporated by stirring with a glass rod. After applying a gentle heat for a few minutes, the floating clear liquid was pipetted off. This substance is apparently a compound of zinc-ethyl and the lead radicals. It fumes slightly in the air, and no digestion with chloride of lead appeared to resolve it entirely into the lead base.

A great part of the zinc-ethyl, however, is removed by subsequent distillation; but the temperature should not be permitted to rise above 140° or 150° C. The substance in the retort is then treated with water and dilute hydrochloric acid, when the radical separates, and sinks in the form of colourless drops. When distilled cautiously, the thermometer soon rises to 200°; but beyond this point the vapour is very prone to decomposition, with deposit of metallic lead.

From this tendency to change, there is some difficulty in obtaining the substance wholly pure from bodies with lower boiling-points. The larger portion came over between 198° to 202°. Its specific gravity was found to be 1·55.

Analysis led to the formula

$$Pb\,C_8\,H_{10},\ \text{or}\ Pb\,(C_4\,H_5)_2.$$

It should, however, be noticed that a trifling excess in the percentage of carbon obtained, showed an increase rather than a decrease in the number of equivalents of ethyl.

This radical, for which the provisional name of plumbic bis-ethyl is suggested, is a colourless fluid, possessing little or no odour. It is insoluble in water, but perfectly miscible with ether. It burns readily with a beautiful orange-coloured flame, edged with blue, and gives off fumes of oxide of lead.

The radical appears to be incapable of forming salts without a

partial decomposition. With weak acids there is no perceptible action; but when they are concentrated and gently heated, a gas is given off, and crystalline salts are produced.

The chloride is insoluble in water, but soluble in alcohol and in ether, from which last liquid it crystallizes in satiny needles, which are very volatile and provoke sneezing and lachrymation. It burns with the characteristic lead flame, and by long digestion with concentrated hydrochloric acid, is converted into chloride of lead and volatile products.

The sulphate also appears as a crystalline mass when plumbic-bis-ethyl is gently warmed with a few drops of concentrated sulphuric acid. It is conveniently prepared by agitating the materials in a stoppered bottle, an exit being made from time to time for the gas which is liberated.

Both these salts require analyses to fix their composition, the details of which the author hopes shortly to be able to communicate.

The Action of Zinc-ethyl on Chloride of Silver.

These substances react with some violence, and a black substance sinks in the liquid, which proved to be a mixture of chloride and metallic silver. The zinc-ethyl seems partly to escape decomposition, even when the chloride is in excess and considerable heat is applied. On the addition of water, effervescence sets in, and chloride of zinc is alone found in solution.

In another experiment dry ether was employed instead of water, under a supposition that a solid compound might be formed, soluble in that menstruum. The only reaction, however, appeared to be that expressed by the equation,

$$C_4 H_5 Zn + Ag Cl = Zn Cl + Ag + C_4 H_5.$$

A similar negative result was obtained when zinc-ethyl was made to react on protochloride of platinum, $Pt Cl$. The action is violent, and the platinum is thrown down in the form of platinum-black.

The same remark also applies to protochloride of copper, $Cu_2 Cl$, when similarly treated; no combination of copper and ethyl could be thereby eliminated.

Action of Zinc-ethyl on Iodide of Stan-ethyl.

This iodide, $C_4 H_5 Sn, I$, was readily obtained by heating sealed tubes containing excess of tinfoil and iodide of ethyl from 150° to 160° C. The pure transparent crystals which were obtained by a little management, were introduced, in a melted state, into a retort containing zinc-ethyl. It is necessary to cool the apparatus with water. After breaking up the resulting mass, the retort was heated until the thermometer marked 210° C., and the distillate, which contained a slight excess of zinc-ethyl, was agitated with water, and treated with dilute acid, as before described.

The resulting heavy liquid was again distilled, and fractionized with the thermometer. By far the larger portion came over between 170° and 180° as a clear and colourless body, insoluble in water, but soluble, like the other radicals, in ether. That section which possessed a boiling-point between 176° and 180° C., was taken for examination, and was found, when burned with oxide of copper, to give the formula

$$Sn\,C_8\,H_{10}, \text{ or } Sn\,(C_4\,H_5)_2.$$

This compound, for which the name stannic bis-ethyl is proposed, has a specific gravity of 1·192. In its external and more prominent characters it resembles plumbic bis-ethyl; but an exception may be made, that it is more stable. It is very combustible, burning with a coloured flame and scintillation like that exhibited by the metal tin under the flame of the hydro-oxygen blowpipe.

This radical appears to differ in several particulars from the organo-metal stan-ethyl, $C_4 H_5 Sn$, obtained by Dr. Frankland by acting on sheet-zinc with a salt of stan-ethyl. This last body is described as a thick, oily substance, possessed of a powerful odour, and having a specific gravity of 1·55. It differs also in its lower boiling-point, which is about 150° C.

Pure stannic bis-ethyl is perfectly limpid, inodorous, and is acted upon by hydrochloric acid with difficulty. A gas is slowly evolved on the application of heat, and a chloride is formed which seems to be richer in tin than the radical itself.

The chloride appears to crystallize with difficulty, and at usual temperatures has the consistence of an oil. It possesses a powerfully pungent odour, and when heated, a vapour which painfully attacks the skin of the face, and produces fits of sneezing.

A corresponding bromide is formed when bromine is added to stannic bis-ethyl. It is an oily body, with an irritating odour. When acted upon by ammonia, an oxide is precipitated, which with acids forms beautiful crystallizable salts, readily soluble in water.

A complete history of these salts, and their decompositions with zinc-ethyl, will possess much interest, and may prove of value in referring to a few simple radicals the numerous complex bodies described by Löwig, &c.

The author is at present engaged on this branch of the inquiry, a detailed account of which he hopes to embody in a communication to the Royal Society, the present paper being intended only as an outline to be hereafter filled in.

In conclusion, the author would remark that a rich harvest can scarcely fail to be reaped, from submitting to the action of zinc-ethyl the metallic compounds of other groups, such as arsenic, bismuth, and antimony.

XIV. "Preliminary Notice of Additional Researches on the Cinchona Alkaloids."—Part III. By W. Bird Herapath, M.D. &c. Communicated by Professor Stokes, Sec. R.S. Received June 17, 1858.

Since the author had the honour of presenting to the Royal Society his paper entitled "Researches on the Cinchona Alkaloids," Parts I. and II., he has been much occupied with a continuation of the subject, and he has arrived at important results, which, although in an unfinished state, he hastens to lay before the scientific world, in order to assure himself of the priority of discovery.

Having had occasion to make some experiments upon the rotatory power of the β-quinidin mentioned in the first part of his paper, he arrived at the conclusion that some other feebly dextro-gyrate alkaloid accompanied it, and of a more soluble and less crystallizable character. Consequently, on its further purification by frequent recrystallization from alcohol, the quinidin was obtained perfectly pure; it then had the molecular rotation assigned to it by Pasteur, namely $250°\cdot75 \nearrow$. Two examinations have given the following elements:—

I. Its solution having been made in rectified spirit of ·836 by boiling,

and crystallized at 62° F., the concentrated solution decanted gave the following elements for Biot's formula:—

ϵ.	δ.	l.	Arc blue violet.	
·02728	·85172	315·8	$18°{\cdot}5 \nearrow =$	$251°{\cdot}7 \nearrow$

II. Its sulphate, perfectly neutral, and concentrated at 61° F.:—

ϵ.	δ.	l.	Arc.	
·0088441	1·00406	315·8	$7° \nearrow =$	$249°{\cdot}55 \nearrow$

These observations were all made with the naked eye, and the tint of passage was that of the blue-violet petal. When the pink violet, or lilac tint was employed, the arc observed was 20°·25 for No. I. experiment, which with the same elements of calculation gave $274°{\cdot}093 \nearrow$; and with No. II., the arc 25°·75, which, as before, gave $279°{\cdot}7 \nearrow$. The slightly dextro-gyrate alkaloid existing as a contamination was quinicine; and upon its removal, the β-quinidin had the same solubility in ether as the quinidin of Pasteur. One very peculiar circumstance elicited during this examination, was the fact that the perfectly pure recrystallized quinidin, if made into the neutral sulphate and crystallized by cooling, produces, if made with distilled water at 212° F., a slightly greenish solution, however great the care which might have been taken to remove all the mother-water by washing the crystal on the filter. This green tint deepens considerably during concentration, or by boiling, and at length gives rise to the erroneous impression that some salt of copper is present: in this condition, a tube having a length of 315·8 millims., when filled with the solution, is absolutely impervious to light. It is probable that some molecular change is produced by the action of boiling, even if only for a short time; therefore it was necessary to make a concentrated solution at 120° F., and set in repose for some days at 62° F., by which precaution the solution experienced only a very slight discoloration. When formerly experimenting on β-quinidin, the author obtained an iodo-sulphate very different from that which he has described as indicative of the quinidin of Pasteur: having pursued this inquiry, he is now enabled to state that his former discrepancies arose from the fact that quinidin forms two iodo-sulphates, according to the manner in which it is treated.

1st. When a dilute solution of the acid sulphate of quinidin is

mixed with one-third or one-half its bulk of rectified spirit and raised to 160° or 180°, then treated with tincture of iodine in small quantities, the red iodo-sulphate is produced, having the characters previously described as indicative of quinidin,—quinine, when similarly treated, invariably producing the optical salt.

The only precaution necessary to be taken in the case of the alkaloid quinidin is to avoid adding an excess of iodine; otherwise an amorphous resinoid substance is deposited which will not crystallize.

2ndly. But when we treat the acid sulphate of quinidin in a concentrated form, diluted with from thirty to forty times its bulk of rectified spirit at a temperature from rather below 120° F., with the tincture of iodine, even in greater proportions, an optical salt of quinidin is produced, being the perfect analogue of the quinine salt.

It crystallizes from this strong spirituous solution as acicular long lanceolate prisms, the form of which appears to be a rhomboid having 30° for the acute and 150° as the obtuse angles; but they are more frequently found with a termination like the blade of an ordinary bleeding-lancet. These prisms have a frequent disposition to hemitropism, but in superposition, so that two plates may be often found *overlying each other in a parallel position*, wholly obstructing light in those portions *where they cover each other*, but transmitting an olive- or yellowish-green tint where separate.

Sometimes the terminal planes are rectangular. This imbricated mode of crystallization is very peculiar; and although the author has made thousands of experiments with quinine, yet he never saw anything similar to it; for this alkaloid invariably crystallizes from *dilute alcoholic* solutions as the α-prism, obstructing light when the length is perpendicular to the plane of reflected light polarized in a vertical plane,—or from *strong* alcoholic solutions, where the spirit is about two-thirds the bulk, as β-prisms, which obstruct light in the opposite plane, or, as the author has described it, when the planes of their length "lie in a plane parallel to that of the polarized beam with which they are examined." In the case of quinine, these two sets of prisms never occur together; but if made by separate operations *and then artificially mixed on the same slide*, they present the optical characters of this new quinidin salt, viz. *obstructing light when two long prisms overlie each other in a parallel position.* They are there-

fore α- and β-prisms crystallizing together from the *same strong* alcoholic solution.

The more frequent form in which this salt shows itself, however, is as the α-prism, from solutions in which the alcohol is vastly predominant over the water; whereas with quinine, β-prisms always develope themselves under similar circumstances (*vide* 'Proceedings,' vol. vi.). This new quinidin salt has a very great similarity in its optical property to the quinine salt. Its reflected tint is a metallic blue-green, when in liquid or in contact with glass; but after filtering, and when exposed on paper, it has a brownish-olive colour, and loses all appearance of metallic reflexion to the naked eye. Its transmitted tint is, when polarized parallel to its axis, a brownish-yellow green, even in thin plates, but verging to brown in thicker. Its "indicative body" colour is brownish red.

One great peculiarity attends upon this salt; if it be permitted to remain in the acid mother-liquid, it disintegrates by gradual solution, and disappears, whilst, upon the side of the bottle, solid and large crystals slowly form, of a rhombohedric form, or having some of its modifications, the more frequent of which is that with replacement upon the short axis of the rhombohedron by triangular planes. These crystals have a deep sienna-brown colour by transmission, and a dark steel-blue by reflexion, verging on purple; they strongly polarize light, and differ materially from the garnet-red iodo-sulphate previously described, by the greater intensity of their optical properties.

When we attempt to purify the optical thin prisms by recrystallization from alcohol, the same modification appears to be produced; but the crystals are acicular rhombic prisms; the optical characters are the same, however, as those of the rhombohedral form.

The characters, therefore, by which this salt is known from quinine are many.

1st. Its crystallizing as α-prisms, or as α- and β-prisms from strong spirituous solutions.

2nd. Its brownish-olive reflected tint as seen by the naked eye.

3rd. Its deeper yellow and brownish-green transmitted tint.

4th. The probable difference in the primary form of the laminated variety, being a very acute prism of a rhombic form, having 30° as the acute, and 150° as the obtuse angles.

5th. The modification which it undergoes by resolution or recrystallization, and the formation of a salt more resembling the garnet-red iodo-sulphate, but having strongly marked differential characters from this beautiful salt, viz. its strong tourmaline powers of absorption and its deeper colour, being nearly a brown-purple, and by its disposition to assume the rhombohedric form.

The author has not yet analysed this salt, but hopes ere long to accomplish this matter and communicate his results to the Royal Society; but he ventures to hope that it will be found to contain 2 atoms sulphuric acid and 3 atoms iodine, like the analogous quinine and cinchonidin salts.

The author has also assured himself that there is an analogous class of salts produced by ethyle-quinine and ethyle-quinidin, but optically distinct from those of quinine and quinidin. He has already produced three salts from ethyle-quinine, having optical characters different from any previously described.

1st. A deep purple-red salt by transmitted light, in thicker plates or aciculæ quite impervious to light. This salt occurs as very slender acicular prisms; it has a brilliant metallic-green reflected tint, but very little double absorption.

2nd. There is a foliaceous salt, having a plate-like form, a deep red or orange-red colour, transmitting an orange-yellow, having only slight optical powers.

3rd. A salt having many of the characters of the new quinidin salt when first produced, viz. the optical characters and the α-form; but on attempting to recrystallize it, the orange-red plates just described are alone produced.

The only salt yet produced from ethyle-quinidin is one very similar to the red salt described above, but it has only been very partially examined. The iodide ethyle-quinidin is a very beautiful silky salt, less soluble than the iodide ethyle-quinine. The author is not aware that it has yet been described. It is readily made by mixing an alcoholic solution of quinidin with an etherial solution of iodide-ethyle; on repose, the new iodide ethyle-quinidin separates in long, slender, silky aciculæ; and further crops can be repeatedly produced by diluting the original solution with water until precipitation *begins* to follow; on long repose, the iodide crystallizes and may be removed by filtration, and washed with dilute spirit.

Note.—In reference to the rotatory power of the cinchona alkaloids, the calculation of the molecular rotation gives an excellent plan of deciding on *the purity* of the alkaloid employed; for if the absolute molecular rotation be obtained precisely identical with those given by other optical chemists, the purity may be inferred as proved. But it is possible for a large quantity of two alkaloids to be present in solution, one dextro-, the other levo-gyrate, and in such proportions that the polariscope shall give no indication of the presence of either.

Thus a highly concentrated solution of the acid sulphate of quinine, marking a left-handed rotation of 57°, was mixed with rather more than double its bulk of a similar solution of quinidin marking 24°. The resultant solution gave no rotation at all, the one effect perfectly neutralizing the other.

In experimenting upon *non-fluorescent* solutions of quinine or quinidin in the polariscope, it was found that these solutions were still possessed of their original molecular rotation upon plane-polarized light, even undiminished, if care were taken not to dilute the fluid when destroying the fluorescence by the soluble chloride, &c., which was always done by adding it in the solid state.

XV. "Sur la Relation entre les Courants induits et le Pouvoir Moteur de l'Electricité." By Professor CARLO MATTEUCCI of Pisa. Communicated by W. R. GROVE, Esq. Received May 20, 1858.

Dans la 1ère partie de ces recherches j'ai étudié l'influence des extra-courants induits sur le fil même de la spirale d'un électro-aimant, sur les propriétés électro-magnétiques et électrolytiques du courant qui met la spirale en action. Cette influence intervient nécessairement dans le jeu des moteurs électro-magnétiques, et la recherche de la corrélation des forces présentée par un de ces moteurs ne pourrait être complète sans pouvoir déterminer rigoureusement la quantité d'action chimique qui a lieu dans la pile. Voici les résultats que j'ai établi par des expériences exactes.

1°. Dans les expériences faites sans avoir les bobines de l'électro-aimant dans le circuit, la force électro-magnétique du courant est

approximativement la même quelque soit le nombre des interruptions, tandis que les quantités des produits électrolytiques sont proportionnelles à la durée de l'expérience ; mes résultats, d'accord avec les lois des courants électriques en général, font voir une petite différence entre l'hydrogène du voltamètre et celui calculé sur le poids du cuivre, qui par sa constance ne paraît pas être due à une erreur d'expérience.

2°. Lorsque les bobines de l'électro-aimant entrent dans le circuit, la force électro-magnétique du même courant et les produits électrolytiques deviennent beaucoup moindres, et cela proportionnellement à la vitesse de rotation du commutateur, ou au nombre des interruptions dans un temps donné. En comparant les résultats obtenus avec les mêmes vitesses du commutateur, avec et sans bobines, on trouve que la force électro-magnétique souffre une diminution plus grande que son action électrolytique, et que ces différences sont d'autant plus marquées que la vitesse de rotation du commutateur est plus grande.

3°. Avec les bobines dans le circuit, la quantité d'hydrogène du voltamètre n'est plus équivalente à la quantité de cuivre déposé sur les lames de platine de la pile ; l'hydrogène obtenu est d'autant moindre que le nombre des interruptions du circuit est plus grand. Les quantités de zinc qui sont dissoutes dans les mêmes expériences conduisent à la même conséquence.

4°. En tenant fermé le circuit des bobines induites, la force électro-magnétique et les produits électrolytiques augmentent, et à mesure qu'on diminue la vitesse de rotation du commutateur, le courant tend à se rapprocher au courant obtenu dans le circuit sans les bobines*.

Dans la 2ième partie de ces recherches j'ai étudié un cas présenté par un moteur électro-magnétique dont les électro-aimants sont formés de deux bobines superposées. Voici l'expérience principale. Je suppose de faire passer un courant dans une des bobines ; lorsque l'axe des armatures a pris une vitesse uniforme de rotation, on réunit les deux bouts de la seconde bobine, et au même moment on voit l'axe de la machine s'arrêter, ou ne tourner plus que très-lentement. En même temps les étincelles qui avaient eu lieu à chaque interruption du commutateur sont devenues à peine visibles. En ouvrant le cir-

* Déjà en 1854 (Cours sur l'induction, pages 11 et 31), j'avais signalé ce résultat et rapporté les nombres obtenus dans une expérience.

cuit de la spirale induite les étincelles reparaissent et l'axe de la machine reprend sa vitesse primitive. On peut varier l'expérience en ayant adapté un tambour de bois à l'axe de la machine de manière à obtenir l'élévation d'un poids. Je suppose qu'on ait déterminé le poids que la machine peut élever avec une certaine vitesse lorsque la spirale induite est ouverte : au moment où cette spirale est fermée, il faut pour faire tourner la machine avec la même vitesse, substituer un poids beaucoup plus petit au premier. En partant de ce résultat on comprend facilement comment on doit faire l'expérience pour déterminer l'équivalent mécanique de la chaleur. Il s'agit de mesurer le travail mécanique de la machine dans les deux cas, c'est à dire, à spirale induite ouverte et à spirale induite fermée, et de comparer la différence des deux nombres à la quantité totale de chaleur développée par les courants induits. Voici les nombres trouvés dans une expérience dans laquelle j'ai obtenu le *maximum* des différences entre le travail mécanique de la machine à spirale induite ouverte et le travail de la machine à spirale induite fermée. Dans le premier cas la machine a soulevé un poids de 473 grammes avec la vitesse du 58 secondes pour 10 mètres. La spirale induite étant fermée, le poids soulevé avec la même vitesse était reduit à 71 grammes. La différence de 0·402 kilogr. multipliée par 189 mètres d'élévation représente la différence cherchée, qui est égale à 75·98 kilogr. mètres, et qui doit être équivalente à 173·086 unités de chaleur développées par les courants induits. On tire de là pour l'équivalent mécanique de la chaleur le nombre 438·96, qui s'accorde suffisamment avec les nombres trouvés par d'autres observateurs dans des conditions bien différentes. Cette détermination fondée sur une expérience très-simple conduirait à des résultats rigoureux et constants si le dérangement du commutateur n'altérait pas la marche de la machine. Pour concevoir ces variations dans la marche de la machine qui dépendent de l'altération du commutateur il faut se rappeler que la force d'une machine électro-magnétique dépend de la durée du contact et du moment de l'interruption du commutateur. Ainsi pour obtenir la plus grande vitesse il faut que le circuit s'ouvre au moment que l'armature qui est attirée arrive tout près du bord de l'électro-aimant, ce qui fait que la machine peut se mouvoir indifféremment dans les deux sens suivant l'impulsion primitive. J'ai trouvé que dans cette position la diminution de la vitesse due à l'influence des courants induits est la

moindre possible. La différence augmente à mesure que par la position donnée au commutateur on laisse persister l'aimantation pour plus longtemps en présence de l'armature attirée, ce qui produit une diminution dans la force de la machine. J'ai pu de cette manière parvenir au *maximum* du travail mécanique à spirale induite ouverte et à spirale induite fermée. Cela nous aide à expliquer la manière d'agir des courants induits pour produire la diminution du travail mécanique de la machine. En effet dans la position du commutateur qui donne la plus grande différence on conçoit que pour peu que le contact et l'aimantation se prolongent, les armatures se fixent et la machine cesse de marcher. Or l'action de la spirale induite fermée produit nécessairement deux effets qui tendent à rallentir la désaimantation : le premier c'est l'augmentation du courant de la pile, et par conséquent la force magnétique plus grande et plus persistante des électro-aimants ; le second effet de l'induction c'est de neutraliser l'extra-courant négatif qui certainement rend plus prompte la désaimantation.

Enfin, ce qui rendrait ces expériences rigoureuses serait la détermination avec des calorimètres distincts de la quantité totale de la chaleur développée en même temps par la pile et dans les spirales de l'électro-aimant, le circuit induit étant tantôt ouvert tantôt fermée.

XVI. "On the Influence of the Gulf-stream on the Winters of the British Islands." In a Letter from Professor HENNESSY to Major-General SABINE, V.P. and Treas. R.S. Communicated by Major-General SABINE. Received May 24, 1858.

35 Upper Leeson Street, Dublin,
May 19, 1858.

MY DEAR SIR,—In your work on 'Pendulum Experiments,' and subsequently in a paper printed in the 'Philosophical Magazine' for April 1846, you have directed attention to the influence of the Gulf-stream on the winters of the British Islands. You have been led to attribute the remarkably mild winters which we sometimes experience, to an abnormal extension of the warm waters of that stream towards our latitudes. In this view I entirely concur, and beg to submit the following additional proof of its correctness.

An abnormal extension of the Gulf-stream in the direction of the

British Isles necessarily implies that the waters bathing our coasts acquire a temperature which exceeds their mean temperature for the season of the year at which the extension takes place. The temperature of the air over the sea, and finally of the air over the islands, becomes sensibly increased. The entire temperature at any point will thus depend chiefly on what it gains from sunshine, and from the warm sea-air, and on what it loses by radiation. If the excess of what it gains from sunshine over its losses by radiation be considerable compared to its gain from the influence of the sea, the temperature will depend principally on the latitude. If, on the contrary, the thermal influence of the sea be very considerable, places at different latitudes may possess nearly equal temperatures. It follows that during cold winters we should expect a greater difference between the temperatures of the southern coasts of Great Britain and Ireland, and the remainder of their coasts, than during mild winters. It also follows, that during warm winters the difference of temperature between stations situated on coast and inland stations having nearly the same latitude, should be greater than during cold winters.

Although I have not yet finished all the calculations necessary for the complete illustration of these conclusions, I have been enabled to show that during some recent winters the observed results as to temperature entirely conform to these laws.

The mildness of the winter which has just passed away, has been universally remarked, and Mr. Glaisher's returns for the meteorology of England and Scotland during December 1857 fully illustrate the matter. I have not yet received the returns for January and February, but I feel assured that they will exhibit corresponding results.

During December 1857 the temperatures of the coast stations were as follows:—

South Coast.		North and West Coasts.		East Coast.	
Helston	51°·2	(Orkney) Stornoway	46°·1	Aberdeen	44°·3
Truro	49·3	Elgin	45·3	Arbroath	43·8
Teignmouth	48·8	Liverpool	48·3	Pittenween	45·8
Ventnor	49·2	Isle of Man	48·9	N. Shields	45·6
Worthing	48·0			Scarborough	45·0
Hastings	47·3			Holkham	44·5
Ryde	46·9				
Mean	48·7		47·1		44·8

Mean of all the coast stations	47·0
Excess of south coast above north and west coasts	1·6
Excess of south coast above east coast	3·9
Mean excess of stations on the south coast above all the rest	3·0

December 1856.

South Coast.		North and West Coasts.		East Coast.	
Helston	46°·7	Stornoway	39°·5	Aberdeen	39°·6
Falmouth	45·8	Elgin	39·5	Arbroath	36·4
Truro	45·6	Liverpool	42·8	Anstruther	38·4
Teignmouth	43·6	Isle of Man	42·4	N. Shields	39·7
Torquay	44·5			Scarborough	40·9
Ventnor	43·9			Holkham	39·6
Ryde	43·0				
Worthing	41·0				
Hastings	41·8				
Mean	44·0		41·0		39·1

Mean of all the coast stations	41·8
Excess of south coast above north and west coasts	3·0
Excess of south coast above east coast	4·9
Mean excess of stations on the south coast above all the rest	4·1

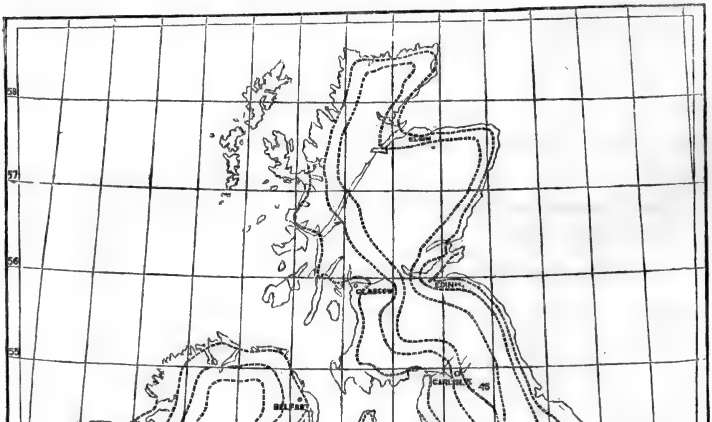
December 1855.

Helston	44°·6	Elgin	36°·9	Aberdeen	36°·7
Falmouth	43·6	Liverpool	38·9	Arbroath	35·1
Truro	43·3	Isle of Man	39·4	Anstruther	35·7
Torquay	40·6	Sandwick (Orkney)	39·4	N. Shields	35·9
Teignmouth	41·3			Scarborough	36·7
Ventnor	40·4			Holkham	35·8
Ryde	38·8			Boston	35·9
Worthing	37·7				
Mean	41·3		38·6		36·0

Mean of all the coast stations	38·8
Excess of south coast above north and west coasts	2·7
Excess of south coast above east coast	5·3
Mean excess of south coast stations above all the rest	4·35

The December of last year, which was much warmer than the Decembers of the two preceding years, appears thus to comply with such conditions as to temperature as would lead to the conclusion that a greater extension of the Gulf-stream had existed about the end of 1857, than towards the close of 1856 or 1855.

*Isothermals of the British Isles**.*



It will be interesting to compare the mean temperature of a southern inland station, where the observations may be depended upon as being of the best class. The mean temperature of Oxford during the December of 1857 was 45°·0; in December 1856, 40°·5; in December

* The figures denote the mean annual temperature, in degrees Fahr., corresponding to each isothermal line.

1855, 37°·2. All the stations on the west coast are situated in higher latitudes, yet their mean temperature was in excess of that of Oxford in December 1857 by 2°·1; in the colder Decembers of 1856 and 1855, by 0°·5 and 1°·4 respectively. During the warmest month, the mean of all the coast stations exceeded the temperature of Oxford by 2°·0; and during the other two Decembers by 1°·3 and 1°·6 respectively.

I propose to make more complete calculations, which will embrace the other months belonging to the winter; and by comparing the results during different years, it is probable that corresponding inferences will be suggested regarding the variations of mean temperature which are incapable of explanation by changes of solar radiation alone.

I was induced to select December at first, because the amount of sunshine received in our hemisphere being least during that month, it was natural to expect that the comparative effects of the other thermal influences would be most distinctly manifested.

Having been for some time occupied in studying the distribution of heat over islands, I have been led to the general proposition, that the isothermals may be represented by curves having some relation to the coast-line, and that the positions of the centres of these curves depend upon the relation between solar influence and oceanic temperature. At seasons when the latter becomes important, compared to the former, the isothermals tend to assume re-entrant shapes similar to the mean annual isothermals of Ireland. When the isothermals of a mild winter month, like December of 1857, shall be laid down, I anticipate that they will distinctly exhibit the increased thermal influence of the ocean by presenting such an appearance.

HENRY HENNESSY.

Major-General Sabine, V.P.R.S.

XVII. "On the Influence of Temperature on the Refraction of Light." By Dr. J. H. GLADSTONE, F.R.S., and the Rev. T. P. DALE, M.A., F.R.A.S. Communicated by Dr. GLADSTONE. Received June 17, 1858.

(Abstract.)

Those who have occupied themselves with the determination of refractive indices, must have noticed that changes of temperature

influence the amount of refraction; yet few of the observations on record have affixed to them the temperature at which they were made, and few, if any, numerical researches have been published on the subject. To determine, if possible, the amount and character of this effect of heat was the object of the present inquiry.

The instrument employed was that described by the Rev. Baden Powell in the British Association Report for 1839, and was kindly lent by him for the purpose. The substances more or less fully examined, were bisulphide of carbon, water, ether, methylic, vinic, amylic, and caprylic alcohols, the two principal constituents of creasote—hydrate of phenyle and hydrate of cresyle, phosphorus, oil cassia, and camphor dissolved in alcohol.

Of the tabulated results the following two will suffice to illustrate the main conclusions:—

Bisulphide of Carbon.

Temperature.	Refractive Index of A.	Refractive Index of D.	Refractive Index of H.	Difference per 5° C. for D.	Length of spectrum.	Dispersive power.
0° C.	1·6217	1·6442	1·7175	·0045	·0958	·01487
5	1·6180	1·6397	1·7119	·0051	·0939	·01468
10	1·6144	1·6346	1·7081	·0043	·0937	·01477
15	1·6114	1·6303	1·7035	·0042	·0921	·01462
20	1·6076	1·6261	1·6993	·0041	·0917	·01463
25	1·6036	1·6220	1·6942	·0040	·0906	·01460
30	1·5995	1·6180	1·6896	·0040	·0901	·01457
35	1·5956	1·6140	1·6850	·0037	·0894	·01456
40	1·5919	1·6103	1·6810	·0050	·0891	·01460
42	1·5900	1·6083	1·6778		·0878	·01443

Water.

Temperature.	Refractive Index of A.	Refractive Index of D.	Refractive Index of H.	Difference per 5° C. for D.	Length of spectrum.	Dispersive power.
0° C.	1·3293	1·3330	1·3438	·0001	·0143	·00429
5	1·3291	1·3329	1·3436	·0002	·0145	
10	1·3288	1·3327	1·3434	·0003	·0146	·00439
15	1·3284	1·3324	1·3431	·0004	·0147	
20	1·3279	1·3320	1·3427	·0003	·0148	·00446
25	1·3275	1·3317	1·3420	·0006	·0145	
30	1·3270	1·3309	1·3415	·0006	·0145	·00438
35	1·3264	1·3303	1·3410	·0009	·0146	
40	1·3257	1·3297	1 3405	·0008	·0148	·00449
45	1·3250	1·3288	1·3396	·0009	·0146	
50	1·3241	1·3280	1·3388	·0012	·0147	·00448
55	1·3235	1·3271	1·3380	·0010	·0145	
60	1·3223 ?	1·3259	1·3367		·0144	·00442
65	1·3218	1·3249		·0012	·0138	
70	1·3203	1·3237	1·3344	·0012 (A)	·0141	·00435
80	1·3178	....	1·3321		·0143	

The following are the conclusions arrived at:—

1. In every substance the refractive index diminishes as the temperature increases. This is seen in the first four columns of the tabulated results, which represent the refractive indices of the fixed lines of the spectrum A, D, and H respectively at the temperatures indicated, while the succeeding column shows the amount of difference for each five degrees Centigrade. This change of refractive index by heat, for which the term *sensitiveness* is proposed, varies greatly in amount in different substances, melted phosphorus and bisulphide of carbon being the most, and water the least sensitive of the liquids examined.

2. The length of the spectrum varies as the temperature increases. The difference between the refractive indices of the lines A and H, or $\mu_{H}-\mu_{A}$, is taken as the measurable length of the spectrum, and is given in the sixth column. In the case of highly dispersive substances, as bisulphide of carbon and hydrate of phenyle, it decreases considerably; in the case of less dispersive bodies, as the alcohols, it decreases to a less extent; while with water the change is not appreciable.

3. In some substances the dispersive power is diminished, in others it is augmented by a rise of temperature; that is, in such substances as bisulphide of carbon, it is the numerator of the fraction $\frac{\mu_{H}-\mu_{A}}{\mu_{D}-1}$ that decreases fastest, while in such substances as water it is the denominator. The result of this is shown in the last column.

4. The sensitiveness of a substance is independent of its specific refractive or dispersive power. Thus water and ether are very similar as to the actual amount of the refraction and dispersion exhibited by them, but ether is many times more sensitive to heat than water is.

5. The amount of sensitiveness is not directly proportional to the change of density produced by alterations of temperature; yet there is some relationship between the two phenomena. Thus in water the index of refraction and the density both change much more rapidly at high than at low temperatures; again, the remarkable reversion of the increase of density that takes place at 4° C. is not without its indication in the amount of sensitiveness; and the large decrease of density at the freezing of water is accompanied by a similar decrease of refraction.

Substance.	Mean refraction ($\mu_D - 1$).	Specific gravity.	Ratio.
Ice	0·3089	0·9184	2973
Water at 0° C.	0·3330	0·9993	3001

Moreover, as a general rule, those substances that are most affected in density by heat are the most sensitive.

6. No sudden change of sensitiveness occurs near the boiling-point; at least this is true in respect to bisulphide of carbon, ether, and methylic alcohol.

XVIII. "On the Adaptation of the Human Eye to varying Distances." By Charles Archer, Esq., Surgeon, Bengal Army. Communicated by Prof. Stokes, Sec. R.S. Received June 17, 1858.

(Abstract.)

The following is a summary of the author's views on the question:—

1. The eye is adapted to varying distances principally by an alteration in the fibrous arrangement of the lens itself. Moreover, that when the lens is removed after an operation for cataract, the power of adaptation is nearly lost, and can only be exerted within very confined distances.

2. That the purpose of focalizing light at short distances is doubtless assisted, as suggested by Bowman, by the contractions of the ciliary muscle, in its antero-posterior direction, bringing forward the ciliary processes.

3. That as the posterior hemisphere of the capsule is firmly united to the hyaloid membrane, this portion must always remain quiescent, and therefore the antero-posterior contractions of the ciliary muscle must be very limited as regards the lens.

4. That the ciliary muscle, being placed around the eye, and its fibres being of a somewhat plexiform character, the contractions of the muscle will relax those yielding portions of the eye placed within its circumference.

5. That the relaxations of the ciliary processes will deprive the capsule of its firm support. It will be pressed forward by the lens,

which will meet with no further resistance to the expansion of its short axis.

6. That the lens itself, as microscopically described by Bowman and Kölliker, is admirably adapted to the varying changes which take place in the capsule.

7. That the posterior capsule being firmly united to the hyaloid membrane, the alteration in the diameters of the cavity of the capsule must take place from the periphery of the lens to its centre, and from behind forwards, but not from before backwards, on account of the close union of the posterior capsule to the hyaloid membrane.

8. That to allow such alteration to take place without endangering the achromatism of the lens, the alterations in the plane of its long diameter must be synchronous with the alterations in the plane of its short diameter. To allow of this, the margin of the lens is free in the canal of Petit; were it not the case, chromatic aberration would result.

9. That the elasticity of the capsule of the lens and the ciliary muscle are antagonistic; that on the ciliary muscle becoming relaxed, the capsule of the lens is free to exert that elasticity.

10. That, by the pressure exerted by the anterior hemisphere of the capsule by means of the polygonal cells of Virchow on the anterior face of the lens, the organ is able to fulfil all the requirements for adapting it to receive focalized light from long distances.

11. That the polygonal cells of Virchow are placed on the posterior surface of the anterior hemisphere of the capsule with the view before mentioned, and that they are arranged with their long diameters in an antero-posterior direction, that pressure may not injure their transparency, which would be the case if placed laterally.

12. That these cells are not found in other parts of the capsule.

13. That the fibres of the lens are serrated for the purpose of uniting either to other, so as to allow them greater freedom of motion without altering their ultimate relations to each other.

14. That the ciliary muscle is very highly endowed with nervous matter to supply all these varying requirements.

15. By the above postulates, all the modern discoveries in the microscopical anatomy of the eye receive a distinct expression of their individual functions, and, by so doing, adapt the organ of vision to the acknowledged laws of light.

XIX. "On Curves of the Third Order." By the Rev. GEORGE SALMON, of Trinity College, Dublin. Communicated by ARTHUR CAYLEY, Esq. Received May 20, 1858.

(Abstract.)

The author remarks that his paper was intended as supplementary to Mr. Cayley's Memoir "On Curves of the Third Order" (Philosophical Transactions, 1857, p. 415). He establishes in the place of Mr. Cayley's equation, p. 442, a fundamental identical equation, which is as follows, viz. if substituting in the cubic U, $x+\lambda x'$, $y+\lambda y'$, $z+\lambda z'$ for x, y, z, the result is

$$U+3\lambda S+3\lambda^2 P+\lambda^3 U';$$

so that S and P are the polar conic and polar line of (x', y', z'), with respect to the cubic, viz.

$$3S=x'\frac{dU}{dx}+y'\frac{dU}{dy}+z'\frac{dU}{dz}; \quad 3P=x\frac{dU'}{dx'}+y\frac{dU'}{dy'}+z\frac{dU'}{dz'};$$

and if making the same substitution in the Hessian H, the result is

$$H+3\lambda\Sigma+3\lambda^2\Pi+\lambda^3 H',$$

so that Σ and Π are the polar conic and polar line of the Hessian—then the identical equation in question is

$$3(S\Pi-\Sigma P)=H'U-HU'.$$

And it follows that when (x', y', z') is a point on the cubic, the equation $U=0$ of the cubic may be written in the form

$$S\Pi-\Sigma P=0,$$

an equation which is the basis of the subsequent investigations of the paper. The author refers to a communication to him by Mr. Cayley, of an investigation of the equation of the conic passing through five consecutive points of the cubic, in the case where the equation of the cubic is presented in the canonical form $x^3+y^3+z^3+6lxyz=0$, and he shows that by the help of the above mentioned identity, the investigation can be effected with equal facility when the equation of the cubic is presented in the general form; and he establishes various geometrical theorems in relation to the conic in question. Finally, the author considers an entirely new question in the theory of cubics, viz. the determination of the points of a cubic, through which it is possible to draw an infinity of cubics having a nine-point

contact, or complete osculation, with the given cubic. It is shown that the points in question are those which are their own third tangentials, and this suggests the consideration of the new canonical form, $x^2y+y^2z+z^2x+2mxyz=0$, of the equation of the cubic; this inquiry, however, is not pursued in the paper.

XX. "Researches on the Foraminifera."—Part III. On the Genera *Peneroplis*, *Operculina*, and *Amphistegina*. By W. B. Carpenter, M.D., F.R.S. &c. Received June 17, 1858.

(Abstract.)

In his preceding memoirs, the author has shown that two very dissimilar types of structure present themselves among Foraminifera, one characterized by its simplicity, the other by its complexity. In the former, of which *Orbitolites*, *Orbiculina*, and *Alveolina* are typical examples, the calcareous skeleton does not present any definite indications of organization, but seems to have been formed by the simple calcification of a portion of the homogeneous sarcode-body of the animal; that sarcode-body is but very imperfectly divided into segments, the communications between the cavities occupied by these segments being very free and irregular; the form of the segments themselves, and the mode of their connexion, are alike inconstant; and even the plan of growth, on which the character of the organism as a whole depends, though preserving a general uniformity, is by no means invariably maintained. In the latter, to which *Cycloclypeus* and *Heterostegina* belong, the calcareous skeleton is found to present a very definite and elaborate organization. The several segments of the body are so completely separated from each other, that they remain connected only by delicate threads of sarcode. Each segment thus isolated has its own proper calcareous envelope, which seems to be moulded (as it were) upon it; and this envelope or shell is perforated with minute parallel tubuli closely resembling those of dentine, except in the absence of bifurcation; the partition-walls between adjacent segments are consequently double, and are strengthened by an intermediate calcareous deposit, which is traversed by a system of inosculating passages that seems properly to belong to it. The form of the segments, their mode of

communication, and consequently the general plan of growth, have a very considerable degree of constancy; and altogether the tendency is strongly manifested in this type to the greater individualization of the parts of the composite body, which in the preceding must be looked upon rather as constituting one aggregate whole.

In the present memoir this contrast is fully carried out by a detailed comparison of two characteristic examples from these types respectively, each of them having its own features of peculiar interest.

In *Peneroplis* we find, both as to the simplicity of the structure of the shell, and the general disposition of the segments of the animal, a close resemblance to the spiral forms of *Orbiculina*; the only difference being the absence of the transverse or secondary divisions of the chambers. In what is considered its typical form, the shell is a flattened spire, opening out widely in its last whorl; and the chambers communicate with each other (as does the last chamber with the exterior) by single rows of isolated pores disposed at regular intervals along the septa. But the spire is occasionally found to be more turgid, and the rows of apertures to become doubled; and instead of opening out in the last whorl, it is frequently prolonged in a rectilineal direction. In tropical seas there are found minute shells resembling those of *Peneroplis* in their very characteristic external markings, but having a very turgid spire, and having the row of pores in each septum replaced by a single large orifice with irregularly radiating prolongations. This type of structure has been characterized by M. d'Orbigny as a separate genus, under the name of *Dendritina*; and when its spire, as in many forms of *Peneroplis*, is continued rectilineally, it has been distinguished as a third genus under the name *Spirolina*. The author shows, by an extensive comparison of individuals, that the single dendritic orifice is to be regarded as formed by the coalescence of separate pores; and that the extension of these into a single line, or their aggregation into a cluster, is related to the form of the septal plane, as determined by the degree of flattening or of turgescence of the spire. Consequently in his view *Dendritina* and *Spirolina* are but varieties of *Peneroplis*; the former, which are by far the largest and the most highly developed, being of tropical growth, whilst the most flattened forms of the latter are the com-

paratively stunted inhabitants of the Mediterranean and other seas of less elevated temperature.

In *Operculina*, on the other hand, we find the shell presenting the minutely tubular structure which was first shown by the author to exist in *Nummulites*; to which genus *Operculina* is so closely allied in structure, that the only positive difference between them seems to lie in the tendency of *Operculina* to open out widely in the last whorl, whilst *Nummulites* (according to MM. d'Archiac and Haime) tends to close in. The author minutely describes the structure of *Operculina*, which presents a very remarkable development of the canaliferous system; he also enters into a detailed inquiry into the relation of the numerous strongly-marked varieties of form which it presents,—a question of much importance in regard to the value of the characters of the reputed species among *Nummulites*; and shows that the range of individual variation in form and surface-markings is so wide (as is proved by the gradational transitions which present themselves between what at first sight appear to be widely-separated types), that only where some very decided and constant difference of internal conformation presents itself, will it be safe to assume a specific diversity. In one case, in which he had thought that a certain series of specimens was sufficiently distinguished by its peculiar physiognomy from the rest, residual forms presented themselves which could not be with certainty assigned to either type, so completely do they link together the two by the softening down of the peculiarities of each. And a yet more remarkable link of connexion is established by examples collected on the coast of Japan by the American expedition to that country, in which the most distinctive characters of each type are curiously combined.

Closely related to *Operculina* is another genus, *Amphistegina*, which bears an equally near resemblance to *Nummulites*, though it has been completely separated from both in the classification of M. d'Orbigny, who has placed it in a distinct order, *Entomostègues*, on account of the unsymmetrical form of its shell and the alternating disposition of its chambers. But the author has found, from an extensive comparison of individuals, that this want of symmetry is so little constant, as to be altogether valueless in a systematic point of view, many specimens being perfectly symmetrical, whilst others are very far from being so, and every gradation presenting itself

between these two extremes. The most common among existing species is the *Amphistegina gibbosa*, which is very extensively diffused through the tropical ocean, and which, though generally of small size, acquires in the Philippine region dimensions nearly equal to those of the fossil *Amphistegina* of the Vienna and other tertiary deposits. But Mr. Cuming's Philippine collection contains another and far larger species, which is distinguished by the extraordinary thinning-out of the last whorl; and it is remarkable that in this species the canal-system is highly developed, although completely absent in *A. gibbosa*,—a difference of structure, which, going along with very close resemblance in external aspect and general conformation, seems only to be accounted for on the supposition that the difference in size requires a difference in the arrangement of the nutrient apparatus.

XXI. "Further Researches on the Grey Substance of the Spinal Cord." By J. Lockhart Clarke, Esq., F.R.S. Received June 17, 1858.

(Abstract.)

In this communication it is proposed, for reasons assigned, to divide each lateral half of the posterior grey substance into two portions:—1. The *caput cornûs posterioris*; 2, the *cervix cornûs posterioris*. The caput consists of the broad or expanded extremity of the cornu, and is separated from the cervix by an imaginary line drawn across from the opposite anterior extremities of the gelatinous substance; the cervix comprises the remaining anterior portion of the cornu.

The *caput cornûs* consists of two different portions:—1. an outer and comparatively transparent portion, the *gelatinous substance*; 2. an inner and more opaque portion, or base.

1. The outer portion or gelatinous substance consists of,—

A. Nerve-fibres, transverse, longitudinal, and oblique.

B. Nerve-cells, large, small, and intermediate.

C. Blood-vessels, and connective tissue, with numerous nuclei.

2. The inner or more opaque portion of the *caput cornûs* is continuous with the grey substance of the cervix, and surrounded

behind and on each side by the gelatinous substance, with which it varies in shape at different regions of the cord. In addition to blood-vessels and connective tissue, it consists of,—

A. Nerve-fibres, transverse, longitudinal, and oblique.

B. Nerve-cells, both large and small.

A. The longitudinal fibres form bundles of various sizes, and are broader and coarser than those of the *gelatinous substance*, which, however, they immediately adjoin.

The transverse and oblique fibres are continuous with the posterior roots of the nerves, and partly with the longitudinal fibres, which they also cross in a great variety of ways.

About the middle of the dorsal region, in the spinal cord of the higher vertebrata, the posterior cornua are united in a single mass. The inner or median half of each *cervix cornûs* is occupied by a remarkable longitudinal column, which is cylindrical or oval,—the *posterior vesicular column*. This consists of a cylinder of fibres interspersed with and surrounded by cells and thin processes. The fibres are derived from the posterior roots of the nerves, and interlace with each other in an intricate manner. The cells are oval, fusiform, and variously stellate, and differ considerably in size, but the largest are equal to those of the anterior cornu. They are elongated with thin processes transversely, longitudinally, and obliquely, and are continuous with fibres in the same direction, including the posterior roots.

At the lateral border of the grey substance, between the anterior and posterior cornua, is a small and somewhat triangular tract, which is more transparent than the rest, and projects more or less into the lateral column. This tract, which was pointed out by the author in 1851, and is named the *tractus intermedio-lateralis*, consists of oval, fusiform, and triangular cells, which are smaller and of more uniform size than those of the surrounding substance. Some of them are elongated transversely and longitudinally,—transversely both in a lateral and antero-posterior direction,—and send their processes on the one hand to the transverse commissure, and on the other to the anterior and posterior cornua.

In receding from the dorsal to the cervical region, the central portion or cylinder of each posterior vesicular column is reduced in size and less completely circumscribed. In the middle of the cervical

enlargement it entirely disappears, but the whole inner half of the *cervix cornûs* is still interspersed with numerous cells of various shapes, and traversed by the posterior roots and the fibres of the transverse commissure. At the origin of the third pair of cervical nerves, a darker mass reappears in the same situation, but gradually diminishes as it ascends to the medulla oblongata.

The *tractus intermedio-lateralis* is larger in the upper part than in the middle of the dorsal region, and projects further into the lateral column. As it ascends, however, through the cervical enlargement, it gradually diminishes, and at length disappears; but the lateral portion of the grey substance contains numerous branched and elongated cells, amongst which are a few that resemble those of the *tractus intermedio-lateralis*; it is traversed by the anterior and posterior roots, and by the lowest roots of the spinal-accessory nerve on their way to the anterior cornu. In the region of the first pair of cervical nerves, a distinct vesicular tract reappears at the lateral part of the grey substance. It is traversed by the roots of the spinal-accessory nerves, and partly by those of the spinal nerves. Its cells are elongated transversely and longitudinally. Ascending the medulla oblongata, this vesicular tract makes its way inwards to the space behind the central canal, where it forms the nucleus of the upper roots of the spinal-accessory nerve.

In descending the cord from the dorsal region, the grey substance undergoes a series of changes nearly similar to those which are observed in ascending to the cervical enlargement. But in the upper part of the lumbar enlargement, the posterior vesicular columns are much larger than in any other region of the cord, and contain more large cells. Through the rest of the lumbar enlargement the number of large cells diminishes; but they are still traversed and surrounded by the posterior roots of the nerves, and by the transverse commissure.

In the spinal cord of Man, the form of the grey substance differs in some respects from that in Mammalia. Throughout the whole of the dorsal region the posterior cornua stand completely apart. The posterior vesicular columns are oval, but in structure resemble those in the Ox. In the middle of the cervical and lumbar enlargements, their cells, in connexion with the posterior roots, are very small, but numerous.

The *tractus intermedio-lateralis* in Man presents nearly the same appearance as in Mammalia, and contains the same kind of cells. In the lumbar region it is still prominent at the side of the grey substance, but its cells are less numerous than in the dorsal region. In the upper part of the cervical region a similar tract reappears, which is traversed by the roots of the spinal accessory, and those of the spinal nerves.

In Birds, as in Mammalia, the posterior cornua are united in a single mass, both in the dorsal region and lower part of the *conus medullaris*; and the gelatinous substance extends uninterruptedly across from side to side. There are no dark masses corresponding to those of the posterior vesicular columns of mammalia, although numerous cells are scattered through the same space. There are no traces of any distinct *tractus intermedio-lateralis*. In Reptiles it is only in the *conus medullaris* that the posterior cornua form a single mass. A distinct stratum of small fusiform cells, in connexion with the fibres of the posterior roots, extends diagonally from the point of each cornu to the transverse commissure.

In the Ox and Sheep the epithelium of the canal consists, not of cylindrical, but of fusiform cells arranged in close apposition. The fibres proceeding from them are precisely similar in appearance to those of the connective tissue which surrounds the cord, and, like those fibres, they are in connexion at intervals with minute nuclei; in the *filum terminale* the author has satisfactorily traced them *through* the grey substance to the surface of the cord. In the *filum terminale*, where the nerve-cells and nerve-roots entirely disappear, the canal, and consequently the number of epithelium-cells, are much greater than in the cervical or lumbar enlargement, where the nerve-cells and nerve-roots are abundant. These facts are opposed to the statements of those observers who profess to have traced their connexion with nerve-cells and nerve-fibres.

The white columns of the cord are traversed by a network of connective tissue, which abounds with nuclei and small cells precisely similar to those found in the grey substance.

In the *conus medullaris*, the author has distinctly seen some of the anterior roots of the nerves form loops around the group of stellate cells, instead of terminating in them.

XXII. "On some new Ethyl-compounds containing the Alkali-metals." By J. A. Wanklyn, Esq. Communicated by Edward Frankland, Ph.D. Received June 10, 1858.

(Abstract.)

The very remarkable composition and properties of that class of substances comprehending kakodyl and zinc-ethyl, have justly attached no ordinary degree of interest to the so-called organo-metallic compounds.

Influenced by that interest, I was led to inquire whether the series might not include members into whose composition the alkali-metals entered. It was a question whether combination between so powerfully electro-positive a body as potassium or sodium on the one hand, and a hydrocarbon radical on the other, did not involve impossible conditions. It seemed that the answer to this query would not be valueless as a contribution to the store of facts out of which we may hope some day to evoke the conditions of chemical combination.

My researches in this direction have already enabled me to produce combinations of ethyl with potassium and sodium; and I have little doubt that I shall be able to produce similar compounds containing lithium, barium, strontium, calcium, and magnesium. Combinations containing methyl in place of ethyl will also be sought. The present paper will be devoted chiefly to the ethyl-compound of sodium.

Sodium-ethyl.

Experiments made with a view to the formation of this body by reactions similar to that by which zinc-ethyl is produced, yielded negative results; but some months ago I made the observation that potassium and sodium decomposed zinc-ethyl, and I found the action to consist in the replacement of a portion of the zinc by the metal employed.

Sodium-ethyl was prepared as follows:—A tube of soft glass was closed at one end and filled with coal-gas. In it was then placed a single clean piece of sodium; its open extremity was then closed with the finger, and whilst still filled with coal-gas, the tube was contracted about the middle, drawn out and bent twice at right angles; pure zinc-ethyl, in quantity about ten times the weight

of the sodium, was next introduced, and the tube hermetically sealed. So prepared, the apparatus was afterwards placed in cold water, and left therein for several days, being cautiously shaken up at intervals.

During this time the following changes were noted in the contents of the tube. The sodium became coated with zinc, and gradually disappeared, whilst the total volume of the solid and liquid contents diminished considerably. The liquid became also viscid, and sometimes separated into two portions non-miscible with each other, becoming, however, homogeneous as the operation advanced. There was no evolution of gas.

After the lapse of some days the apparatus was found to contain metallic zinc and a clear colourless liquid. The former was weighed and found to correspond to the sodium dissolved, one equivalent of zinc being precipitated for each equivalent of sodium dissolved.

The clear liquid was made the subject of special examination. It consisted of zinc-ethyl holding in solution a crystalline compound containing sodium, zinc, and ethyl. It was inflammable to the last degree, burning explosively, on exposure to the air, with a yellow flame, and leaving a very alkaline residue. Owing to its extreme tendency to become oxidized, its manipulation presented great difficulties. It was requisite to decant it into bulbs filled with dry hydrogen or coal-gas; and since heat produced partial decomposition, the bulbs had to be double, so that the heated bulb might not receive the liquid.

The clear liquid deposited large quantities of beautiful crystals when cooled to zero; and when gently warmed in a stream of dry hydrogen gas, so long as zinc-ethyl came off it yielded also a mass of crystals. Some crystals were prepared in the latter manner; they fused at about 27° C., but once fused they remained fluid at several degrees below that point. Numerous analytical determinations prove that these crystals contain two equivalents of zinc for every equivalent of sodium, and that their formula is

$$\left.\begin{matrix} Na \\ C_4H_5 \end{matrix}\right\}, 2\left\{\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right. .$$

The reaction by which they are produced may be thus expressed:

$$6\left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\} + \left.\begin{matrix} Na \\ Na \end{matrix}\right\} = \left.\begin{matrix} Zn \\ Zn \end{matrix}\right\} + 2\left(\left.\begin{matrix} Na \\ C_4H_5 \end{matrix}\right\}, 2\left\{\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right.\right).$$

For the body $Na\,C_4\,H_5$ I propose the name *sodium-ethyl*, and for the crystals that of *double compound of sodium-ethyl with zinc-ethyl.*

Many attempts were made to obtain sodium-ethyl free from zinc-ethyl, but without success.

By distillation it was found to be equally impossible either to distil off $\left.\begin{matrix} Na \\ C_4H_5 \end{matrix}\right\}$ from the crystals, or to distil off all $\left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\}$ so as to leave pure $\left.\begin{matrix} Na \\ C_4H_5 \end{matrix}\right\}$ behind. When the crystals are moderately heated in a bulb, a singular phenomenon occurs. Gas is evolved, and there remains behind metallic sodium, also metallic zinc, but no carbonaceous residue. This reduction of a sodium-compound by heat alone is an anomaly in chemistry.

When the crystals are heated in the water-bath with potassium, a sudden evolution of gas occurs, and there results metallic zinc, with a liquid alloy of potassium and sodium—a result likewise peculiar.

When the crystals are heated in the water-bath with excess of sodium, evolution of gas likewise takes place.

From these experiments it would seem that the conjoined zinc-ethyl is necessary to the existence of sodium-ethyl; or more precisely, that some adjunct of a less positive nature than sodium-ethyl is requisite to make the existence of the latter possible.

Passing on to the other reactions of the crystals $\left.\begin{matrix} 2(ZnC_4H_5) \\ NaC_4H_5 \end{matrix}\right\}$:—

With water there is given pure hydride of ethyl, and hydrated oxides of zinc and sodium. The reaction takes place with great evolution of heat.

With carbonic acid there is given propionate of soda, which unites with zinc-ethyl forming a double compound, decomposed on the addition of water. To the account of this reaction, published elsewhere, I have to add that it takes place without evolution of ethyl or any other gas—a result which further confirms the formula of sodium-ethyl adopted in this paper.

With carbonic oxide there is also a reaction, which is in course of examination.

Cyanogen gas is instantly absorbed, with the formation of a brown solution.

With ether there seems to be no reaction. For the rest, with oxygen, iodine, &c., I should predict reactions quite analogous to those of zinc-ethyl, but have not specially examined the point.

Potassium-ethyl.

Zinc-ethyl and potassium react still more readily than the former body and sodium. So far as at present ascertained, the cases greatly resemble one another. Just as with sodium, I obtain crystals readily soluble in zinc-ethyl, which contain in this case abundance of potassium.

Seeing that the *kind* of reaction brought under notice in this paper is apparently unique, it is necessary to offer a few observations upon it.

$$2 \cdot \left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\} + \left.\begin{matrix} Na \\ Na \end{matrix}\right\} = 2 \cdot \left.\begin{matrix} Na \\ C_4H_5 \end{matrix}\right\} + \begin{matrix} Zn \\ Zn \end{matrix}$$

The reaction here formulated may be regarded as an electrolytic decomposition—as an ordinary case of precipitation of one metal by a more electro-positive metal. Here ethyl is the electro-negative, and zinc the electro-positive member: sodium is more electro-positive than zinc, and accordingly sodium displaces zinc.

Following out the hypothesis—where the organo-metallic body contains a metal less electro-positive than the hydrocarbon radical, I should expect that the hydrocarbon radical would be eliminated by the action of sodium. Kakodyl, for instance, should give methyl and arsenide of sodium.

$$\left.\begin{matrix} C_2H_3 \\ C_2H_3 \end{matrix}\right\} As + \left.\begin{matrix} Na \\ Na \end{matrix}\right\} = \left.\begin{matrix} Na \\ Na \end{matrix}\right\} As + \begin{matrix} C_2H_3 \\ C_2H_3 \end{matrix}$$

A case in point is afforded by the reaction of the alkali-metals with ammonia.

$$2 \cdot \left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} K \\ K \end{matrix}\right\} = 2 \cdot \left.\begin{matrix} K \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} H \\ H \end{matrix}\right\}$$

Of the same kind is the reaction of zinc-ethyl upon ammonia*.

$$\left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\} = \left.\begin{matrix} Zn \\ H \\ H \end{matrix}\right\} N + \left.\begin{matrix} H \\ C_4H_5 \end{matrix}\right\}$$

To develope the hypothesis still further: just as the positive side admits of displacement by a more electro-positive radical, so should the negative side admit of displacement by a more electro-negative body.

* See Frankland's paper, Trans. Royal Soc. 1857.

The ordinary reactions of zinc-ethyl may be looked upon as illustrating this proposition, and can be written so as to exhibit a double displacement.

$$\overset{+}{Zn}, \overset{-}{C_4H_5} + \overset{+-}{I\,I} = \underbrace{\overset{+-}{ZnI} + \overset{+}{C_4H_5}\overset{-}{I}}$$

$$\text{also}\quad \overset{+}{Zn}\overset{-}{C_4H_5} + \overset{+-}{OO} = \overset{+-}{ZnO} + \overset{+}{C_4H_5}\overset{-}{O}$$

Inspection will show in all these cases, that an electro-positive radical displaces a less electro-positive radical; and an electro-negative radical displaces a less electro-negative one.

In accordance with the theory would be the displacement in sodium-ethyl of the ethyl by mercury, or by copper, &c., platinum, &c.

$$\begin{matrix} Na, C_4H_5 \\ Na\ C_4H_5 \end{matrix} + \begin{matrix} Cn \\ Cn \end{matrix} = \begin{matrix} NaCn \\ NaCn \end{matrix} + \begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}$$

Also a like displacement by arsenic or by nitrogen would be according to theory.

Pushing the hypothesis to its furthest limits, I should say that sodium-ethyl is only in *equilibrium* with bodies whose respective electrical sides lie either both of them *within*, or both of them *without* the space lying between the electro-positive sodium and the electro-negative ethyl.

XXIII. "Note on Sodium-ethyl and Potassium-ethyl." By Edward Frankland, Ph.D., F.R.S. Received June 17, 1858.

The recent interesting discovery of sodium-ethyl and potassium-ethyl by Mr. Wanklyn, led me to investigate the cause of the non-formation of these bodies by reactions analogous to those successfully used for the production of zinc-ethyl and similar organo-metallic compounds. In my earlier experiments upon the isolation of the organic radicals, I studied the action of potassium and sodium upon iodide of ethyl, and found that the latter compound was readily decomposed by either of the metals at a temperature of from 100° to 130° C. The separated ethyl was, however, transformed almost completely into hydride of ethyl and olefiant gas, whilst not a trace of potassium-ethyl or sodium-ethyl was produced. Mr. Wanklyn has

since repeated this experiment with the addition of ether, and has obtained the same result as regards the non-formation of an organo-metallic compound.

The temperature at which sodium decomposes iodide of ethyl is much lower than that at which sodium-ethyl is broken up, consequently no explanation of the phenomenon can be obtained from this source. In his observations on the formation of ethyl*, Brodie mentions that iodide of ethyl is decomposed at 170° C. by zinc-ethyl; and it therefore occurred to me that sodium-ethyl, owing to its more powerful affinities, might effect the decomposition of iodide of ethyl at a lower temperature than that at which iodide of ethyl is decomposed by sodium; in which case the production of sodium-ethyl, by the action of sodium upon iodide of ethyl, would be an impossibility. Experiment completely confirmed this anticipation. A quantity of a strong solution of sodium-ethyl in zinc-ethyl was thrown up into a dry receiver filled with mercury, and an equal volume of pure iodide of ethyl added to it. Immediately on the mixture of the two liquids, a lively effervescence set in, a considerable quantity of gas collected in the receiver, and a white deposit of iodide of sodium rendered the liquid thick and turbid. The reaction was complete in two or three minutes without the application of heat. An analysis of the gas, previously freed from the vapours of iodide of ethyl and zinc-ethyl, showed it to consist of equal volumes of hydride of ethyl and olefiant gas, mixed only with a mere trace of ethyl. This reaction may therefore be thus expressed:—

$$C_4H_5Na + C_4H_5I = NaI + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} + C_4H_4.$$

It is therefore evident that sodium-ethyl, and the remark no doubt applies also to potassium-ethyl, could not be obtained by the action of sodium upon iodide of ethyl, even if the decomposition of the latter could be effected at ordinary temperatures, since each particle of the organo-metallic compound being in contact with iodide of ethyl at the moment of its formation, would be instantly decomposed in the manner just described. That olefiant gas and hydride of ethyl, with mere traces only of ethyl, constitute the gaseous product of the decomposition of iodide of ethyl by sodium, is strong evidence that this formation and immediate decomposition of sodium-ethyl actually

* Journal of the Chemical Society, vol. iii. p. 405.

takes place. Sodium-ethyl thus stands in the same relation to iodide of ethyl as hydride of zinc does to hydriodic acid; and consequently all attempts to produce hydride of zinc by the action of the metal upon the hydrogen acids have failed. These considerations, taken in connexion with Mr. Wanklyn's mode of forming sodium-ethyl and potassium-ethyl, afford a clue to the nature of the reactions by which we shall probably eventually succeed in forming the hydrogen compounds of the highly positive metals. Although the hydrogen compounds of arsenic, antimony, phosphorus, and tellurium are by no means exact analogues of zinc-ethyl, it would nevertheless be interesting to ascertain the action of sodium upon these bodies, with a view to the formation of hydride of sodium.

The nature of the gas evolved by the action of sodium-ethyl upon iodide of ethyl, has some interest in connexion with the formation of ethyl by the action of zinc upon iodide of ethyl. Brodie expressed, in the memoir above alluded to, an ingenious and highly probable hypothesis, that the true source of the ethyl is the decomposition of its iodide by zinc-ethyl, thus:—

$$C_4H_5Zn + C_4H_5I = ZnI + \left.\begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}\right\} ;$$

and that the secondary products of the reaction (olefiant gas and hydride of ethyl) which always accompany the ethyl, result from the primary action of zinc upon iodide of ethyl, thus:—

$$2(C_4H_5I) + 2Zn = C_4H_4 + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} + 2ZnI.$$

The composition of the gases produced in the above reaction of sodium-ethyl upon iodide of ethyl seems, however, to indicate that the reverse of this hypothesis is true, and that the source of the ethyl is to be found in the primary action of zinc upon iodide of ethyl,—

$$2(C_4H_5I) + 2Zn = \left.\begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}\right\} + 2ZnI,$$

whilst the secondary products are derived from the decomposition of iodide of ethyl by zinc-ethyl,—

$$C_4H_5I + ZnC_4H_5 = \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} + C_4H_4 + ZnI.$$

XXIV. "Experimental Inquiry into the Composition of some of the Animals fed and slaughtered as Human Food." By J. B. Lawes, Esq., F.R.S., F.C.S., and J. H. Gilbert, Ph.D., F.C.S. Received June 17, 1858.

(Abstract.)

After alluding to the importance of the chemical statistics of nutrition in relation to physiology, dietetics and rural economy, and explaining that the branch of the subject comprehended in the present paper is that of *Animal Composition*, the authors proceed in the first place to state the general nature of their investigations, and the manner in which they were conducted.

To ascertain the quantitative relations, and the tendency of development, of the different parts of the system, the weights of the entire bodies, and of the several internal organs, also of some other separated parts, were determined in several hundred animals—oxen, sheep and pigs.

To determine the ultimate composition, and in a sense the proximate composition also, of oxen, sheep and pigs, and to obtain the results in such manner that they might serve to estimate the probable composition of the *Increase* whilst fattening, was a labour obviously too great to be undertaken with a large number of animals. Those selected were—a fat calf, a half-fat ox, a moderately fat ox, a fat lamb, a store or lean sheep, a half-fat old sheep, a fat sheep, a very fat sheep, a store pig, and a fat pig.

It is to the methods and the results of the analysis of these ten animals, to the information acquired as to the quantitative relation of the organs or parts in the different descriptions of animal, and their relative development during the fattening process, and to the application of the data thus provided, that the authors chiefly confine themselves in the present paper.

The analyses of the ten animals were planned to determine the actual and per-centage amounts—of water, of mineral matter, of total nitrogenous compounds, of fat, and of total dry substance—in the entire bodies, and in certain individual and classified parts of the animals. The water and mineral matter were for the most part determined in each internal organ, or other separated part. But, to

confine the labour within reasonable limits, and to facilitate as far as possible the perception of the practical and economic application of the results, the other constituents enumerated are given in—

1st. The collective "carcass" parts; that is, the frame with its covering of flesh and fat, which comprise the most important portions sold as human food.

2nd. The collective "offal" parts; including the whole of the internal organs, the head, the feet, and, in the case of oxen and sheep, the pelt and hair or wool.

3rd. The entire animal (fasted live-weight).

Referring first to the composition of the "*collective carcass parts*," it appeared, comparing one animal with another, that there is a general disposition to a rise or fall in the per-centage of *mineral matter*, with the rise or fall in that of the *nitrogenous compounds*. In fact, all the results tended to show a prominent connexion between the amount of the mineral matters and that of the nitrogenous constituents of the body.

Comparing the relative proportions of *fat* and *nitrogenous compounds* in the respective "carcasses," it appeared that, in every instance excepting that of the calf, there was considerably more of dry fat than of dry nitrogenous compounds. In the carcass of even the store or lean sheep, there was more than $1\frac{1}{2}$ times as much fat as nitrogenous substance; in that of the store or lean pig, twice as much. In the carcass of the half-fat ox, there was one-fourth more fat than nitrogenous matter; and in that of the half-fat sheep, more than twice as much.

Of the fatter animals, the carcass of the fat ox contained $2\frac{1}{3}$ times, that of the fat sheep 4 times, and that of the very fat sheep, 6 times as much fat as nitrogenous substance. Lastly, in the carcass of the moderately fat pig, there was nearly 5 times as much fatty matter as nitrogenous compounds.

From these facts it may be concluded, that in carcasses of oxen in reputed good condition, there will seldom be less than twice as much, and frequently nearly 3 times as much dry fat as dry nitrogenous substance. It may be presumed, that in the carcasses of sheep the fat will generally amount to more than 3, and frequently to 4 (or even more) times as much as the nitrogenous matters; and finally, that in the carcasses of pigs killed for fresh pork, there will seldom

be as little as 4, and in those fed for curing there will be more than 4 times as much fat as nitrogenous compounds.

The *fat* of the bones constituted but a small proportion of that of the entire carcasses; whilst the *nitrogen* of the bones amounted to a considerable proportion of the whole.

It appeared, that whilst the per-centage (in the carcasses) of both mineral and nitrogenous matters *decreased* as the animals matured, that of the fat very considerably *increased*. The increase in the per-centage of fat was much more than equivalent to the collective decrease in that of the other solid matters,—that is to say, as the animal matures, the per-centage in its carcass, of *total dry substance*—and especially of fat—much increases.

The carcass of the calf contained $62\frac{1}{4}$ per cent., that of the lean sheep $57\frac{1}{3}$rd per cent., that of the lean pig $55\frac{1}{3}$rd, and that of the half-fat ox 54 per cent. of water. In the carcass of the fat ox there were $45\frac{1}{2}$ per cent., in that of the fat lamb $48\frac{2}{3}$rds per cent., in that of the half-fat old sheep $49\frac{2}{3}$rds per cent., in that of the fat sheep $39\frac{2}{3}$rds per cent., in that of the very fat sheep only 33 per cent., and in that of the moderately fattened pig only $38\frac{1}{2}$ per cent. of water. The bones of the carcasses contained a less proportion of water than the collective soft or edible portions.

It is inferred, that the average of carcasses of well-fattened oxen will contain 50 per cent., or rather more, of dry substance; that those of properly fattened sheep will contain more still—say 55 to 60 per cent.; those of pigs killed for fresh pork rather more than those of sheep; whilst the sides of pigs fed and slaughtered for curing will be drier still. Lamb-carcasses would seem to contain a smaller proportion of dry substance than those of either moderately fattened oxen, sheep, or pigs. Their proportion of bone was also comparatively high. Veal appeared to be the moistest of all. The carcass of the calf experimented upon, though the animal was considered to be well fattened, contained only $37\frac{3}{4}$ per cent. of dry substance. Its proportion of bone was also higher than in any of the other animals.

Next as to the composition of the *collective offal parts* (excluding the contents of stomachs and intestines), the results showed that in every case the per-centage of nitrogenous substance was greater, and that of the fat very much less, than in the collective carcass parts.

In oxen and sheep, the pelt, hair or wool, hoofs, stomachs and intestines, taken together, contained a large proportion of the total nitrogen of the offal parts. The portions of the nitrogenous offal parts of these animals, generally used for food, are, the head-flesh with tongue and brains, the heart, the liver, the pancreas, the spleen, the diaphragm, and sometimes the lungs. In the pig, the proportion of the nitrogenous offal generally eaten, is greater than in the other animals; but its proportion of fat is generally also greater.

With the higher per-centage of nitrogenous substance, and the less per-centage of fat, in the collective offal parts, they had invariably a less per-centage of total dry substance, and therefore more of water, than the collective carcass parts.

From the composition of the *entire bodies* of the animals analysed, it is estimated, that of *mineral matter*, the average amount, in *store* or *lean* animals, will probably be, in oxen $4\frac{1}{2}$ to 5 per cent., in sheep 3 to $3\frac{1}{2}$ per cent., and in pigs $2\frac{1}{2}$ to 3 per cent. As an average estimate for the *mineral* matter in *fattened* animals, the results indicated $3\frac{1}{2}$ to 4 per cent. in the live-weight of calves and oxen, $2\frac{1}{2}$ to $2\frac{3}{4}$ per cent. in that of sheep and lambs, and $1\frac{1}{4}$ to $1\frac{3}{4}$ per cent. in that of pigs.

Of total *nitrogenous compounds*, there were in the fasted live-weight of the fat ox $14\frac{1}{2}$ per cent., in that of the fat sheep $12\frac{1}{4}$ per cent., in that of the very fat one not quite 11 per cent , and in that of the moderately fattened pig about the same, namely, 10·87 per cent. The leaner animals analysed contained from 2 to 3 per cent. more nitrogenous substance than the moderately fattened ones.

The *Fat* formed the most prominent constituent of the dry or solid substance of the entire animal bodies. The fat calf alone contained less total fat than total nitrogenous compounds. Of the other professedly fattened animals, the entire bodies of the fat ox and fat lamb contained about 30 per cent., that of the fat sheep $35\frac{1}{2}$ per cent., that of the very fat sheep $45\frac{3}{4}$ per cent., and that of the moderately fat pig $42\frac{1}{4}$ per cent. of dry fat.

The average composition of the six animals assumed to be well fattened, showed, in round numbers, 3 per cent. of mineral matter, $12\frac{1}{2}$ per cent. of nitrogenous compounds, and 33 per cent. of fat, in their standing or fasted live-weight.

All the experimental evidence conspired to show, that the so-called "*fattening*" of the animals was properly so designated. During the feeding or fattening process, the per-centage of the collective dry substance of the body considerably increased; and the fatty matter accumulated in much larger proportion than the nitrogenous compounds. The *increase* itself must therefore show a less per-centage of nitrogenous substance (and of mineral matter also), and a higher one of both fat and total dry substance, than the whole body of the fattened animal.

The knowledge thus acquired of the composition of animals in different conditions of maturity, was next employed as a means of estimating the composition of the increase gained in passing from one given point of progress to another.

To this end, the composition of the animals analysed in the lean condition, was applied to the known weights of numbers of animals of the same description, assumed to be in a similar lean condition; and the composition of the fat animals analysed was in like manner applied to the weights of the same series of animals after being fattened. Deducting the amount of the respective constituents in the lean animals, from that of the corresponding constituents in the fat ones, the *actual* amount of each constituent gained was determined. The weight of the gross increase being also known, its estimated *per-centage* composition was thus a matter of easy calculation. The composition of the *increase* of 98 fattening oxen, 349 fattening sheep, and 80 fattening pigs (each divided into numerous lots), was estimated in the manner indicated; and as a control, a statement is given of the composition of the *increase* of the single analysed fat pig, which, at the time it was put to fatten, corresponded in weight and other particulars most closely with the one analysed in the lean condition.

It is concluded, that the *increase* in weight of *oxen*, taken over six months or more of the final fattening period, may be estimated to contain from 70 to 75 per cent. of total dry substance; of which 60 to 65 parts will be fat, 7 to 8 parts nitrogenous substance, and 1 to 1½ mineral matter.

On the same plan of calculation, the final *increase* of *sheep*, feeding liberally during several months, will probably consist of 75 per cent., or more, of total dry substance; of this, 65 to 70 parts

will be fat, 7 to 8 parts nitrogenous compounds, and perhaps $1\frac{1}{2}$ part mineral matter.

The *increase* of *pigs*, during the final two or three months of feeding for fresh pork, may be taken at 70 to 75 per cent. total dry substance, 65 to 70 per cent. fat, 6 to 8 per cent. nitrogenous substance, and less than 1 per cent. of mineral matter. The increase over the last few months of high feeding, of pigs fed for curing, will doubtless contain a higher per-centage of both fat and total dry substance, and a lower one of both nitrogenous compounds and mineral matter, than that of the younger and more moderately fattened animal.

As a general result, it appears that about $\frac{3}{4}$ths of the gross increase in live-weight, of animals feeding liberally for the butcher, will be dry or solid matter of some kind. About $\frac{2}{3}$rds of the gross increase will be dry fat; only about 7 or 8 per cent. of the gross increase (and scarcely more than $\frac{1}{10}$th of the total dry substance) will be nitrogenous compounds; and seldom more than $1\frac{1}{2}$, and frequently less than 1 per cent. mineral matter.

In the case of most of the sheep, and of all the pigs, the composition of whose increase was estimated, the amounts of mineral matter, of nitrogenous compounds, of non-nitrogenous organic substance, of total dry substance, and sometimes of fat, which were consumed during the fattening period, were determined; so that the means are at command for studying the quantitative relation of the constituents estimated to be stored up in the increase, to those consumed in the food which produced it.

Taking first the proportion of each class of constituents stored up *for* 100 *of the same consumed*, it is concluded, that in the case of sheep, liberally fed on a mixed diet of dry and succulent food, the increase of the animal will perhaps generally carry off less than 3 per cent. of the consumed mineral matter—somewhere about 5 per cent. (varying according to the proportion in the food) of the consumed nitrogenous compounds, and about 10 parts of fat for 100 non-nitrogenous substance in the food; and lastly, that for 100 of collective dry substance of food consumed, there will be, in Sheep, about 8 or 9 parts of dry matter in increase stored up.

The food of the fattening pig contained a much smaller proportion of indigestible woody fibre than that of the sheep; and it appeared that the pig appropriated to its increase a much larger proportion of

the organic constituents of its food than the sheep. The average of the estimates for pigs, showed about 17 parts of dry substance of increase stored up, for 100 of collective dry matter of food consumed. For 100 of non-nitrogenous organic constituents in food, about 20 parts of fat were stored up. Of nitrogenous compounds, when the food consisted of about the usual proportions of the leguminous seeds and cereal grains, from 5 to 7 or 8 parts were stored up for 100 consumed. When the leguminous seeds predominated, the *proportion* of the consumed nitrogen stored up was less; and when the cereal grains predominated, it was greater. The estimates showed, that on the average of the cases, there were 4 or 5 times as much fat stored up in increase, as there was of fatty matter supplied in the food. There was obviously therefore a *formation* of fat in the animal body.

Reckoning the amount of the respective constituents of increase stored up, *for* 100 *of the collective dry substance of the food consumed,* the general result was as follows:—It appeared, that of the about 9 parts of dry increase, in sheep liberally fed on corn or oil-cake and succulent roots, for 100 of dry food consumed, about 8 parts were non-nitrogenous substance, that is, *fat*. There was therefore only about 1 part stored as nitrogenous and mineral matters taken together. The average of the estimates showed the produce of 100 of the collective dry substance of the consumed food of sheep to be—about, 0·2 part of mineral matter, 0·8 part nitrogenous compounds, and 8 parts fat, stored up; leaving therefore about 91 parts to be expired, perspired, or voided.

Taking the average of all the estimates of this kind relating to *pigs*—of the 17¼ parts of dry increase for 100 of dry matter of food consumed, about 15¾ parts were estimated as fat, rather more than 1⅓rd part nitrogenous substance, and an insignificant amount as mineral matter. On this plan of calculation, therefore, there would appear to be, in the case of fattening pigs, only from 82 to 83 parts of food-constituents expired, perspired, or voided, for 100 of the collective dry substance of food consumed.

It is obvious that the ultimate composition of the dry substance of increase must be very different from that of the 100 of dry substance consumed. This is strikingly illustrated in the case of the fat. In most of the experiments with pigs, the fatty matter in the

food was determined. On the average of the cases it amounted to less than $\frac{1}{4}$th as much as was estimated to be stored up in the increase of the animals. There was obviously therefore a *formation of fat in the body,* from some other constituent or constituents of the food. Supposing the $\frac{3}{4}$ths or more of the stored-up fat which must have been formed in the body to have been produced from *starch,* it was estimated that it would require $2\frac{1}{2}$ parts of starch to contribute 1 part of produced fat. Accordingly, it would appear that a much larger proportion of the consumed dry matter is, as it were, directly engaged in the production of the dry fatty increase, than is represented by the amount of the dry increase itself.

Thus, taking the average of the cases in which the fatty matter in the food of the pigs was determined, it was estimated that 17·4 parts of dry increase were produced for 100 of dry matter of food consumed. Of the 17·4 parts of dry increase, 16·04 are reckoned as fat. But there were only 3·96 parts of ready-formed fatty matter supplied in the food. At least 12·08 parts of fat must therefore have been produced from other substances. If from starch, it would require (at the rate of $2\frac{1}{2}$ parts of starch to 1 of fat) 30·2 parts of that substance for the formation of 12·08 parts of the produced fat. The ready-formed fat and the starch, together, thus supposed to contribute to the 16·04 parts of fat in the increase, would amount to 34·16 parts out of the 100 of dry matter of food consumed. But there were, further, 1·36 part of nitrogenous and mineral matters stored up in the increase. In all, therefore, 35·52 parts out of the 100 of gross dry matter consumed, contributed, in this comparatively direct manner, to the production of the 17·4 parts of gross dry increase.

According to the illustration just given, it appears that there was pretty exactly twice as much of the dry substance of the food, involved in the direct production of the increase, as there was of dry increase itself; hence instead of their being, as before estimated, 82 to 83 parts of the consumed dry matter expired, perspired, or voided, without as it were being directly involved in the production of the increase, it is to be inferred that, in the sense implied, only about 65 parts were so expired, perspired, or voided.

It having been thus found that by far the larger proportion of the solid increase of the so-called fattening animals is really *fat*

itself,—as moreover, it is probable that, at least in great part, the fat formed in the body is normally derived from starch, and other non-nitrogenous constituents of the food—and since the current fattening foods contain such a very large amount of nitrogen compared with that eventually retained in the increase—it can hardly be surprising that, contrary to the usually accepted opinions, the comparative values of our staple food-stuffs are much more nearly measurable by their amount of digestible and assimilable non-nitrogenous constituents, than by that of the digestible and assimilable nitrogenous compounds.

In order to determine the relative development of the several organs and parts in different descriptions of animals, and in animals of the same description in different conditions of growth and maturity, the weights alive, and of the separate internal organs and some other parts, of 16 calves, heifers and bullocks, of 249 sheep, and of 59 pigs, were taken.

It appeared that in oxen the stomachs and contents constituted about $11\frac{1}{2}$, in sheep about $7\frac{1}{2}$, and in the pig only about $1\frac{1}{4}$ per cent. of the entire weight of the body. The amounts of the intestines and their contents stood in the opposite relation. They amounted in the pig to about $6\frac{1}{4}$, in the sheep to about $3\frac{1}{2}$, and in the oxen to only about $2\frac{3}{4}$ per cent. of the whole body. These facts are of considerable interest, when it is borne in mind that in the food of the ruminant there is so large a proportion of indigestible woody fibre, and in that of the well-fed pig a comparatively large proportion of starch—the primary transformations of which are supposed to take place chiefly after leaving the stomach, and more or less throughout the intestinal canal.

Taken together, the stomachs, small intestines, large intestines, and their respective contents, constituted, in oxen more than 14 per cent., in sheep a little more than 11 per cent., and in pigs about $7\frac{1}{2}$ per cent. With these great variations in the proportion in the different descriptions of animals, of these receptacles and first laboratories of the food (with their contents), the further elaborating organs, if we may so call them (with their fluids), appear to be much more equal in their proportion in the three cases. This is approximately illustrated in the fact, that taking together the recorded per-centages of "heart and aorta," "lungs and windpipe," "liver," "gall-bladder

and contents," "pancreas," "milt or spleen," and the "blood," the sum indicated is for the oxen about 7 per cent., for the sheep about $7\frac{1}{4}$ per cent., and for the pigs about $6\frac{2}{3}$rds per cent. Excluding from this list the blood, which was more than $\frac{1}{3}$rd of a per cent. lower in amount in the pigs than in the other animals, the sums of the per-centages of the other parts enumerated would agree even much more closely for the three descriptions of animal.

With regard to the influence of progression in maturity and fatness of the animal, upon the relative development of its several parts, the results showed that the internal organs and other offal-parts pretty generally *increased* in *actual weight* as the animals passed from the lean to the fat or to the very fat condition. The *per-centage proportion* to the whole live-weight of these offal-parts as invariably *diminished* as the animals matured and fattened. The carcasses, on the other hand, invariably increased, not only in *actual* weight, but in *proportion* to the whole body.

The conclusion is, that in the feeding or fattening of animals, the apparatus which subserves for the reception and elaboration of the food does not increase commensurately with those parts which it is the object of the feeder to store up from that food. These parts are comprised in the "carcass" or frame-work, with its covering of flesh and fat. Of the carcasses which thus constitute the greater part of the increase, the nitrogenous portions increase but little, whilst the *fat* does so in very much larger proportion. Of the internal parts, again, it is also the *fat* which increases most rapidly.

The maturing process consists, then, in diminishing the proportional amount in the whole body, of the collective muscles, tendons, vessels, fleshy organs, and gelatigenous matters—the motive and functional, or so to speak, working parts of the body—the constituents of which alone can increase the amount of or replace the transformed portions of similar matters in the human body. It consists, further, in increasing very considerably the deposition of *fat*—one of the *non*-flesh-forming, but most concentrated of the respiratory and fat-storing constituents of human food.

It is then in our *meat-diet*, of recognized good quality, to which is generally attributed such relatively high *flesh-forming* capacity, that we carefully store up such a large proportion of *non*-flesh-forming, but concentrated respiratory material.

One of the most important applications which can be made of a knowledge of the composition of the animals which constitute the chief sources of our animal food, is to determine the main points of distinction between such food and the staple vegetable substances which it substitutes or supplements in an ordinary mixed diet.

By the analysis of some of the most important animals fed and slaughtered as human food, it was found that the *entire bodies*, even when in a reputed lean condition, may contain more dry fat than dry nitrogenous substances. Of the animals "ripe" for the butcher, a bullock and a lamb contained rather more than twice, a moderately fat sheep nearly three times, and a very fat sheep and a moderately fat pig about four times as much dry fat as dry nitrogenous matter. Of the professedly fattened animals analysed, a fat calf alone contained rather less fat than nitrogenous compounds.

It was estimated, that of the whole *nitrogenous substances* of the body, 60 per cent. in the case of calves and oxen, 50 per cent. in lambs and sheep, and 78 per cent. in pigs, would be consumed as human food. Of the total *fat* of the bodies, on the other hand, it was supposed, that in calves and lambs 95 per cent., in oxen 80 per cent., in sheep 75 per cent., and in pigs 90 per cent. would be so applied.

Assuming the proportional consumption of the fat and nitrogenous compounds to be as here estimated, there would be in the fat calf analysed $1\frac{1}{2}$ time, in the fat ox $2\frac{3}{4}$ times, in the fat lamb, fat sheep, and fat pig nearly $4\frac{1}{2}$ times, and in the very fat sheep $6\frac{1}{4}$ times as much dry fat as dry nitrogenous or flesh-forming constituents consumed as human food.

It would perhaps be hardly anticipated, that in the staple of our meat-diet, to which such a high relative flesh-forming capacity is generally attributed, there should be found such a high proportion of non-flesh-forming to flesh-forming matter as above indicated. The result of such a comparison as present knowledge permits in regard to the same point between the staple of our animal food and the more important kinds of vegetable food, will certainly not be less surprising

Of the staple *vegetable* foods, *wheat-flour bread* is, at least in this country, the most important. It will be interesting, therefore, to contrast with this substance the estimated consumed portions of the

analysed animals. To this end some assumption must be made as to the relative values (on the large scale), for the purposes of respiration and fat-storing, of the starch and its analogues in bread, and the fat in meat. It is assumed that, in round numbers, 1 part of fat may be considered equal to $2\frac{1}{2}$ parts of starch in these respects. If, therefore, the quantity of fat in the estimated consumed portions of the analysed animals be multiplied by 2·5, it is brought to what may be conveniently called its "*starch-equivalent*;" and in this way the Meat and the Bread can be easily compared with one another in regard to the relation of their flesh-forming, to their respiratory and fat-forming capacities.

Reckoning the amount—say 1 per cent.—of fat in Bread itself (and it probably averages not more than $\frac{1}{2}$ per cent.), to be equal to $2\frac{1}{2}$ parts of starch, and adding this to the amount of the actual starch and allied matters which it on the average contains, the calculation gives—assuming this *starch-equivalent* to represent specially the respiratory and fat-forming, and the nitrogenous substances, the flesh-forming matter—6·8 parts of respiratory and fat-forming to 1 of flesh-forming material in *Bread*.

Taking the relation of the one class of constituents to the other, in the estimated total consumed portions of the animals assumed to be in fit condition for the butcher, there was only one case—that of the fat calf—in which the proportion of the so measured respiratory and fat-forming to the flesh-forming capacity was in this our meat-diet lower than in Bread. In the estimated total consumed portions of the fat ox, the proportion of the *starch-equivalent* of non-flesh-forming matter to 1 of nitrogenous compounds, was 6·9, or rather higher than in Bread. In the estimated consumed portions of the fat lamb, the fat sheep, and the fat pig, the proportion was more than $1\frac{1}{2}$ time as great as in Bread; and in those of the extra fat sheep it was more than twice as great. Taking the average of the 6 cases, there were nearly 10 parts of *starch-equivalent* to 1 of nitrogenous compounds, against 6·8 to 1 in Bread. In the half-fat ox, and the half-fat old sheep, neither of which were in the condition of fatness of such animals as usually killed, the relation of the *starch-equivalent* to the nitrogenous compounds (assuming only the same proportion of the total fat as before to be eaten), was in the former considerably, and in the latter slightly lower than in Bread, namely,

as 3·83 to 1 in the half-fat ox, and as 6·28 to 1 in the half-fat old sheep.

It will perhaps be objected, that when animals are so far fattened as to attain the relations above stated, the feeder is simply inducing disease in the animals themselves, and frustrating that which, it is considered, should be the special advantage of a meat-diet, namely, the increase in the relative supply of the flesh-forming constituents in our food. It cannot be doubted, however, that in animals that would be admitted, by both producer and consumer, to be in only a proper condition of fatness, there would be a higher relation of non-nitrogenous substance, in its respiratory and fat-forming capacity to flesh-forming material, in their total consumed portions, than in the average of our staple vegetable foods. It may be true, that with the modern system of bringing animals very early forward, the development of fat will be greater, and that of the muscles and other nitrogenous parts less than would otherwise be the case; but it is certain, that if meat is to be economically produced, so as to be within the reach of the masses of the population, it can only be so on the plan of early maturity. Nor will it be questioned, that the admixture with their otherwise vegetable diet, of the meat so produced is, in practice, of great advantage to the health and vigour of those who consume it.

It is true that individual joints or other parts, as sold, will frequently have a less proportion of fat to flesh-forming matter than, according to the above supposition, will be consumed. Some fat will also be removed in the process of cooking. But this portion will generally still be consumed in some form. And where fresh meat is bought, so also are suet, lard, and butter, which, either add to the fatness of the cooked meats, or are used further to reduce the relative flesh-forming capacity of the collaterally consumed vegetable foods.

It would, indeed, appear to be unquestionable, that the influence, on the large scale, of the introduction of animal food to supplement our otherwise mainly farinaceous diet, is to reduce and not to increase the relation of the nitrogenous or peculiarly flesh-forming to the non-nitrogenous constituents (reckoned in their respiratory and fat-forming capacity) of the food consumed.

That, nevertheless, a diet containing a due proportion of animal

food is, for some reason or other, generally better adapted to meet the collective requirements of the human organism than an exclusively bread or other vegetable one, the testimony of common experience may be accepted as sufficient evidence. Whatever may prove to be the exact explanations of the benefits arising from a mixed animal and vegetable diet, it is at any rate pretty clear, that, independently of any difference in the physical, and perhaps even chemical relations of the nitrogenous compounds, they are essentially connected with the amount, the condition, and the distribution of the *fat* in the animal portions of the food.

Fat is the most concentrated respiratory, and of course fat-storing material also, which our food-stuffs supply. It cannot be doubted that, independently of the mere supply of constituents, the conditions of concentration, of digestibility, and of assimilability of our different foods must have their share in determining the relative values, for the varying exigences of the system, of substances which, in a more general or more purely chemical sense, may still justly be looked upon as mutually replaceable.

By the aid of chemistry it may be established that, in the admixture of animal food with bread, the relation (in respiratory and fat-forming capacity) of the non-flesh-forming to the flesh-forming substances will be increased, and, further, that in such a mixed diet the proportion of the non-flesh-forming constitutents, which will be in the concentrated form, so to speak, of *fat itself*, will be considerably greater than in bread alone. Common experience also testifies to the fact of advantages so derived. It remains to Physiology to lend her aid to the full explanation of that which Chemistry and common usage have thus determined.

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

I. Note "On the Formation of the Peroxides of the Radicals of the Organic Acids." By B. C. Brodie, F.R.S., Professor of Chemistry in the University of Oxford. Received July 22, 1858.

The researches of Gerhardt showed a close resemblance which exists between the monobasic organic acids and the metallic protoxides. We have the chloride of acetyl corresponding to the chloride of the

metal, and the hydrated and anhydrous acetic acid corresponding to the hydrated and anhydrous oxide. These investigations have been succeeded by others, which have had their origin in the consistent development of these ideas. The following discovery extends and completes these analogies. I have to add a new term to this series, of which hitherto no analogue has existed. This term is the peroxide of the organic radical,—the body which in the series of acetyl corresponds to the peroxide of hydrogen or barium in the series of the metal. Of these remarkable substances I have prepared two,—the peroxides of benzoyl and of acetyl; but the method by which these are procured is doubtless of extensive application, and we may consider ourselves as in possession of a class of bodies of a new order, the study of which cannot fail greatly to extend our knowledge.

These peroxides are prepared by the action of the anhydrous acid, or the corresponding chloride, upon the peroxide of barium. It is first necessary to prepare this peroxide in a pure condition. This is effected by precipitation of the solution of the peroxide of barium in hydrochloric acid by baryta water, and by drying *in vacuo* the precipitate thus obtained. The peroxide of barium thus procured is perfectly pure, with the exception of a trace of carbonate. In appearance it resembles magnesia.

To prepare the peroxide of benzoyl, the chloride of benzoyl and the peroxide of barium are taken in equivalent proportions and mixed in water. A mutual decomposition takes place; and a substance is formed which, after crystallization from anhydrous ether, gave the following results to analysis:—

Carbon	69·23
Hydrogen	4·10
Oxygen	26·67
	100·00

The calculated numbers for the peroxide of benzoyl are

C_{14}	168	69·42
H_{10}	10	4·13
O_4	64	26·45
	242	100·00

This substance contains an atom of oxygen more than the anhy-

drous acid, and (reducing the formula to its simplest expression) one atom of hydrogen less than the hydrated acid. Thus we have $C_{14}H_{10}O_3$ anhydrous benzoic acid, $C_{14}H_{10}O_4$ peroxide of benzoyl, and $C_7H_6O_2$ hydrated benzoic acid, $C_7H_5O_2$ peroxide of benzoyl, as we have H_2O water, and H_2O_2 or HO for the peroxide of hydrogen. This body crystallizes from ether in large and brilliant crystals. Heated a little above the boiling-point of water, it decomposes, with a slight explosion and the evolution of carbonic acid. Boiled with a solution of potash, it is resolved into oxygen gas and benzoic acid.

The peroxide of acetyl is prepared by mixing anhydrous acetic acid and peroxide of barium, in equivalent proportions, in anhydrous ether. The mixture is to be effected very gradually, being attended with evolution of heat. The ether, after filtration from the acetate of baryta produced, is to be carefully distilled off at a low temperature, and the fluid which remains washed with water. After three or four washings, the water ceases to be acid, and a viscid liquid remains, which is the peroxide of acetyl. This substance possesses the following properties:—It is extremely pungent to the taste; the smallest portion of it placed upon the tongue burns like cayenne pepper. The substance suspended in water immediately decolorizes a solution of sulphate of indigo. It instantly peroxides the protoxide of manganese, and converts the yellow prussiate of potash to the condition of red prussiate. Baryta-water poured upon the substance is converted to the condition of peroxide of barium, with formation of acetate of baryta. Lastly, a single drop of the substance itself, placed on a watch-glass and heated, explodes with a loud report, shivering the glass to atoms.

To analyse the peroxide of acetyl, I availed myself of its decomposition by baryta-water. An undetermined quantity of the substance was thus decomposed, and the oxygen estimated which was evolved by the decomposition of the peroxide of barium formed, by platina-black, and the acetate of baryta determined as sulphate. The result is the same as though the peroxide of acetyl were decomposed into anhydrous acetic acid and oxygen, thus,

$$C_4H_6O_4 = C_4H_6O_3 + O.$$

Thus for every 16 parts of oxygen evolved, 2 equivalents of acetate

of baryta and 1 of sulphate of baryta, $SO_4\,Ba_2$, would be produced. Now we have

$$\begin{array}{ccccccc} SO_4\,Ba_2 & & O & & & & \\ 233{\cdot}2 & : & 16 & :: & 100 & : & 6{\cdot}86. \end{array}$$

In the actual experiment 1·776 gr. of sulphate of baryta was obtained, and 0·1225 of oxygen evolved.

$$1{\cdot}776 \;:\; 0{\cdot}1225 \;::\; 100 \;:\; 6{\cdot}89.$$

It has not yet been in my power to pursue further the study of these substances. I may, however, observe, that the peroxide of acetyl contains the elements of carbonic acid and of the acetate of methyl, and the peroxide of benzoyl the elements of carbonic acid and of the benzoate of phenyl. I have ascertained that the peroxide of benzoyl, when carefully heated, loses exactly one equivalent of carbonic acid; but the substance formed, although isomeric with the benzoate of phenyl, has not the properties of that body. It is a yellow resin, soluble in ether and alkalies, from which latter solution it is precipitated by acids.

The existence of a hydrated peroxide may be anticipated, intermediate between the organic peroxide and the peroxide of hydrogen, in the same sense as the organic acid is intermediate between water and the anhydrous acid. This substance in the series of benzoyl would be isomeric with salicylic acid. My efforts, however, to procure these bodies have, as yet, been unsuccessful; and it is to be remembered that we have no evidence of the existence of a hydrated peroxide of barium, or of any other metal, corresponding to the hydrated protoxide. In the series of ethyl the diatomic alcohol of of Wurtz ($C_2\,H_6\,O_2$) is isomeric with the hydrated peroxide. But the true peroxide of ethyl remains yet to be discovered.

The question naturally arises as to what would be the result of making similar experiments with the chlorides and the anhydrides of the bibasic acids. Now carbonic acid may be regarded as the peroxide of oxalic acid: it is the constant product of the action of oxidizing agents upon that body; and were we able to procure the unknown anhydride of oxalic acid, it would not be an unreasonable anticipation that with the peroxide of barium it would decompose into oxalate and carbonic acid, thus

$$2\,C_2\,O_3 + Ba_2\,O_2 = C_2\,O_4\,Ba_2 + 2CO_2.$$

A similar experiment with anhydrous succinic acid would produce succinate of baryta and a homologue of carbonic acid, the existence of which is also indicated by other considerations. It is premature to dwell upon this point; but in this direction also I have made some experiments.

II. "Notice of Researches on the Sulphocyanide and Cyanate of Naphtyl, conducted by VINCENT HALL, Esq." By A. W. HOFMANN, Ph.D., F.R.S. &c. Received August 10, 1858.

The transformation of phenylcarbamide and phenylsulphocarbamide under the influence of anhydrous phosphoric acid, respectively into cyanate and sulphocyanide of phenyl, an account of which I submitted to the Society several months ago, suggested the probability that the hitherto unknown cyanates and sulphocyanides of radicals similar to phenyl might be obtained by analogous processes.

To establish this point experimentally, Mr. Vincent Hall has examined, in my laboratory, the deportment of some of the derivatives of *naphtylamine* under the influence of agents capable of fixing ammonia and its analogues.

Mr. Hall has found that the crude naphtaline, such as it is obtained from the gas-works, submitted at once, without sublimation, to the action, first of fuming nitric acid, and subsequently of acetic acid and metallic iron, furnishes the naphtylamine sufficiently pure for these experiments. The crude product thus obtained was digested with bisulphide of carbon in order to convert it into *naphtylsulphocarbamide*.

By distilling naphtylsulphocarbamide with anhydrous phosphoric acid, Mr. Hall has obtained a beautiful crystalline compound of a faint but peculiar odour, readily fusible, easily soluble in alcohol and ether, insoluble in water.

The analysis of this compound has led to the formula

$$C_{22}H_7NS_2 = C_{20}H_7, C_2NS_2,$$

showing that it is in fact *sulphocyanide of naphtyl*, formed according to the equation:—

$$\left.\begin{matrix} (C_2 \ S_2)'' \\ (C_{20}H_7)_2 \\ H_2 \end{matrix}\right\} N_2 = \left.\begin{matrix} C_{20}H_7 \\ H_2 \end{matrix}\right\} N + C_{20}H_7, C_2NS_2.$$

Boiled with an alcoholic solution of naphtylamine, this compound readily reproduces naphtylsulphocarbamide, which by its insolubility is easily distinguished and separated from the sulphocyanide.

Gently heated with phenylamine, the new sulphocyanide gives rise to the formation of a crystalline compound, of properties very similar to those of the naphtylsulphocarbamide. This new body is *phenyl-naphtyl-sulphocarbamide**, containing—

$$C_{34}\,H_{14}\,N_2\,S_2 = \left.\begin{matrix} (C_2\,S_2)'' \\ C_{12}\,H_5,\,C_{20}\,H_7 \\ H_2 \end{matrix}\right\}N_2.$$

Naphtylcarbamide, as obtained by the action of potassa on the corresponding sulpho-compound, or by the distillation of oxalate of naphtylamine, is likewise powerfully attacked by anhydrous phosphoric acid. Among the products of distillation a compound is found, which, by its chemical properties, is readily identified as *cyanate of naphtyl*.

$$C_{22}\,H_7\,NO_{20} = C_2\,H_7,\,C_2\,NO_2,$$

although the small quantity in which this body is produced—by far the greater amount of the naphtylcarbamide being charred by the action of anhydrous phosphoric acid—has hitherto prevented Mr. Hall from fixing the nature of the compound by an analysis.

* By the action of sulphocyanide of phenyl upon naphtylamine, I have obtained a crystalline compound very similar in its general characters to the body which Mr. Hall procures by the action of sulphocyanide of naphtyl on phenylamine. This substance likewise contains

$$C_{34}\,H_{14}\,N_2\,S_2 = \left.\begin{matrix} (C_2\,S_2)'' \\ C_{12}\,H_5,\,C_{20}\,H_7 \\ H_2 \end{matrix}\right\}N_2,$$

for

$$C_{12}\,H_5\,H_2N + C_{20}\,H_7\,C_2\,NS_2 = C_{20}\,H_7,\,H_2\,N + C_{12}\,H_5,\,C_2\,NS_2 = C_{34}\,H_{14}\,N_2\,S_2.$$

Are these two bodies identical, or only isomeric? [A.W.H.]

III. "Preliminary Account of an Inquiry into the Functions of the Visceral Nerves, with special reference to the so-called 'Inhibitory System.'" By JOSEPH LISTER, Esq., F.R.C.S. Eng. & Edin., Assistant Surgeon to the Royal Infirmary of Edinburgh; in a Letter to Dr. Sharpey, Sec.R.S. Received August 13, 1858. Communicated by Dr. SHARPEY.

MY DEAR SIR,—The fact that the irritation of visceral nerves sometimes causes arrest of the movements of organs supplied by them, as shown by Edward Weber's experiment of stopping the action of the heart by stimulating the vagus, and by Pflüger's more recent observation that the application of galvanism to the splanchnic nerves produces quiescence of the small intestines, appears to me to have an intimate bearing upon the question how inflammation is developed through the medium of the nervous system at a distance from an irritated part; and as the nature of the inflammatory process has lately engaged my especial attention, I have been led to make an experimental inquiry into this "inhibiting" agency, the true interpretation of which is, as you are aware, still *sub judice*. I now propose to state the principal results at which I have arrived, reserving further details for a more extended communication which I hope soon to offer to the Royal Society.

The view which has been advocated by Pflüger*, and I believe very generally accepted, viz. that there is a certain set of nerve-fibres, the so-called "inhibitory system of nerves" (Hemmungs Nervensystem), whose sole function is to arrest or diminish action, seemed to me from the first a very startling innovation in physiology; and you may possibly recollect my mentioning to you in conversation, when in London last Christmas, my suspicion that the phenomena in question were merely the effect of excessive action in nerves possessed of the functions usually attributed to them. On further reflection upon the subject, the consideration of the contraction produced in the arteries of the frog's foot by a very mild stimulus, as compared with the relaxation of the vessels caused by stronger irritants acting through the same nerves, confirmed my previous notions.

* Eduard Pflüger ueber das Hemmungs Nervensystem, 1857.

For I could hardly doubt that the cause of the quiescence of the heart or intestines on irritation of the vagus or splanchnic nerves was analogous to that of arterial dilatation in the web, and that, provided a sufficiently mild stimulus were applied to the so-called "inhibitory nerves," increased action of the viscera would occur, corresponding to the vascular constriction.

To test the truth of this hypothesis, I made several experiments between the 17th of June and the 14th of July of this year, with regard to the movements of the heart and intestines. The means used for stimulating the nerves and spinal cord were sometimes mechanical irritation, but more commonly galvanism, applied with a magnetic coil battery of a single pair of plates, the strength of which could be regulated in a rough way, with great facility, by the height at which the acid solution stood in the jar and the extent to which the rods of soft iron were inserted in the helix. The mildest action employed was such as was but just perceptible to the tip of the tongue, placed between the fine silver-wire extremities of the poles, when the rods were fully in the helix, but inappreciable after their complete withdrawal; the spring carrying the magnetic bar being made to vibrate by a touch with the finger: the greatest action of the battery, on the other hand, was so powerful as to elicit sparks when the poles were applied to the tissues.

My attention was first directed to the intestines, and it may be well to mention first all the results obtained with reference to them. The animals operated on were generally rabbits, they being very easily managed, and also favourable for the purpose on account of the large amount of movement which occurs in their intestines. Chloroform was generally not administered, on account of its depressing effect upon the action of the nervous centres.

In the first experiment, the ends of the poles having been fixed to the spinous processes of the ninth and twelfth dorsal vertebræ, according to Pflüger's original method, and the intestines allowed to protrude through a wound in the abdominal parietes, a series of interrupted currents were transmitted, a very small amount of acid being in the jar, and the rods fully in the helix. The effect was complete relaxation and quiescence of the small intestines, which had been previously in considerable movement, while the muscles of the limbs were thrown into spasmodic action; but on the discontinuance

of the galvanism the previous intestinal motion returned. The rods were then removed from the helix, and the battery, thus diminished, was applied on several occasions, with markedly increased action of the intestines in every instance during the first twenty-five minutes. In the next half hour the increase of action from the galvanism, though still distinct, was less strongly marked; and at the end of that period, the rods having been reintroduced, the inhibiting influence was also found to be much less complete than before, indicating that the parts of the nervous apparatus concerned were in a less active condition, no doubt in consequence of exhaustion. The arches of the tenth and eleventh dorsal vertebræ having been removed before the experiments with galvanism, I subsequently introduced a fine needle into the exposed part of the cord, with the effect of causing in repeated instances increased movements of the intestines, which were especially striking on account of the occurrence of peculiar local contractions not seen at other times. Further observations upon this animal tended to confirm those which have been mentioned, as did an experiment of the same kind performed the next day upon another rabbit.

I afterwards found that the best mode of proceeding was to remove the skin and one or two layers of muscles from a portion of the abdomen till the parietes were sufficiently thinned to permit the intestines to be distinctly seen through them; by this means the complication produced by exposure of the intestines to the atmosphere was avoided, and the most satisfactory results were obtained; the increase of the peristaltic movements during the transmission of extremely feeble shocks being strikingly apparent and constant on every occasion. During the experiment performed in this way I noticed several times that a violent struggle on the part of the rabbit, when the intestines were in pretty free movement, was followed by absolute and universal quiescence of those organs for several seconds; this appeared to me of great interest, as proving that the inhibitory influence is certainly sometimes exerted in the natural actions of the animal, and is not merely the result of artificial stimulation.

In the course of the above experiments several other observations were made. In the first place I verified the statement of Pflüger, that if, when the intestine is lying relaxed under the inhibiting

influence of galvanism applied to the spine, a particular part be irritated, local contraction occurs, but is not propagated to neighbouring parts. This fact is of fundamental importance, since it proves that the inhibitory influence does not operate directly upon the muscular tissue, but upon the nervous apparatus by which its contractions are, under ordinary circumstances, elicited.

Another point which seemed to require investigation was the well-known increase of peristaltic action which takes place after death, and which continues in spite of cutting off the mesentery close to the gut. Those who believe in a constantly restraining function of certain nerves during life might argue that the intestine has always a tendency to such active movements, but is kept in check by the "inhibitory nerves," and released from their control when they have lost their power after death. A different explanation, first suggested, I believe, by Bernard, is that the increased action of the intestines is the result of failure of the circulation in the part; and to this view I felt disposed to agree, in consequence of having noticed curious irregular contractions in the arteries of the frog's foot from a similar cause. In order to decide the question, I tied three adjoining arterial branches in the mesentery of a rabbit, thus depriving about 3 inches of the intestine of its circulation; the parts so affected being accurately defined by the extent of absence of pulsation in the minute vessels close to the gut. In about a minute and a half, vermicular movements commenced in this part; the rest of the intestines being at the time very quiet. Powerful interrupted galvanic currents were then transmitted through the posterior dorsal region of the spine, with the effect of causing perfect quiescence of the whole of the intestine, including the part whose arteries had been tied. After cessation of the galvanism the movements recurred in the portion devoid of circulation, while elsewhere they were almost entirely absent. This experiment was repeated on another occasion with similar results. In one of the cases I divided the mesentery close to the gut, after ligature of the vessels, but no change took place in the character of the movements which had been previously induced, indicating that the increased action in these cases had been of the same nature as that which results from death. The arrest of the movement on the application of galvanism proved that the delicate operation of ligature of the mesenteric vessels had been performed without

injury to the adjacent nervous branches; and it therefore followed that the movement in the parts supplied by those vessels was not due to any injury of the nerves, but simply to the arrest of circulation. It further appears from these experiments, that, in whatever way the cessation of the flow of blood through the vessels operates in increasing the peristaltic action, it does so through the medium of the nervous apparatus, and not by directly influencing the muscular tissue. For, in the latter case, the movement would have continued in spite of the inhibiting influence, which, as we have seen, has no effect upon muscular irritability.

The fact that the movements continue in a portion of gut deprived of its mesentery, proves that the nervous apparatus by which the muscular contractions are induced and coordinated in *post mortem* peristaltic action, is contained within the intestine.

The distinction between the coordinating power and muscular contractility was very strikingly shown in the further progress of one of these experiments. The peristaltic movements of the portion of gut supplied by the ligatured arteries ceased entirely about twenty minutes after the vessels were tied, and the surface of the gut became there perfectly smooth and relaxed, contrasting strongly with the wrinkled aspect of other parts. But muscular irritability had outlived the coordinating power, as was shown by energetic, purely local contraction taking place in a part pinched. Similar observations confirmatory of this point were afterwards made upon a rabbit which had died of hæmorrhage an hour before.

The mechanism by which the muscular contractions are regulated is, doubtless, the rich ganglionic structure lately demonstrated in the submucous tissue by Dr. Meissner of Bâle*. Professor Goodsir gave me the first information of this anatomical fact on my mentioning to him the foregoing physiological proofs of the existence within the intestines of a coordinating apparatus distinct from the muscular tissue. I have since verified Meissner's observations, and found abundant well-marked nerve-cells in the submucous tissue of the Ox, exactly corresponding with his descriptions.

But while muscular irritability outlives the coordinating power in the intestines, the latter lasts much longer than the inhibiting

* Henle and Pfeufer's Zeitschr. 2nd series, vol. viii.

property in the spinal system, for I find that Pflüger's experiment does not succeed in a dead animal, unless performed soon after death, although the intestines may continue to move for a long time.

In another experiment I divided with fine scissors, at a little distance from the intestine, all the visible branches of nerves in a portion of mesentery corresponding to an inch and three-quarters of the gut, leaving the vessels uninjured. No effect was produced on the peristaltic movements, which happened to be pretty active at the time, and continued the same at the seat of the operation as elsewhere. To ascertain whether the division of the nerves had been thoroughly effected, I now transmitted powerful galvanic currents through the spine, as in former experiments; when all movements ceased in the intestine, except in the small piece whose nerves had been cut, which continued in vigorous action as before. The persistence of the vermicular motion after complete division of the mesenteric nerves shows that the movement which occurs during life, like that which takes place *post mortem*, is effected by a mechanism within the intestine; and its continuance in the portion of gut so treated, while other parts were relaxed, on the application of galvanism to the spine, proves that the inhibiting influence acts through the mesenteric nerves, whose integrity is necessary to the effect.

This being established, it follows that if a quiet state of the intestine, such as very frequently occurs in its natural condition, were due to a controlling agency on the part of the so-called "inhibitory system," the complete division of the mesenteric nerves supplying a portion of gut which is at rest, would liberate it from this restraint, and movement would be the result. I performed the operation in one case under such circumstances, but the portion of intestine concerned remained as tranquil as the rest.

To sum up the above, it appears that the intestines possess an intrinsic ganglionic apparatus which is in all cases essential to the peristaltic movements, and, while capable of independent action, is liable to be stimulated or checked by other parts of the nervous system; the inhibiting influence being apparently due to the energetic operation of the same nerve-fibres which, when working more mildly, produce increase of function.

After the above conclusions had been arrived at, my attention was

directed by Professor Goodsir to a paper by Dr. O. Spiegelberg, published last year, in which he shows that the movement of the intestines is increased by mechanical irritation of the cord. His results are particularly satisfactory, as having been obtained incidentally during an inquiry into the movements of the uterus, and so without any preconceived theory*. Spiegelberg also attributes the increased peristaltic action after death to arrest of the circulation; having found that the same thing occurs during life, when the aorta or vena cava is compressed above the origin of the mesenteric vessels.

To proceed to the experiments upon the cardiac movements: some of these consisted in irritation of the vagus in rabbits, and this was followed by different results in different instances: thus, on one occasion the pinching of the cardiac end of the left nerve, divided in the neck, was followed by considerable increase in the number of beats as felt through the walls of the chest, but similar treatment of the right nerve afterwards caused great depression of the heart's action. Again, in one animal the evidence obtained from mechanical irritation of the vagus was almost entirely negative. In another case, the left vagus having been exposed, feeble galvanic currents transmitted through the nerve, isolated by a plate of glass placed beneath it, were succeeded by slight increase in the number of contractions. The strength of the battery having been then increased by introducing the rods into the helix, it produced first irregularity, and then complete arrest of the action of the heart, which had been previously exposed. No sign of recurrence of contraction appearing, I filled the jar to the top with acid solution, and sent powerful currents through the vagus, with the instantaneous effect of reviving the action of the heart, which, on their immediate discontinuance, continued to beat, though feebly, for several minutes. During this time I again applied the galvanism very mildly, and the result was great increase in the number of beats on several successive trials. The apparent discordance of these facts is, I believe, partly owing to differences in the state of the nerves in different cases as respects irritability and exhaustion, as will be better understood from the sequel; and, on the whole, the experiments appear to show that, in a

* Henle and Pfeufer's Zeitschrift, 3rd series, vol. ii. pt. 1.

healthy state of the nervous system, very gentle irritation of the vagus increases the heart's action, while a slightly stronger application diminishes the frequency and force of its contractions. This conclusion is in harmony with an observation which I made incidentally upwards of a year ago, that irritation of the posterior part of the brain of a frog with a fine needle was repeatedly followed by improvement in the circulation, whereas it was by the application of a stronger stimulus, that of galvanism, to the same part of the cerebro-spinal axis that Weber first induced an inhibitory action on the heart.

It is said, on apparently good authority*, that division of the vagus in mammalia is invariably followed by increase of the action of the heart; this, if true, would be a strong ground for believing in an inhibiting influence constantly operating upon it through this nerve. But it is also stated that the same thing does not occur in frogs; and this circumstance appeared to me to throw much doubt upon the evidence regarding mammalia. I therefore made careful experiments on the effects of cutting both vagi, once upon a calf and four times upon rabbits; taking the number of the heart's beats immediately before and immediately after section of each nerve by the momentary stroke of a sharp pair of scissors. In no case was the rate increased at all by the operation, and the very gradual diminution in frequency that commonly took place appeared to depend on general exhaustion from other circumstances attending the experiment. In one rabbit, in which I had removed the skin and pectoralis major from the præcordial region, so as to see the movements of the heart distinctly through the transparent pericardium and intercostal muscles, I noticed particularly that the strength of the contractions, as well as their frequency, remained quite unaffected by the division of the vagi. From these facts I feel warranted in concluding that, whatever may occur under exceptional circumstances, there is certainly no constant control exercised over the heart's action through those nerves.

The influence of the spinal system upon the heart is, however, very apparent after a struggle, which almost invariably increases the frequency and force of the beats; and I found that this continued

* Pflüger, *op. cit.*

to be the case after division of both vagi, implying that those nerves are not the only channels through which this influence is transmitted. A new field of investigation was thus opened. For, supposing the inhibitory agency to be simply the greater action of an ordinary nerve, it would probably not be exercised exclusively by the vagus, but also by the other nerves connecting the cerebro-spinal axis with the cardiac ganglia, viz. the sympathetic branches in the neck; in which case the action of the heart should be increased or diminished, according to the strength of the stimulus, by the application of galvanism to the cervical region of the spine after the pneumogastric nerves had been cut.

In an experiment performed with this view, the poles having been fixed to about the fourth cervical and fifth dorsal spinous processes, and both vagi divided in the neck, galvanic currents only just perceptible to the tip of the tongue were first transmitted. This excessively feeble action of the battery, though apparently not very favourably situated for influencing the cord, produced marked effects upon the heart's action, increasing the number of beats, which were about forty in ten seconds, by from three to ten in that period. This effect having been observed for a considerable time, the rods of soft iron, which had been till then only inserted half-way in the helix, were pushed fully in. The battery, thus strengthened, instead of increasing, as before, the rate of the pulsations, diminished it by two in ten seconds on several successive trials. On again half withdrawing the rods, the galvanism, when applied, again increased the number of beats. A little more of the acid solution was afterwards poured into the jar of the battery, when the stronger currents which it produced reduced the number by about five in ten seconds.

Yet distinct as was this inhibiting influence, the shocks were still quite tolerable to the tongue even when the rods were fully in the helix.

These results were of great interest, as proving how slight an increase of the feeble stimulus which promoted the action of the heart sufficed to produce the opposite (inhibiting) effect. But it was by no means clear that the influence had not been exerted through cardiac branches arising from the vagi above the parts where they were divided, or even through the trunks of those nerves, which might

possibly have been affected by the galvanism acting through the superjacent spinal column. In order to eliminate the vagi completely, I divided in another rabbit all the soft parts in front of the spine, except the trachea and œsophagus, at the level of the cricoid cartilage, having previously cut each carotid artery between two ligatures. The incisions were carried fairly down to the bodies of the vertebræ, and outwards beyond the tips of the transverse processes, so as to ensure the section not only of the vagi and their branches, but also of the sympathetic cords, with any filaments of those nerves which they might contain. Also the poles of the battery were fixed to the spinous processes of the seventh dorsal and first lumbar vertebræ, so as to avoid all possibility of direct action of the galvanism upon either the vagi or other cardiac nerves. Feeble currents being then transmitted, diminution of the number of beats to the extent of two to four in ten seconds occurred in several successive trials, the results being so constant as to leave no doubt that they were produced by the galvanism.

It may appear almost incredible that such extremely mild galvanic currents, applied through the spinous processes of the posterior dorsal region, should be capable of thus affecting the heart; but that their effects were really very considerable, was clear from the further progress of this experiment and from others somewhat similar, which showed that this apparently trivial stimulation gradually exhausted the part of the nervous system through which the heart is acted on by the cord. Thus, in one case, currents only just perceptible to the tongue, transmitted for about thirty seconds at a time through the lower cervical and upper dorsal regions of the spine, at intervals of nine minutes on the average during two hours and twenty minutes, produced at first decided increase of the heart's action, but during the last hour failed to affect it at all. The strongest possible action of the battery which, as proved by other experiments, would, at the outset, have entirely arrested the cardiac movements, was then set on, but with no effect whatever on the organ.

When partial exhaustion has occurred, a much stronger galvanic stimulus is required, to produce the same effect upon the heart, than at the commencement of an experiment; and thus an action of the battery which, when first applied, causes marked diminution in the number of beats, may after a while come to have the opposite effect,

and increase the heart's action as decidedly as it had previously lowered it; while at an intermediate period it may seem to have no influence at all. This principle gives the clue to understanding what had before appeared incomprehensible in these experiments, showing that facts which at first seemed utterly inconsistent, were really perfectly harmonious. The case before related, in which revival of the heart's action resulted from powerful stimulation of the vagus, which, had the organ been contracting as usual, would have arrested its movements and probably finally destroyed them, will now be understood. I have seen other analogous cases of revival of action by very powerful galvanism, which under ordinary circumstances would have arrested it, viz. twice in the heart and twice in the intestines. The observation published so long ago as 1839 by Valentin*, that mechanical or chemical irritation of the vagus in the neck of an animal recently dead, and with the nerves consequently enfeebled, causes contraction of the ventricles, admits of a similar interpretation, as also does a corresponding fact regarding the splanchnic nerves, given without explanation by Kupfer and Ludwig, in a paper just published†, viz. that they lose their inhibitory influence a certain time after death, and acquire a motor power over the intestines.

Two more experiments require mention, as they exclude the possibility of the agency in them, of either the vagi or the part of the brain from which the vagi spring, having been performed upon decapitated rabbits. In one of these cases, the carotids having been tied near the head, the neck was completely severed behind the first vertebra, care being taken to avoid hæmorrhage from the vertebral arteries, and artificial respiration, for which provision had been made, was carried on for an hour and a half after decapitation. The results of moderate galvanism, applied to the posterior dorsal region of the spine, to which the poles had previously been attached, were at first not distinct, but afterwards decided increase of action was produced by it when applied at intervals during half an hour; the effect being perfectly apparent in the heart which lay exposed before me. Exhaustion of the nerves concerned having then taken

* Valentin, De Functionibus Nervorum, p. 62.

† Henle and Pfeufer's Zeitschrift, 3rd series, vol. ii. pt. 3.

place, the most powerful action of the battery failed to influence the character of the contractions.

In the other case, the poles having been fixed as before, and the head similarly removed, powerful galvanic currents were immediately transmitted. The pulsations of the heart in the opened chest at once fell from thirty-five to sixteen in ten seconds, but rose again to twenty on the removal of the stimulus.

Hence it is clear that the sympathetic branches connecting the cord with the cardiac ganglia have equal claims with the vagi to be called "inhibitory nerves." In fact this expression seems to me altogether objectionable, since there is good reason to think that the same fibres which check the movements, much more commonly enhance them. The only evidence afforded by my experiments that the inhibiting influence is ever exerted in the natural actions of the animal, consisted in the quiescence of the intestines sometimes seen after a struggle, and two doubtful observations of retardation of the heart's beats from the same cause. Indeed it appears very questionable whether the motions of either of these viscera are, under ordinary circumstances, ever checked by the spinal system, except for very brief periods; whereas the increased action of both heart and intestines, familiarly known to result from mental emotion, may last for a very considerable time. The fact that the nerves of these organs are capable of setting them at rest under conditions of extraordinary irritation is nevertheless a matter of great importance, especially in a pathological point of view, and appears to afford an explanation of facts in medicine hitherto little understood, —such as failure of the heart's action from violent emotion or pain, and the constipation which attends strangulated omental hernia.

From the observations of Spiegelberg*, it would appear that the uterine contractions are promoted by mechanical irritation of the cord, and arrested by transmitting a powerful stream of galvanism through the spine. Also the forcible expulsion of urine very frequently seen in the lower animals in consequence of fear, and the temporary palsy of the detrusor often witnessed in the human subject in surgical practice as the result of severe injury, seem to me

* Henle and Pfeufer's Zeitschrift, 3rd series, vol. ii. part. 1.

to imply that the bladder, too, while sometimes stimulated through the cerebro-spinal axis, is paralysed by its very powerful operation. Hence it seems probable that the movements of all the hollow viscera are liable to similar influence from the spinal system. At the same time it appears to be a mistake to regard this influence in the light of a strict control; for the experiments related in this letter show pretty distinctly that the contractions of the heart and the peristaltic action of the intestines are regulated, under ordinary circumstances, by the independent operation of the intrinsic ganglia.

Professor Schiff has, I understand, observed increase of the heart's action to result from very gentle stimulation of the vagus*, and has come to the conclusion, as stated by Spiegelberg in his paper before referred to, that the inhibiting influence depends upon nervous exhaustion. There are some circumstances which make me entertain great doubt as to the correctness of this view. In the first place, the very rapid recovery of the cardiac or intestinal actions when the inhibiting galvanic currents are discontinued, contrasts strongly with the length of time that the impairment of function resulting from a protracted experiment, and certainly due to exhaustion, lasts both in the intrinsic cardiac nerves and in those that connect them with the spinal system. Secondly, although very powerful galvanism not only arrests for the time, but permanently impairs the action of the heart, no such effect is observed to follow the inhibiting influence when it is caused by milder stimulation; indeed, according to my experience, less injurious effects are produced upon the heart by a protracted series of experiments of the latter kind than by a corresponding set with the currents still more feeble, that increase, while acting, the frequency of the contractions. But if the diminished rate of the pulsations were caused by a partial exhaustion of the cardiac ganglia, an opposite result might have been anticipated.

Again, there can be little doubt that dilatation of the blood vessels, in consequence of a stimulus, is due to an effect produced upon the nervous centres for the arteries, similar to that experienced by the visceral ganglia when subject to the inhibiting influence. Now an inflammatory blush of long continuance may subside rapidly when

* Henle and Meissner's Bericht, 1857.

the source of irritation is withdrawn. Thus I have seen redness which had existed for about three days in the human skin in consequence of tight stitches connecting the lips of a wound, give place at once to pallor on their removal. Had the arterial dilatation in this case been the result of nervous exhaustion continued during so long a period, such speedy recovery could hardly, one would think, have taken place.

These and other considerations, to which the already excessive length of this letter forbids me to allude, induce me to think it safest in the present state of science to regard as a fundamental truth not yet explained, that one and the same afferent nerve may, according as it is operating mildly or energetically, either exalt or depress the functions of the nervous centre on which it acts. It is, I believe, upon this that all inhibitory influence depends, and I suspect that the principle will be found to admit of a very general application in physiology.

I am, &c.,

JOSEPH LISTER.

November 18, 1858.

RICHARD OWEN, Esq., Vice-President, in the Chair.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting for the election of Council and Officers.

William Henry Harvey, M.D., was admitted into the Society.

Robert William Bunsen, Louis Poinsot, and Carl Theodor von Siebold, were recommended by the Council for election as Foreign Members, and to be balloted for at the next meeting of the Society.

Dr. Arnott, Sir George Back, Mr. Bell, Mr. Hodgson, and Mr. Gwyn Jeffreys, having been nominated by the President, were elected Auditors of the Treasurer's Accounts on the part of the Society.

The Secretary explained that the Croonian Lecture, delivered at the last Meeting, but not yet published in the 'Proceedings,' would be printed in the report of the proceedings of the present Meeting.

The Report of the Joint Committee of the Royal Society and the British Association, on Magnetical and Meteorological Observations, was communicated by order of the President and Council, with the view of its being published in the 'Proceedings.'

Papers were read, from Theophilus Thomson, MD., F.R.S., John Lubbock, Esq., F.R.S., and R. M'Donnell, MD.

I. The Croonian Lecture.—" On the Theory of the Vertebrate Skull." By Thomas H. Huxley, Esq., F.R.S. Delivered June 17, 1858.

The necessity of discussing so great a subject as the Theory of the Vertebrate Skull in the small space of time allotted by custom to a lecture, has its advantages as well as its drawbacks. As, on the present occasion, I shall suffer greatly from the disadvantages of the limitation, I will, with your permission, avail myself to the uttermost of its benefits. It will be necessary for me to assume much that I would rather demonstrate, to suppose known much that I would rather set forth and explain at length; but on the other hand, I may consider myself excused from entering largely either into the history of the subject, or into lengthy and controversial criticisms upon the views which are, or have been, held by others.

The biological science of the last half-century is honourably distinguished from that of preceding epochs, by the constantly increasing prominence of the idea, that a community of plan is discernible amidst the manifold diversities of organic structure. That there is nothing really aberrant in nature; that the most widely different organisms are connected by a hidden bond; that an apparently new and isolated structure will prove, when its characters are thoroughly sifted, to be only a modification of something which existed before,—are propositions which are gradually assuming the position of articles of faith in the mind of the investigators of animated nature, and are directly, or by implication, admitted among the axioms of natural history.

And this is not wonderful; for no living being can be attentively studied without bearing witness to the truth of these propositions. The tyro in comparative anatomy cannot fail to be struck with the resemblances between the leg and the jaw of a crustacean; between the parts of the mouth of a beetle and those of a bee; between the wing of the bird and the fore-limb of the mammal. Everywhere he finds unity of plan, diversity of execution.

Or again, how can the intelligent student of the human frame consider the backbone, with its numerous joints or vertebræ, and trace the gradual modification which these undergo downwards into the sacrum and coccyx, and upwards into the atlas and axis, without the notion of a vertebra in the abstract, as it were, gradually dawning upon his mind; the conception of an ideal something which shall be a sort of mean between these various actual forms, each of which may then easily be conceived as a modification of the abstract or typical vertebra?

Such an idea, once clearly apprehended, will hardly permit the mind which it informs to rest at this point. A glance at a section of that complex bony box formed by the human skull and face, shows that it consists of a strong central mass, whence spring an upper arch and a lower arch. The upper arch is formed by the walls of the cavity containing the brain, and stands in the same relation to it, as does the neural arch of a vertebra to the spinal cord, with which that brain is continuous. The lower arch encloses the other viscera of the head, in the same way as the ribs embrace those of the thorax. And not only is the general analogy between the

two manifest, but a young skull may be readily separated into a number of segments, in each of which it requires but little imagination to trace a sort of family likeness to such an expanded vertebra as the atlas.

What can be more natural then than to take another step—to conceive the skull as a portion of the vertebral column still more altered than the sacrum or the coccyx, whose vertebræ are modified in correspondence with the expansion of the anterior end of the nervous centre and the needs of the cephalic end of the body, just as those of the sacrum are fashioned in accordance with the contraction of the nervous centre and the mechanical necessities of the opposite extremity of the frame?

Two generations have passed away since, perhaps, by some such train of reasoning as this, such a conception of the nature of the vertebrate skull arose in the mind of the philosophic poet, Goethe; and a somewhat shorter period has elapsed since a poetical, or perhaps I might more justly say a fanciful, philosopher, Oken, published a "Theory of the Skull" embodying such a conception; and since the excellent Dumeril allowed a like hypothesis to be strangled in the birth by the small wit of a French academician.

The progress of modern science is so rapid, that one is unaccustomed to see half a century elapse after the promulgation of a doctrine, which is capable of being tested by readily accessible facts, without either its firm establishment or its decisive overthrow. But nevertheless, at the present day, the very questions regarding the composition of the skull, which were mooted and discussed so long ago by the ablest anatomists of the time, are still unsettled; the theory of the vertebrate skull is one of the most difficult and, apparently inextricably confused subjects, which the philosophic anatomist can attack, and in consequence, not a few workers in science look, somewhat contemptuously, upon what they are pleased to term mere hypothetical views and speculations.

Indeed, though the germ of a great truth did really lie in these same hypotheses, its late or early development into a sound, and consequently fruitful, body of doctrine depended upon the manner in which biologists set about solving the problem presented to them; upon the clearness with which they apprehended the nature of the questions they wished to put, and the consequent greater or less

fitness of the method by which their interrogation of nature was conducted.

I apprehend that it has been and is, too often forgotten that the phrase "Theory of the Skull" is ordinarily employed to denote the answers to two very different questions; the first, Are all vertebrate skulls constructed upon one and the same plan?—the second, Is such plan, supposing it to exist, identical with that of the vertebral column?

It is also forgotten that, to a certain extent, these are independent questions; for though an affirmative answer to the latter implies the like reply to the former, the converse proposition by no means holds good; an affirmative response to the first question being perfectly consistent with a negative to the second *.

As there are two problems, so there are two methods of obtaining their solution. Employing the one, the observer compares together a long series of the skulls and vertebral columns of adult *Vertebrata*, determining, in this way, the corresponding parts of those which are most widely dissimilar, by the interpolation of transitional gradations of structure. Using the other method, the investigator traces back skull and vertebral column to their earliest embryonic states, and determines the identity of parts by their developmental relations.

It were unwise to exalt either of these methods at the expense of its fellow, or to be other than thankful that more roads than one lead us to the attainment of truth. Each, it must be borne in mind, has its especial value and its particular applicability, though at the same time it should not be forgotten, that to one, and to one only, can the *ultimate* appeal be made, in the discussion of morphological questions. For seeing that living organisms not only *are*, but *become*, and that all their parts pass through a series of states before they reach their adult condition, it necessarily follows that it is impossible to say, that two parts are homologous or have the same morphological

* There is a wide difference, too, in the relative importance of either question to the student of comparative anatomy. Unless it can be shown that a general identity of construction pervades the multiform varieties of vertebrate skulls, a concise, uniform, and consistent nomenclature becomes an impossibility, and the anatomist loses at one blow the most important of aids to memory, and the most influential of stimulants to research. The second question, on the other hand, though highly interesting, might be settled either one way or the other without exerting any very important influence on the practice of comparative anatomy.

relations to the rest of the organism, unless we know, not only that there is no essential difference in these relations in the adult condition, but that there is no essential difference in the course by which they arrive at that condition. The study of the gradations of structure presented by a series of living beings may have the utmost value in suggesting homologies, but the study of development alone can finally demonstrate them.

Before the year 1837, the philosophers who were occupied with the Theory of the Skull, confined themselves, almost wholly, to the first-mentioned mode of investigation, which may be termed the "method of gradations." If they made use of the second method at all, they went no further than the tracing of the process of ossification, which is but a small, and by no means the most important part of the whole series of developmental phenomena, presented by either the skull or the vertebral column.

But between the years 1836 and 1839, the appearance of three or four remarkable Essays, by Reichert, Hallmann, and Rathke*, inaugurated a new epoch in the history of the Theory of the Skull. Hallmann's work on the Temporal Bone is especially remarkable for the mass of facts which it contains, and for that clearness of insight into the architecture of the skull, which enabled him to determine the homologies of some of the most important bones of its upper arch throughout the vertebral series. Rathke showed the singular nature of the primordial cranial axis, and Reichert pointed out in what way alone the character of its lower arches could be determined. For the first time, the student of the morphology of the skull was provided with a criterion of the truth or falsity of his speculations, and that criterion was shown to be Development.

My present object is to lay before you a brief statement of some of the most important results to which the following out of the lines of inquiry opened up by these eminent men seems to lead. Much

* The titles of these works are,—Reichert, 'De Embryonum arcubus sic dictis Branchialibus,' 1836, which I have not seen; the same writer's essay, 'Ueber die Visceralbogen der Wirbelthiere im Allgemeinen,' Müller's Archiv, 1837. Hallmann, 'Die vergleichende Osteologie des Schläfenbeins,' 1837. Rathke, 'Entwickelungsgeschichte der Natter,' 1839. I regret that, in spite of all efforts, I have hitherto been unable to procure a copy of another very important work of Rathke's, the 'Programm,' contained in the "Vierter Bericht von dem naturwissenschaftlichen Seminär zu Königsberg."

of what I have to say is directed towards no other end than the revival and justification of their views—a purpose the more worthy and the more useful, since with one or two honourable exceptions—I allude more particularly to the recent admirable essays of Prof. Goodsir —later writers on the Theory of the Skull have given a retrograde impulse to inquiry, and have thrown obscurity and confusion upon that which twenty years ago had been made plain and clear.

I have said that the first question which offers itself is, whether all vertebrate skulls are or are not, constructed upon a common plan, and in entering upon this inquiry I shall assume (what will be readily granted), that if it can be proved that the same chief parts, arranged in the same way, are to be detected in the skulls of a Sheep, a Bird, a Turtle, and a Carp, the problem will be solved affirmatively, so far, at any rate, as the osseous cranium is concerned.

Composition of the Skull of a Sheep (fig. 1).

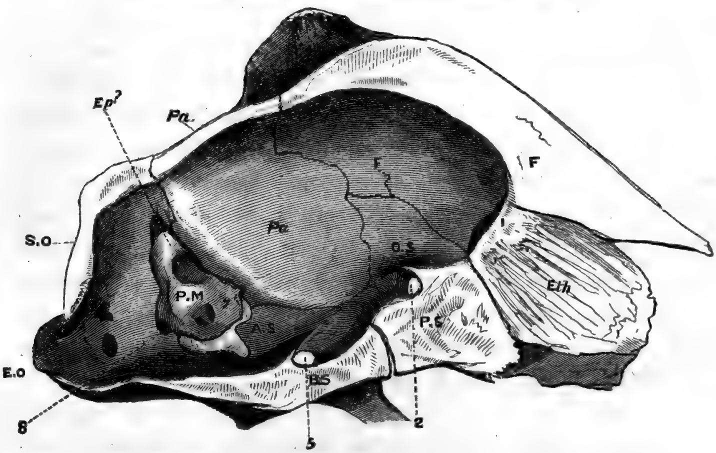


Fig. 1.—Longitudinal section of the skull of a sheep. In this and the following sections of Crania the letters have the same meaning.

B.O. Basioccipital.
B.S. Basisphenoid.
P.S. Presphenoid.
Eth. Ethmoid (lamina perpendicularis).
E.O. Exoccipital.
M. Mastoid.
P. or P.S. Petrosal.
P.M. Petromastoid.

A.S. Alisphenoid.
O.S. Orbitosphenoid.
Pf. Prefrontal.
Sq. Squamosal.
Ep. Epiotic.
S.O. Supraoccipital.
Pa. Parietal.
F. Frontal.

Foramina for nerves.

1. Olfactory; 2. optic; 3 & 4. oculomotor and pathetic nerves; 5. third division of trigeminal; 7. portio dura and mollis; 8. pneumogastric; Epiph. Pineal gland, or epiphysis cerebri.

On examining a section of the cranium of a sheep, made either along a vertical and longitudinal, or a transverse and horizontal plane, a more or less completely ossified mass is observed in the middle line below, which forms part of the floor of the cranial cavity, but extends beyond it. This may be termed the 'craniofacial axis.' Posteriorly it is a broad plate flattened from above downwards, and is nearly parallel with the long axis of the cranial cavity; but from a point immediately behind the sella turcica, it becomes thicker and is compressed from side to side, so that, at the anterior boundary of the sella turcica, the craniofacial axis is much deeper than wide, and assumes the form of a vertical plate. From the anterior boundary of the cranial cavity onwards, or in its facial portion, the axial plate is very deep and very thin, and a line drawn through its longitudinal axis would cut that of the cranial cavity at a very considerable angle. The craniofacial axis then is naturally divisible into three regions; a middle thick part, lodging the sella turcica, and composed of the basisphenoid behind and presphenoid in front, the two being separated by a suture; a posterior, lamellar, horizontally-flattened part, forming in the young animal a distinct bone, the basioccipital, bounding the occipital foramen behind and uniting with the basisphenoid in front; and an anterior laterally compressed portion, composed of the bony "lamina perpendicularis" of the ethmoid above and behind, united by the cartilaginous septum narium to the bony vomer below. This anterior division of the axis may be termed its ethmovomerine portion. Its posterior edge helps to close the anterior outlet of the cranial cavity, from which it is otherwise completely excluded.

The sella turcica lodges the pituitary body, and the synchondrosial union between the basisphenoid and presphenoid is situated so far forwards that the anterior wall of the fossa is almost wholly formed by the rostrum-like anterior prolongation of the basisphenoid. The spinal cord passes out behind the posterior margin of the basioccipital. The olfactory nerves leave the skull on each side of the ethmovomerine division of the craniofacial axis.

The walls of the cranial cavity are formed by a number of bones, which are divisible into two series, a superior and a lateral. Of the latter, four pairs of bones, separated by natural lines of demarcation, or sutures, are distinguishable, three of which abut directly upon the cranio-facial axis, while the fourth pair are only indirectly connected

with it. Behind are the exoccipitals *, united with the basioccipital, and forming the lateral boundaries of the occipital foramen. In front of these are the petromastoids, complex bones which contain the auditory labyrinth, and are connected with the anterior part of the basioccipital and the posterior and superior part of the basisphenoid, only by cartilage.

Next come the alisphenoids, which are attached to the infero-posterior and the anterior portions of the basisphenoid. And, lastly, the orbitosphenoids articulate with the upper margins of the vertically elongated presphenoid.

In the superior series only four bones can be counted, of which two are single and two are pairs. The hindermost is the supra-occipital bone. It articulates with both the exoccipitals and the petromastoids. The next, in front, is the parietal, single in the adult sheep, but composed of two symmetrical halves in the lamb. It articulates with the petromastoids and with the alisphenoids. The frontals, or anterior paired bones, lastly, unite with the orbito-sphenoids, and, in front of them, with the ethmoid.

Most important relations exist between the contents of the cranium and these constituent elements of its walls. The par vagum makes it exit between the exoccipital and the petromastoid; the portio dura and portio mollis enter the petromastoid; the third division of the trigeminal passes through the large "foramen ovale," which, in the sheep, has the exceptional peculiarity of being situated nearly in the middle of the alisphenoid; the optic nerve passes through a foramen included between the orbito- and pre-sphenoids, while, as has been mentioned above, the olfactory nerve passes out beside the ethmoid and in front of the orbitosphenoid. The relation of the pituitary body, or hypophysis cerebri, to the upper surface of the basisphenoid, has already been alluded to; it, of course, gives more or less nearly the position of the third ventricle and crura cerebri. A style passed horizontally through the corpora quadri-

* In speaking of these bones I shall avail myself, for the most part, of the useful translation of the Cuvierian nomenclature adopted by Prof. Owen. It is, doubtless, more convenient to say "alisphenoid" than "grande aile" or "aile sphenoidale," and "orbitosphenoid" instead of "aile orbitaire," the slightness of the real change thereby effected being one of its principal recommendations. The adoption of the terms will, of course, not be held to imply any recognition of the justice of the views of either their inventor or their adopter.

gemina, or mesencephalon, would strike against, or close to, the anterior margin of the petromastoid bone.

On turning to the exterior of the skull, certain bones come into view which were before invisible, as they take no share in forming the lateral walls of the cranial cavity, but are, as it were, stuck on to the outer surface of these walls. The principal of these is the great squamosal bone, applied to the outer surfaces of the petromastoid, parietal and alisphenoid bones, sending off its zygomatic process to unite with the jugal, and furnishing the articular surface for the condyle of the lower jaw.

Partly articulated with the squamosal and partly with the petromastoid, is the irregular capsule of the tympanic bone, to which the tympanic membrane is attached, on whose removal the ossicula auditûs come into view, consisting of the malleus, incus, and stapes. The processus gracilis of the first of these bones lies between the tympanic and the squamosal. The short process of the incus abuts against the inner wall of the tympanum, just below the squamosal and close to the line of junction of the petrous and mastoid. These are the leading points in the structure of the sheep's cranium to which I wish to direct attention at present. Bearing them in mind, let us now proceed to the consideration of the skull of a bird.

Composition of the Skull of a Bird (fig. 2).

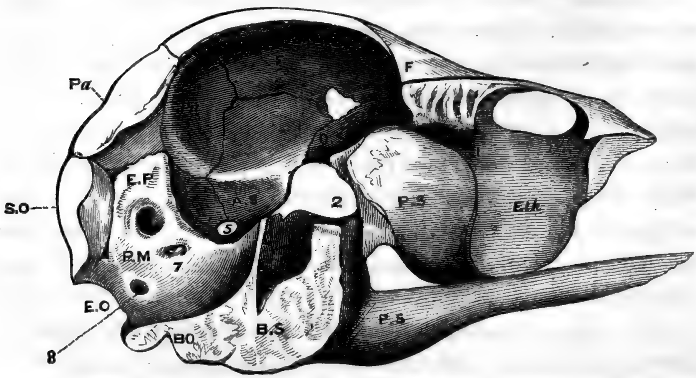


Fig. 2.—Longitudinal section of the Skull of a young Ostrich.

In most adult birds, as is well known, the bones of the cranium have coalesced so completely as to be undistinguishable. But in the chick, and to a greater or less extent, in the adult struthious

bird, the boundaries of the various bones are obvious enough; and I will therefore select for comparison with the mammalian skull that of an ostrich, and that of a young chicken.

The craniofacial axis of the bird has the same general figure as that of the sheep, consisting of a thick, solid, median portion, lodging the sella turcica; of a posterior, horizontally, and of an anterior, vertically, expanded division; but it is comparatively shorter and thicker in correspondence with the greater shortness, in proportion to its depth, of the cranial cavity. The sella turcica is very deep, and its front wall is very thick. The lower and anterior half of this wall is produced into a long tapering process, which extends forwards far beyond the anterior limit of the bony lamina perpendicularis of the ethmoid, to end in a point.

Overlying this process, and articulated with more than the posterior half of its upper surface, there is, in the ostrich, a strong, thick, vertical, bony plate, narrower in front and behind than in the middle, and below than above. A curved vertical ridge on each lateral surface marks the line of its greatest transverse diameter, and seems to indicate a primitive division of the mass into two parts, an anterior and a posterior. The latter is connected above with the bony plates representing the orbitosphenoids. The former exhibits on each side, posteriorly and superiorly, a groove, in which the olfactory nerve rests and, above this, expands into an arched process, which supports the anterior extremity of the frontal bone. Anteriorly, the superior end of the bone widens into a rhomboidal plate, which appears externally between the nasal bones. These anterior and posterior processes of the superior edge of the bone are connected by a delicate ridge, which passes from one to the other above, but leaves an irregular oval gap below.

The anterior edge of the bony plate in question is continued into the unossified septum narium, which below supports the delicate bony representative of the vomer.

In the chick, the whole of the parts just described are unossified, but the composition and structure of the rest of the axis is essentially the same as in the ostrich.

It is not difficult to identify in the craniofacial axis of the bird, parts corresponding with those which have been shown to exist in the mammal. In the chick, the basioccipital can be readily sepa-

rated from the basisphenoid. The latter has the same relation to the sella turcica in the bird as in the mammal; and only differs from it in that singular beak-like process, into which its inferior portion is prolonged anteriorly, and which is produced, according to Kölliker *, by the coalescence with the basisphenoid of a distinct ossification, which is developed in the presphenoidal cartilage and partially represents the presphenoid of the mammal. The rest of the presphenoidal cartilage is more or less completely ossified, and appears to be represented in the ostrich by that part of the "vertical bony plate" which lies behind the curved ridge referred to above; while that part of the plate which is situated in front of the ridge, answers to the lamina perpendicularis of the ethmoid.

Nothing can be more variable, in fact, than the mode in which the ossification of the presphenoidal and ethmoidal portions of the craniofacial axis takes place in birds; while nothing is more constant than the general form preserved by these regions, and their relation to other parts, irrespectively of the manner in which ossification takes place in them. And in these respects birds do but typify the rest of the oviparous *Vertebrata*.

If we compare the inferolateral walls of the ostrich's cranium with those of the sheep, we find the most singular correspondences. Posteriorly are the exoccipitals, which contribute to form the single condyloid head for articulation with the atlas, but otherwise present no important differences. In front of the exoccipital lies a considerable bony mass, which unites, internally and inferiorly, with the basioccipital and basisphenoid bones, and posteriorly is confluent with the exoccipitals. Its anterior margin is distinguishable into two portions, a superior and an inferior, which meet at an obtuse angle. The anterior inferior portion articulates with the alisphenoid; the anterior superior portion with the parietal. The anterior, posterior and inferior, relations of this bone are therefore the same as those of the petromastoid of the sheep.

Superiorly and posteriorly, a well-marked groove (which, however, is not a suture) appears to indicate the line of demarcation between the supraoccipital and this bone, whose pointed upper extremity appears consequently to be wedged in between the supraoccipital and the parietal.

* Berichte von der Königlichen Zool. Anstalt zu Würzburg, 1849, p. 40.

The par vagum passes out between the bony mass under description and the exoccipital; the third division of the trigeminal leaves the skull between it and the alisphenoid. The portio dura and the portio mollis enter it by foramina very similarly disposed to those in the sheep. Superiorly there is a fossa on the inner face of the bone, which corresponds with a more shallow depression in the sheep, and, like it, supports a lobe of the cerebellum. Finally, the anterior inferior edge of the bone traverses the middle of the fossa which receives the mesencephalon. In every relation of importance, therefore, this bony mass corresponds exactly with the petromastoid of the sheep, while it differs from it only in its union with the exoccipitals and the supraoccipital posteriorly, and its contact with the craniofacial axis below.

If from the ostrich we turn to the young chick (fig. 3), the condition of this part of the walls of the skull will be found to be still more instructive. The general connexions of the corresponding bony mass, Pt. M. Ep., are as in the ostrich; but while it is even more evident that the groove appearing to separate its upper end from the supraoccipital is no longer a real suture (whatever it may have been), a most distinct and clear suture, of which no trace is visible in the ostrich's skull, traverses the bone at a much lower point, dividing it into an inferior larger piece, united with the exoccipital, and a superior portion, anchylosed with the supraoccipital. The latter contains the upper portions of the superior and external semicircular canals.

Moreover, on endeavouring to separate the inferior bone from the exoccipital, it readily parts along a plane which traverses the fenestra ovalis externally, and the anterior boundary of the foramen of exit of the par vagum internally. The posterior smaller portion remains firmly adherent to the exoccipital, while the other larger portion comes away as a distinct bone.

The latter answers exactly to the mammalian petrosal, while the small posterior segment corresponds with the mammalian mastoid. Like that of the mammal, it is eventually anchylosed with the petrosal; but unlike that of the mammal, it is also, and indeed at an earlier period, confluent with the exoccipital*.

Thus, to return to the ostrich's skull, the bony mass interposed between the exoccipital, supraoccipital and parietal bones, and the

* See Note I.

craniofacial axis, is in reality composed of three bones, an anterior, petrosal, a posterior, mastoid, and a third, which is distinct from the petrosal and mastoid in the chick, but is anchylosed with them in the ostrich, and which has as yet received no name. I shall term it, from its position with respect to the organ of hearing, the epiotic bone, "os epioticum *."

The homology of the bone here called petrosal, with that of the mammal, is admitted by all anatomists. The bone which lies immediately in front of the petrosal is, with a no less fortunate unanimity, admitted to be the homologue of the mammalian alisphenoid. But it is worthy of particular remark, in reference to the shifting of the relative positions of the lateral elements of the cranial wall, which has been imagined to take place in the ovipara, in consequence of the supposed invariable disappearance of the squamosal from the interior of their skulls; that although precisely the same bones are visible on the inner surface of the cranial cavity in the ostrich as in the sheep, the squamosal being absent in both, yet in the ostrich the third division of the trigeminal does not pass through the middle of the alisphenoid, but between it and the petrosal †.

The orbitosphenoids appear like mere processes of the presphenoid, and their relation to the optic nerves is altered in the same way (when compared with the corresponding bones in the sheep) as that of the alisphenoids to the trigeminal, that is to say the nerves pass behind, and not through them.

The superior series of bones in the cranial wall is exactly the same as in the sheep, and the parietals are distinct in the young ostrich, as in the lamb.

Attached to the exterior of the skull of the ostrich are, as in the sheep, several bones; but the appearance of some of these is widely different from that of the parts which correspond with them in the mammal. This is least the case with the largest and uppermost of these bones, which lies upon the parietal above, the alisphenoid in front, and the exoccipital behind; while internally it is in relation with the petromastoid.

This bone lies immediately above an articular surface, which is furnished to the os quadratum by the petrosal, and more remotely

* My reasons for considering this osseous element to be distinct from the supraoccipital will be given below.

† See Note II.

it helps to roof in the tympanic cavity, but takes no share in the formation of the fenestra ovalis. It sends a free pointed process downwards and forwards, which does not articulate with the jugal. Except in this particular, however, the bone in question resembles in every essential relation the squamosal of the sheep, while to the same extent it differs from the mastoid of that animal.

I have stated that in the ostrich this bone does not appear upon the inner surface of the wall of the skull, and in this respect, while it resembles the squamosal of the sheep and Ruminants generally, t differs from that of most other *Mammalia*, in which the squamosal makes its appearance in the interior of the skull, between the parietal, frontal, alisphenoid and petrosal bones, and so contributes more or less largely to the completion of the cranial wall.

But it has been most strangely forgotten that the relations of the bone in question in birds, are by no means always those which obtain in the ostrich. In the young of the commonest and most accessible of domestic birds, in the chicken, the squamosal may be readily seen to enter largely into the cranial wall; a rhomboidal portion of its anterior and internal surface being interposed in front of the petrosal, between this bone, the parietal, the frontal, and the alisphenoid (Sq. fig. 3).

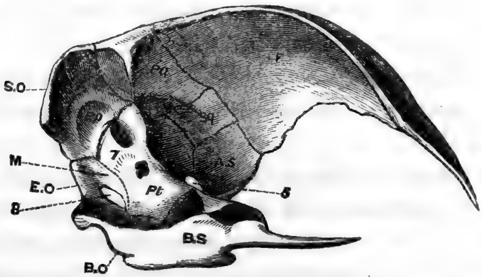


Fig. 3.—Longitudinal section of the Skull of a young Chicken.

There is therefore not a single relation (save the connexion of the jugal) in which this bone does not resemble the squamosal of the *Mammalia*—there is not one in which it does not differ from their mastoid.

The second bone applied externally to the cranium in the bird, is that large and important structure, the os quadratum, which in-

tervenes between the petrosal and squamosal bones above, and the articular portion of the lower jaw below; which articulates with the pterygoid internally, and with the quadratojugal externally, which gives attachment to a part of the tympanic membrane posteriorly, and which is very generally termed the tympanic bone, from its supposed homology with the bone so named in the *Mammalia*. The resemblance to the tympanic bone, however, hardly extends beyond its relation to the tympanic membrane; for in no other of the particulars mentioned above do the connexions of the two bones correspond. The tympanic of the mammal does *not* articulate with the lower jaw, nor with the pterygoid*, nor with the jugal or quadratojugal. On the other hand, if the connexions of the tympanic membrane were sufficient to determine the point, not only the quadratum, but the articular element of the lower jaw, and even some cranial bones, must be regarded as tympanic †.

Again, if we trace the modifications which the tympanic bone undergoes in the mammalian series, we find that in those mammals, such as *Echidna* and *Ornithorhynchus*, which approach nearest to the *Ovipara*, and which should therefore furnish us with some hint of the modifications to which the tympanic bone is destined in that group, the bone, so far from increasing in size and importance, and taking on some of the connexions which it exhibits in the oviparous *Vertebrata*, absolutely diminishes and becomes rudimentary, so that the vast bony capsule of the placental mammal is reduced, in the monotreme, to a mere bony ring.

But it is no less worthy of remark, that in these very same animals the malleus and incus have attained dimensions out of all proportion to those which they exhibit in other mammals, and that they even contribute to the support of the tympanic membrane.

So far, therefore, from being prepared by the study of those *Mammalia* which most nearly approach the *Ovipara*, to find, in the most highly organized of the latter, an immense os tympanicum, with a vanishing malleus and incus, we are, on the contrary, led to anticipate the disappearance of the tympanicum, and the further enlargement of the ossicula auditûs. Thus far the cautious application of the method of gradations leads us, and leads us rightly—

* Though the pterygoid comes close to it in *Monotremata*.

† See Note III.

though the demonstration of the justice of its adumbrations can only be obtained by the application of the criterion of development.

It is twenty-one years since this criterion was applied by Reichert. Since his results were published, they have been, in their main features, verified and adopted by Rathke, the first embryologist of his age; and yet they are ignored, and the quadratum of the bird is assumed to be the tympanic of the mammal, in some of the most recent, if not the newest discussions of the subject. Reichert and Rathke have proved, that in the course of the development of either a mammal or a bird, a slender cartilaginous rod makes its appearance in the first visceral arch, and eventually unites with its fellow, at a point corresponding with the future symphysis of the lower jaw. Superiorly, this rod is connected with the outer surface of the cartilage, in which the petrosal bone subsequently makes its appearance. Near its proximal end, the rod-like "mandibular cartilage" sends off another slender cartilaginous process, which extends forwards parallel with the base of the skull. With the progress of development, ossification takes place in the last-named cartilage, and converts it, anteriorly, into the palatine, and posteriorly, into the pterygoid bone. The mandibular cartilage itself becomes divided into two portions, a short, proximal, and a long, distal, by an articulation which makes its appearance just below the junction of the pterygo-palatine cartilage. The long distal division is termed, from the name of its original discoverer, Meckel's cartilage. It lengthens, and an ossific deposit takes place around, but, at first, not in it. The proximal division in the mammal ossifies, but usually loses its connexion with the pterygoid, remains very small and becomes the incus. In the bird the corresponding part enlarges, ossifies, and becomes the os quadratum, retaining its primitive connexion with the pterygoid. In the mammal, the proximal end of Meckel's cartilage ossifies and becomes the malleus, while the rest ultimately disappears. The ossific mass which is formed around Meckel's cartilage remains quite distinct from the proximal end of that cartilage, or the malleus, gradually acquires the form of the ramus of the lower jaw, and eventually developes a condyle which comes into contact and articulates with, the squamosal. In the bird, on the contrary, the ramus of the jaw unites with the ossified proximal end of Meckel's cartilage; which becomes anchylosed with the ramus, but retaining

its moveable connexion with the quadratum (or representative of the incus), receives the name of the articular piece of the jaw. The rest of Meckel's cartilage disappears.

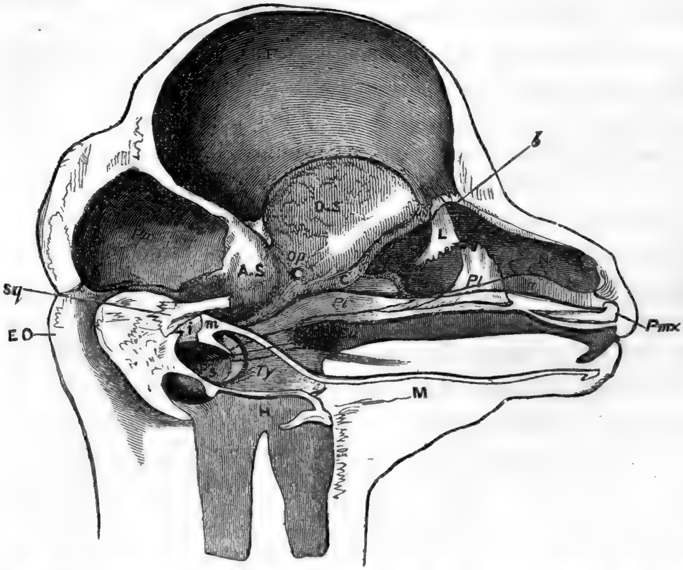


Fig. 4.—Dissection of the cranium and face of a fœtal lamb 2 in. long. The letters have the same signification as elsewhere, except N. Nasal capsules. *a. b. c.* Septum narium. L. Lacrymal. Pl. Palatine. Eu. Arrow indicating the course of the Eustachian tube. *i.* Incus. *m.* Malleus. M. Meckel's cartilage. H. Hyoid. Ps. Petrosal. Ty. Tympanic.

Thus the primitive composition of the mandibular cartilaginous arch is the same in the bird as in the mammal; in each, the arch becomes subdivided into an incudal and a Meckelian portion; in each, the incudal and the adjacent extremity of the Meckelian cartilage, ossify, while the rest of the cartilaginous arch disappears and is replaced by a bony ramus deposited round it. But from this point the mammal and the bird diverge. In the former, the incudal and Meckelian elements are so completely applied to the purposes of the organ of hearing, that they are no longer capable of supporting the ramus, which eventually comes into contact with the squamosal bone. In the latter, they only subserve audition so far as they help to support the tympanic membrane, their predominant function being the support of the jaw.

The tympanic bone of every mammal is, at first, a flat, thin,

curved plate of osseous matter, which appears on the outer side of the proximal end of Meckel's cartilage, but is as completely independent of it as is the ramus of the jaw of the rest of that cartilage. In most birds it has no bony representative*.

It is clear, then, as Professor Goodsir† has particularly stated, that the os quadratum of the bird is the homologue of the incus of the mammal, and has nothing to do with the tympanic bone; while the apparently missing malleus of the mammal is to be found in the os articulare of the lower jaw of the bird.

It would lead me too far were I to pursue the comparison of the bird's skull with that of the mammal further. But sufficient has been said, I trust, to prove that, so far as the cranium proper is concerned, there is the most wonderful harmony in the structure of the two, not a part existing in the one which is not readily discoverable in the same position, and performing the same essential functions, in the other. I have the more willingly occupied a considerable time in the demonstration of this great fact, because it must be universally admitted that the bones which I have termed petrous, squamosal, mastoid, quadratum, articulare in the bird, are the homologues of particular bones in other oviparous *Vertebrata*, and consequently, if these determinations are correct in the bird, their extension to the other *Ovipara* is a logical necessity. But the determination of these bones throughout the vertebrate series is the keystone of every theory of the skull—it is the point upon which all further reasoning must turn; and therefore it is to them, in considering the skulls of the other *Ovipara*, that I shall more particularly confine myself.

Composition of the Skull of the Turtle.

It has been seen that in birds the presphenoid, ethmoid, and orbitosphenoid regions are subject to singular irregularities in the mode and extent of their ossification. In the turtle, not only are the parts of the cranium which correspond with these bones unossified, but its walls remain cartilaginous for a still greater extent. In fact, if a vertical section be made through the longitudinal axis of a turtle's skull, it will be observed that a comparatively small extent of

* See Note III.

† Reichert, however, had already clearly declared this important homology in his 'Entwickelungsgeschichte des Kopfes,' p. 195.

the cranial wall, visible from within, is formed by bone, and that the large anterior moiety is entirely cartilaginous and unossified. The anterior part of the posterior, bony, moiety of the cranial wall is formed by a bone (Pt.), whose long, vertical, anterior-inferior margin forms the posterior boundary of the foramen by which the third division of the trigeminal nerve makes its exit from the skull. The anterior and superior margin of the bone is very short, and articulates with the parietal bone. The superior margin is inclined backwards, and articulates with the supraoccipital. The posterior margin is straight, and abuts against a cartilaginous plate interposed between this bone and that which succeeds it. The inner face of the bone is, as it were, cut short and replaced by this cartilage, whence the inferior edge is also short and is connected only with the basisphenoid, and not with the basioccipital. The anterior margin of the bone corresponds with the middle of the mesencephalon, while its inner face presents apertures for the portio dura and portio mollis. The posterior margin of its outer face forms half the circumference of the fenestra ovalis, and it contains the anterior and inferior portions

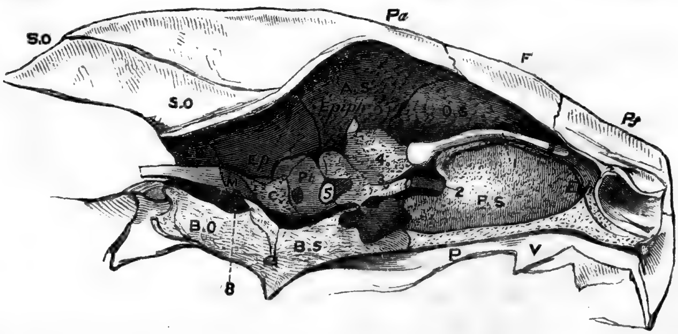


Fig. 5.—Longitudinal section of the Skull of a Turtle (*Chelone mydas*), exhibiting the relations of the brain to the cranial walls. The dotted parts marked AS. OS. PS. and Eth. are cartilaginous.

of the labyrinth. Thus, with the exception of the absence of an inferior connexion with the basioccipital,—a circumstance fully explained by the persistence in a cartilaginous state of part of the bone, —it corresponds in the closest manner with the petrosal of the bird. I confess I cannot comprehend how those who admit the homology of the bone called petrosal in the bird with that called petrosal in the mammal (as all anatomists do), can deny that the bone in ques-

tion is also the petrosal, and affirm it to be an alisphenoid. The general adoption of such a view would, I do not hesitate to say, throw the Theory of the Skull into a state of hopeless confusion, and render a consistent terminology impossible. Where then is the alisphenoid? I reply, that it is unossified. The posterior portion of the cartilaginous side-wall of the skull, in fact, unites with the parietal, the petrosal, and the basisphenoid, just in the same way as the bony alisphenoid of the bird unites with those bones. Furthermore, as in the bird, it bounds the foramen for the third division of the trigeminal nerve anteriorly, and is specially perforated by the second division of the fifth, while the optic and the other divisions of the fifth pass out in front of or through its anterior margin.

Not only is the alisphenoid cartilaginous, but the orbitosphenoid is in the same condition, and a great vertical plate of cartilage represents the whole anterior part of the craniofacial axis, or the presphenoid and ethmovomerine bones*. It has been imagined, indeed, that the rostrum-like termination of the basisphenoid represents the presphenoid, but I think this comes of studying dry skulls. Those who compare a section of the fresh skull of a turtle with the like section of the skull of a lamb, will hardly fail to admit that the rostrum of the basisphenoid in the turtle is exactly represented by that part of the sheep's basisphenoid, which forms the anterior and inferior boundary of the sella turcica, and that the suture between the basisphenoid and the presphenoid in the sheep corresponds precisely with the line of junction between the rostrum of the basisphenoid and the presphenoidal cartilage in the turtle.

Connected with the posterior edge of the petrosal by the cartilaginous plate, which has been referred to above, and between this and the exoccipital, there appears, on the inner aspect of the longitudinal section of the turtle's skull, a narrow plate of bone connected above, with the supraoccipital, behind, with the exoccipital, below, with the basioccipital, and leaving between its posterior margin and the exoccipital an aperture whereby the par vagum leaves the skull. In fact, except in being separated from the petrosal by cartilage, this bone presents all the characters of the mastoid of the bird, which it further resembles in forming one-half of the circumference of the

* Compare Kölliker's account of the primordial skull of a young turtle in the 'Bericht von der Königl. Zool. Anstalt zu Würzburg,' 1849.

fenestra ovalis. In other respects it is more like the mastoid of the sheep, for it is not anchylosed with the exoccipital; it is produced externally into a great bony apophysis, which gives attachment to the representative of the digastric muscle; and it is largely visible external to the exoccipital, when the skull is viewed from behind. Indeed, the resemblance to the mastoid of the mammal is more striking than that to the corresponding bone in the bird. And I think it is hardly possible for any unprejudiced person to rise from the comparison of the chelonian skull with that of the mammal, with any doubt on his mind as to the homology of the two bones.

When the sheep's skull is viewed from behind, the posterior half of the squamosal is seen entering into its outer boundary above the mastoid. On regarding the turtle's skull in the same way, there is seen, occupying the same position, the bone which Cuvier, as I venture to think, most unfortunately, named "mastoid." But if the arguments brought forward above be, as I believe with Hallmann, they are, irrefragable, this bone cannot be the mastoid; and I can discover no valid reason why it should not be regarded as what its position and relations naturally suggest it to be—the squamosal. Its connexions with the mastoid, petrosal, and quadratum are essentially the same as those of the squamosal in the bird and the mammal. The quadratum and articulare of the turtle are on all hands admitted to be the homologues of the similarly-named bones in the bird, and therefore all the reasonings which applied to the one apply to the other. When the petrosal, mastoid, and squamosal are determined in the turtle, they are determined in all the *Reptilia*. But the *Crocodilia*, *Lacertilia*, and *Ophidia* differ from the turtle and *Chelonia* generally, in that their mastoid is, as in the bird, anchylosed with the exoccipital. The squamosal, again, which in the *Crocodilia* essentially resembles that of the turtle, becomes a slender and elongated bone in the *Lacertilia*, and still more in the *Ophidia*, in which the quadratum is carried at its extremity *.

In the *Amphibia* the petrous and mastoid have the same relations as in the *Reptilia*; but it is interesting to remark, that in some *Amphibia* the anterior margins of the petrosal encroach upon the lateral

* See for the manner in which this is brought about, Rathke's 'Entwick. d. Natter.' Rathke, it should be said, regards this bone as the *tympanicum*, but its primitive place and mode of origin are those of the squamosal of the mammal.

walls of the skull so as completely to enclose the exit of the trigeminal, just as the posterior margin of the alisphenoid encroached so as to enclose it, in the sheep. It can be hardly necessary to remark, however, that this result has nothing to do with the disappearance of any element in the postero-lateral cranial walls, which have the same composition in the frog as in the crocodile or lizard.

The determination of the homologues of the squamosal, incudal, Meckelian, and tympanic elements in the amphibian skull is by no means an easy matter, but one requiring a much more careful investigation than it has yet received.

In *Mammalia*, a second arch, the hyoid, is connected with the outer surface of the skull, immediately behind the mandibular, and more particularly with that of the mastoid bone or its rudiment. The proximal end of this arch (which is, at first, like the mandibular arcade, a simple cartilaginous rod), in fact, usually becomes continuously ossified with the mastoid, forming part of the walls of the styloid canal; while below this, and external to the tympanum, it is converted into that slender bone, which is known as the styloid process.

In adult birds and most reptiles, the upper end of the hyoid arch is free, but in some *Reptilia** it is attached by a styloid process to the representative of the mastoid. Whether attached to the cranium or not, in all abranchiate *Vertebrata* the proximal end of the hyoidean arch is quite distinct from that of the mandibular arch.

In the *Amphibia*, however, I find a condition of the proximal ends of these two arches, which seems to foreshadow that intimate connexion between them which obtains in fishes. On the outer side of the petrosal, and of that part of the exoccipital which represents the mastoid, there lies a cartilaginous mass, which is continued downwards into a pedicle, with whose lower end the mandible is articulated. From the anterior edge of the proximal half of this pedicle, the narrow cartilaginous basis of the pterygoid passes forwards and upwards, to become directly continuous with the palatine bone in the frog, but to stop short of that point in the newt. Posteriorly, close to its proximal end, the pedicle becomes connected by a slender, fibrous or fibro-cartilaginous ligament with the upper ex-

* See Cuvier, 'Ossemens Fossiles,' x. p. 65; and Stannius, 'Zootomie der Amphibien,' p. 68.

tremity of the cornu of the hyoid. The hyoid and the mandibular arches are thus suspended to the skull by a common peduncle, which, to avoid all theoretical suggestion, I will simply term the "suspensorium."

The extent of the ossification which takes place, in and about this primitively cartilaginous suspensorium, varies greatly in different genera of *Amphibia*. Sometimes its distal end remains wholly unossified; sometimes, as in the common frog, a small outer portion of its lower extremity is ossified and sends a process forwards, becoming what is termed the quadratojugal bone; sometimes, as in the *Triton*, the distal half of the cartilage becomes more or less completely enclosed in a bony mass.

Another ossific deposit usually takes place in the outer half of the proximal end of the suspensorium, extending for a greater or less distance down towards the distal end, which it may even completely reach. It may be a simple triangular plate, as in *Triton*, or a T-shaped bone, as in *Rana*. In either case its lower end is the narrower, and fits into a kind of groove in the posterior and outer margin of the distal ossification.

This bone was considered by Cuvier to be the equivalent of the tympanic and the temporal (=squamosal); by Dugès it was called "temporomastoid."

The last constituent of this region of the skull in the *Amphibia* is one which is frequently overlooked altogether. In the frog, the membrana tympani is supported by a well-defined cartilaginous and partially ossified hoop, which is originally quite distinct from any of the elements of the suspensorium which have just been described, and which clearly deprives any of them of the right of being considered the homologue of the "tympanicum" of *Mammalia*.

I must defer the attempt to decide what the parts of the suspensorium really are, until the Piscine skull has been under consideration.

Composition of the Skull of the Carp.

The skulls of fishes present difficulties which necessitate, even for my present limited purpose, the entering into greater detail regarding them, than respecting those of the *Reptilia* or *Amphibia*. I select the cranium of the carp for description, as it departs far less widely from the common plan, and therefore forms a better type for com-

parison with the skulls of other *Vertebrata* than that of any acanthopterygian or gadoid fish.

The craniofacial axis presents only four distinguishable bones. Behind, is the short basioccipital, with its cup for articulation with the first vertebra of the spinal column. In front of this is a greatly elongated bone, which, as in the bird, sends a process as far as the vomer, and forms the greater part of the axis of the skull; and which, I believe, represents, as in the bird, the basisphenoid and more or less of the presphenoid. The short vomer terminates the craniofacial axis anteriorly, and bears upon its upper surface a vertical septum, which, as in the bird, expands into a broad plate above, and is the ethmoid.

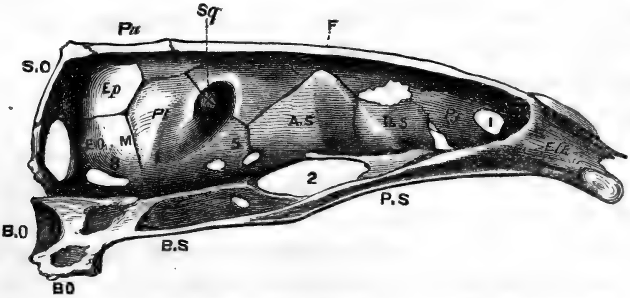


Fig. 6.—Longitudinal section of the Skull of a Carp (*Cyprinus carpio*).

The orbitosphenoids, united below, spring from the upper and anterior part of the presphenoid. Behind them the lateral walls of the skull are formed by the alisphenoid. These bones have the same essential relations as in the bird, for the olfactory nerves pass out of the skull over, and in front of, the orbito-sphenoids; the optic nerves make their exit behind and beneath these and the alisphenoid, while the trigeminal makes its exit behind the posterior edge of the alisphenoid. When viewed from within, the foramen ovale is seen to be as in the bird, a mere conjugational foramen between the alisphenoid and the bone which follows it; and on an external view, the third division of the trigeminal is seen to pass entirely in front of the last-named bone.

The minutest scrutiny of the relations of this bone only strengthens the conviction suggested by the first view of it, that it is the homologue of the petrosal of birds, and therefore of mammals and reptiles. As in the bird, the anterior margin of the fish's petrosal is

divided into a superior and an inferior portion, which meet at an angle, the superior portion articulating with the parietal (and squamosal), the inferior with the alisphenoid. Inferiorly, the petrous articulates with the basisphenoid, and, to a small extent, with the basioccipital. Posteriorly it articulates with a bone through which the pneumogastric passes, and which, guided by the analogy of most *Reptilia*, of *Amphibia*, and of birds, I believe to represent the coalesced or connate mastoid and exoccipital. The bone lodges the anterior part of the auditory labyrinth; its middle region corresponds with the middle of the mesencephalon. But as it does not separate the auditory organ from the cavity of the skull, it naturally presents no foramina corresponding with those through which the portio dura and portio mollis pass in Abranchiate *Vertebrata* and *Amphibia*. There is one relation of the petrosal in the fish, however, in which it seems to differ from that of any of the oviparous *Vertebrata* hitherto described. Superiorly and posteriorly, in fact, it does not unite with the supraoccipital, which is small, comparatively insignificant, and occupies the middle of the posterior and superior region of the skull; but with a large and distinct bone which forms the internal of the two posterolateral angles of the skull, unites internally with the supraoccipital, anteriorly with the parietal and petrosal, inferiorly with the conjoined mastoid and exoccipital. It is the bone which was called "occipital externe" by Cuvier; and he and others have supposed it to be the homologue of that bone in the turtle which, following Hallmann, I have endeavoured to prove to be the mastoid. As I have already shown, the true mastoid of the fish must be sought elsewhere, and consequently the Cuvierian determination is inadmissible. And I must confess, that if our comparisons be confined to adult *Vertebrata*, the only conclusion which can be arrived at seems to be, that this bone is peculiar to fishes.

But a remarkable and interesting observation of Rathke, combined with the peculiar structure of the skull of the chick described above, leads me to believe that when their development is fully worked out, we shall find a distinct representative of this bone in many, if not all, vertebrate crania.

In his account of the development of *Coluber natrix* (see Note IV.), Rathke states that three centres of ossification make their

appearance in that part of the cartilaginous wall of the cranium which immediately surrounds the auditory labyrinth. One of these is anterior, and becomes the petrosal; one is posterior, and eventually unites with the exoccipital; the third is superior, and in the end coalesces with the supraoccipital. The posterior ossification clearly represents the mastoid, and it is most interesting to find it, in this early condition, as distinct as in the Chelonian.

The superior ossification has only to increase in size and remain distinct in the same way as the mastoid of the turtle remains distinct, to occupy the precise position of the "occipital externe" of the fish. But, further, it is most important to remark, that when this primarily distinct bone has coalesced with the supraoccipital, it stands in just the same relation to that bone, to the petrosal, to the mastoid and to the semicircular canals, in the snake, as that lateral element, early confluent or connate with the supraoccipital in the chick, which I have termed the "os epioticum." I believe, then, that this "os epioticum," distinct in the young snake, but afterwards confluent with the supraoccipital, and becoming what may be termed the epiotic ala of that bone in the adult, is the homologue of the corresponding bone, or confluent ala of the supraoccipital, in birds and reptiles, while in the fish it remains distinct, and constitutes the "occipital externe."

For the rest, the superior part of the cranial arch in the carp resembles that of the bird. There are a supraoccipital, two parietals, and two frontals; the squamosal occupies the same position as in the chick, and as in the latter, is, in the dry skull, visible from within, in front of the petrosal.

As in the *Amphibia*, both the mandibular and the hyoidean arches are suspended by a pedicle or suspensorium, which is, to a certain extent, common to both, and presents a complexity of structure which can only be elucidated by the most careful study of development.

In ordinary fishes, such as the carp, stickleback, &c., the proximal end of the suspensorium is constituted by a single bone, Cuvier's "*temporal*," whose cranial end abuts against the squamosal, petrosal, and post-frontal bones.

This *temporal* * gives off posteriorly a process to which the

* In adopting the universally known Cuvierian appellations, I merely desire to avoid for the present all theoretical suggestions.

cornu of the hyoid arch is attached; anteriorly and distally it ends in an expanded plate, with which two bones are connected, in front the *tympanal,* behind the *symplectique.* The distal end of the suspensor is constituted by the triangular *jugal,* whose distal and narrower extremity furnishes the condyle with which the mandible is articulated.

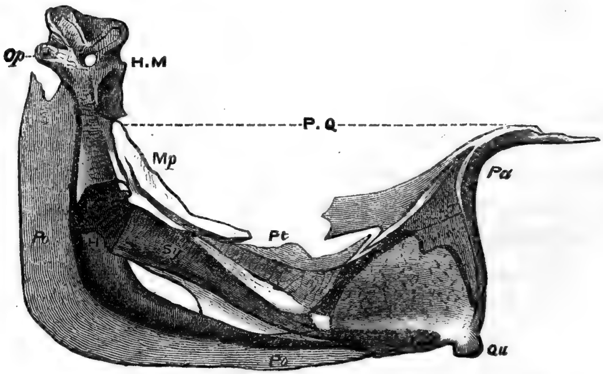


Fig. 7.—Palatosuspensorial arch of *Gasterosteus* from the inner side. HM. Hyomandibular bone. Op. Its articular facet for the operculum. Po. Pre-operculum. H. Articular surface for the styloid bone. Sy. Symplectic. P.Q. Palato-quadrate arch. Pa. Palatine bone. Qu. Quadratum. Pt. Pterygoid. Mp. Metapterygoid.

The elongated styliform *symplectique* is received into a groove on the posterior part of the inner surface of the *jugal,* and extends nearly to the condyle. In front, the jugal articulates with the *transverse,* and more or less with the *pterygoidien,* which again are anteriorly connected with the *palatine.* The flat *tympanal* is fitted in between the *pterygoidien, jugal,* and *temporal.*

Besides these numerous bones, there are four others which enter less directly into the composition of the suspensorium. These are the *pre-opercule,* a sort of splint-like bone which lies on the outer and posterior faces of the *temporal* and *jugal,* and binds the two together; the *opercule,* which articulates with a special condyle developed for it from the posterior edge of the *temporal,* above the attachment of the hyoid; the *sousopercule,* which lies in the opercular membrane beneath this; and lastly, the *interopercule,* the lowest of all, and commonly more or less closely connected with the angle of the lower jaw.

On examining the region in which these bones are eventually found, in an embryonic fish, I discovered, in their place, a delicate inverted cartilaginous arch, attached anteriorly, by a very slender pedicle, to the angles of the ethmoidal cartilage, and posteriorly connected by a much thicker crus with the anterior portion of that part of the cranial wall which encloses the auditory organ (fig. 8).

The crown of the inverted arch exhibits an articular condyle for the cartilaginous rudiment of the mandible. The posterior crus is not, as it appears at first, a single continuous mass, but is composed of two perfectly distinct pieces of cartilage applied together by their edges. The anterior of these juxtaposed pieces is continuous below with the condyle-bearing crown of the arch, and with its anterior crus or pedicle (P.Q.). It is inclined backwards and upwards, and terminates close to the base of the skull in a free pointed extremity.

The posterior piece (S.Y. H.M.), on the other hand, has its broad and narrow ends turned in the opposite direction. Distally, or below, it is a slender cylindrical rod terminating in a rounded free extremity behind, but close to, the condyle for the mandible; above, it gradually widens and becomes connected with the cranial walls. On its posterior edge there is a convexity which articulates with the rudimentary operculum, and below this it gives off a short styloid process, to which the cartilaginous cornu of the hyoid is articulated. Thus the cartilaginous arch, which stretches from the auditory capsule to the ethmo-presphenoidal cartilage, consists, in reality, of two perfectly distinct and separate portions—the anterior division V-shaped, having its anterior crus fixed and its posterior crus free above; the posterior, styliform, parallel with the posterior leg of the V and free below. The anterior division supports the mandibular cartilage, the posterior the hyoidean cornu.

As ossification takes place, that part of the anterior crus of the V-shaped cartilage which is attached to the ethmo-presphenoidal cartilage becomes the *palatine*; its angle becomes the *jugal*; between these two the *transverse* and *pterygoidien* (represented by only one bone in *Gasterosteus*) are developed in and around the anterior crus: the *tympanal* arises in the same way around the free end of the posterior crus. Thus these bones constitute an assemblage which is at first quite distinct from the other elements of the suspensorium, and immediately supports the mandibular cartilage.

The proximal end (H.M.) of the posterior styliform division gradually becomes articulated with the cranial walls, and, ossifying, is converted into the *temporal*. The distal cylindrical end (S.Y.) becomes surrounded by an osseous sheath, which at first leaves its distal end unenclosed. The bone thus formed is the *symplectique*, which is at first free, but eventually becomes enclosed within a sheath furnished to it by the *jugal*, and so strengthens the union of the two divisions of the arch already established by the junction of the *tympanal* with the *temporal*. The *symplectique* and *temporal* do not meet, but leave between them a cartilaginous space, whence the supporting pedicle of the hyoid, which ossifies and becomes the *osselet styloide*, arises.

The operculum, suboperculum, interoperculum, and preoper-

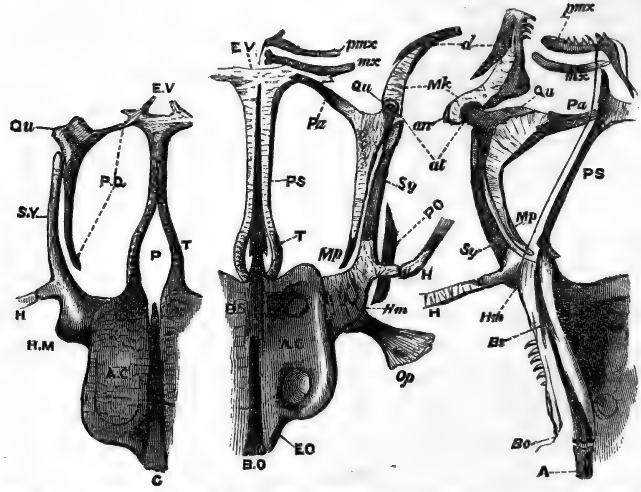


Fig. 8.—Cranium and face of young *Gasterostei* at different ages. The left-hand figure is a view of the base of the skull of a very young fish. The middle figure represents the under aspect, and the right-hand figure, a side view of a longitudinal section, of a more advanced stickleback's skull.

C. Notochord. P. Pituitary space. AC. Auditory capsules. T. Trabeculæ cranii. E.V. Ethmovomerine cartilage. P.Q. Palatoquadrate arch. Qu. Quadratum. S.Y. or Sy. Symplectic. H. Hyoidean arc. H.M. Hyomandibular cartilage. The other letters have the same signification as in the preceding figures, except *pmx*. Premaxilla. *mx*. Maxilla. *d*. Dentale. *an*. Angulare. *at*. Articulare. M*k*. Meckel's cartilage.

culum are not developed from the primitive cartilaginous arch, but make their appearance as osseous deposits in the branchiostegal membrane, behind, and on the outer side of, the posterior crus.

If we turn to the higher *Vertebrata*, we find, as I have stated above, that, at an early period of their embryonic existence, they also present a cartilaginous arch, stretching from the ethmo-presphenoidal cartilage to the auditory capsule, and supporting the mandibular or Meckelian cartilage on the condyle furnished by its inverted crown. The anterior part of the anterior crus of this arch becomes the palatine bone, which is therefore truly the homologue of the fishes' *palatine*. The posterior part of it becomes the pterygoid, which therefore is the homologue of the *pterygoidien* (and *transverse* ?) of the fish.

The produced crown of the arch in the higher *Vertebrata* becomes either the incus, or its equivalent, the quadratum. I therefore entertain no doubt that the *jugal* is really the homologue of the quadratum of other oviparous *Vertebrata*. That the *tympanal* has no relation whatsoever with the bone of the same name in the higher *Vertebrata* is indubitable; and I am unable to discover among them any representative of it. It seems to me to be an essentially piscine bone, to be regarded either as a dismemberment of the quadratum or of the pterygoid. It may be termed the "metapterygoid."

Still less do I find among the higher *Vertebrata* in their adult state, any representative of the posterior division of the suspensor, constituted by the *temporal* and *symplectique*. It is quite clear, that the *temporal* is not, as Cuvier's name would indicate, the homologue of the squamosal. The whole course of its development would negative such an idea, even if we had not a squamosal already; and I shall therefore henceforward term it, from its function of affording support to both the hyoid and mandibular arches, the hyomandibular bone, "os hyomandibulare," while the other bone of this division may well retain the name of symplectic.

It is commonly supposed that the hyomandibular, symplectic, metapterygoid, and quadrate are all to be regarded as mere subdivisions of the quadratum of higher *Vertebrata*. Such a view, however, completely ignores and fails to explain, the connexion of the hyoidean arch with the hyomandibular bone. In no one of the higher *Vertebrata* does such a connexion ever obtain between any part of the quadratum and the hyoid, which are quite distinct, and attached separately to the walls of the cranium, in even young embryos of the abranchiate *Vertebrata*.

Nevertheless, in their very earliest conditions, these embryos are said to present a structure, which, if I mistake not, shadows forth the organization of the fish. The visceral arches, in which the mandibular and hyoid cartilages are developed, are at first separated to the very base of the cranium by a deep cleft, the anterior visceral cleft, so that the semi-cartilaginous rudiments of the mandibular and hyoid are completely separate. Subsequently they are said to coalesce above, as the visceral cleft diminishes, so as to have a common root of attachment to the cranium; and this, I apprehend, answers to the hyomandibular bone, and its prolongation to the symplectic. With advancing development, however, this part does not advance, but remains stationary, and becomes confounded with the wall of the cranium; so that the two arches subsequently appear to be attached to the latter quite independently, and there is nothing left to represent this division of the suspensorium in fishes.

I am strengthened in this view by the structure and development of the palatosuspensorial apparatus in the *Amphibia,* whose consideration I deferred when speaking of the skull in that class. On examining a young tadpole (fig. 9), a cartilaginous process is seen to arise from the walls of the cranium, opposite the anterior part of the auditory capsule, and, passing obliquely downwards and forwards, to end in a rounded condyloid head, which articulates with the representative of Meckel's cartilage. At the anterior boundary of the orbit the process gives off a broad, nearly vertical apophysis (O), which ends superiorly in a free, rounded, and incurved edge. The crotaphite muscle passes to its insertion on the inner side of this, the so-called "orbitar process." From the condyle the cartilaginous process sweeps upwards and inwards, and ends by passing into the ethmo-presphenoidal cartilage. It consequently forms an inverted arch, whose keystone is the condyle for Meckel's cartilage, and is, in its connexions and form, strictly comparable with the cartilaginous arch which I have described in the embryo fish. The posterior crus of the arch, it is true, is not divided into two parts, but nevertheless it represents the whole suspensorium of the fish, and not merely the quadratum of the abranchiate vertebrate, because immediately behind the orbitar process it presents an excavated surface, which articulates with the proximal end of the cornu of the hyoid. That part of the cartilaginous arch, therefore, which lies above and behind

this point, corresponds with the proximal division of the suspensorium in the fish, or with the hyomandibular bone; while that portion which lies below and in front of it, corresponds with the distal division of the suspensorium and the anterior crus of the arch in the fish, or in other words, with the symplectic, quadratum, metapterygoid, pterygoid, transverse, and palatine bones.

In the course of development, in fact, the palatine bone appears, as in the fish, in that part of the arch which is immediately connected with the ethmo-presphenoidal cartilage, and a single pterygoid in that part of its anterior crus which lies between the palatine and the articular portion, which obviously represents the quadratum. But this pterygoid is, in the adult frog, a large bone, which, on the one hand, stretches down on the inner side of the quadrate cartilage, and, on the other, sends a process inwards and upwards, which nearly reaches the base of the skull. If the pterygoid, transverse, and metapterygoid of the fish were anchylosed into one bone, or if the corresponding region of the primitive cartilage were continuously ossified, the result would be a bone perfectly similar to the pterygoid of the frog; and I entertain no doubt that the amphibian pterygoid does really represent these bones.

The inferior ossification in the batrachian suspensorium certainly answers to the quadratum, in *Triton*—whether it should be regarded partly or wholly as a quadrato-jugale in the frog seems to be a question of no great moment—inasmuch as we may be quite sure that the lower end of the frog's suspensorium represents the quadrate or incudal element in other *Vertebrata*.

It is well known that, in the course of the development of the frog, the end of the suspensorium, as it were, travels backwards, so that its axis, instead of forming an acute angle, open forwards, with that of the cranium, as in the tadpole (fig. 9), forms a very obtuse angle, open downwards, in the adult frog. This change is accompanied by a relative and absolute lengthening of that part of the suspensorium which lies between the articulation of the hyoid and that of Meckel's cartilage (containing its proper quadrate portion), and by a relative shortening of that part which lies between the articulation of the hyoid and the skull (or the hyomandibular portion). The consequence of this is, that the articular surface for the hyoid appears constantly to approach the cranial wall, until at length, in the adult,

it seems to be almost in contact with it. If a knife were passed obliquely between the pterygoid and the suspensorium, and then carried through the suspensorium to its posterior margin a little

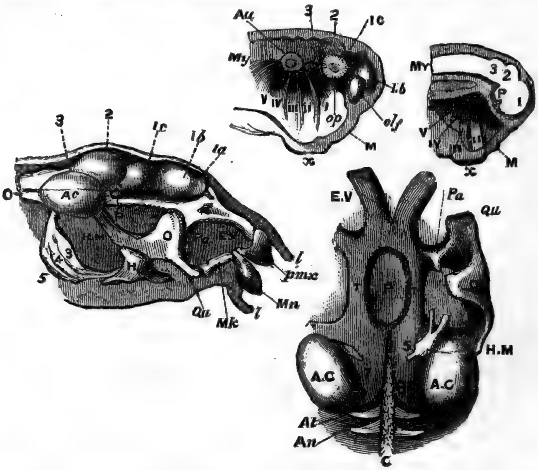


Fig. 9.—The upper right-hand figure represents a longitudinal section of the head of a tadpole just about to be hatched. The upper left-hand figure exhibits a dissection of the head of a tadpole with external gills. The two lower figures represent dissections of the crania of tadpoles with well-developed hinder limbs. In the one, the integuments, organs of sense, &c. of the right side are taken away so as to lay bare the facial cartilages and the brain. In the other the cranium is opened from above, and the brain and myelon are extracted.

The letters have the same signification as before, except My. Myelon. M. Mouth. *olf.* Olfactory sac. *op.* Eye. 1. Anterior cerebral vesicle. 2. Middle cerebral vesicle. 3. Posterior cerebral vesicle. 1*a*. Rhinencephalon. 1*b*. Prosencephalon. 1*c*. Deutencephalon, or vesicle of the third ventricle. I. II. III. IV. V. Branchial arches. *x*. Organs of adhesion. l. Lips. 5. Trigeminal ganglion. 7. Ganglion of the portio dura. 8. Aperture for the exit of the pneumogastric.

above the condyle for the mandible, it would divide the suspensor into a proximal and a distal portion, precisely resembling those which naturally exist in the embryonic fish. If the proximal division ossified, it would clearly represent the hyomandibular and symplectic bones. Now in the *Amphibia*, although the suspensor is not thus divided, it ossifies very nearly as if it were, and the superior or proximal ossification is the so-called "temporo-tympanic," "temporo-mastoid," or "squamosal" bone*.

* See this result, well worked out, by the method of gradation only, by Köstlin (*l. c.* pp. 328–332), who draws particular attention to the resemblance between the suspensorium of the *Amphibia* and that of fishes of the Eel-tribe.

That this bone is really the homologue of the hyomandibular and symplectic in the fish, becomes, I think, still more clear when we compare it with such an aberrant form of piscine suspensorium as is presented by some of the eel-tribe (*Muræna, e.g.*). In these fishes the suspensorium is formed by only two bones, a small distal quadratum, which, as usual, articulates with the lower jaw, and a large wide proximal bone, which articulates above with the post-frontal and squamosal, gives attachment to the operculum and to the cornu of the hyoid, and sends down a process towards the articular head of the quadratum. The single bone, which represents the three pterygoids of other fishes, is articulated for the most part with the quadratum, but partly with this proximal bone. The latter, therefore, clearly represents both the hyomandibular and the symplectic bones of ordinary fishes.

But if the suspensorium of *Triton* be compared with that of *Muræna, e. g.*, it will, I think, be hardly doubted, that while the distal ossification in the former corresponds with the quadratum, the proximal answers (at any rate, chiefly) to the hyomandibular bone of the *Muræna*. Indeed it differs from the latter principally in being an ossific deposit in the outer portion only of the primitive cartilage*.

Thus it would seem, that in the manner in which the lower jaw is connected with the cranium, *Pisces* and *Amphibia*, as in so many other particulars, agree with one another, and differ from *Reptilia* and *Aves* on the one hand, as much as they do from *Mammalia* on the other. And the difference consists mainly, as might be anticipated, in the large development in the branchiate *Vertebrata* of a structure which aborts in the abranchiate classes. A most interesting series of modifications, all tending to approximate the ramus of the mandible more closely to the skull†, is observable as we pass from the fish to the mammal. In the first, the two are separated by the hyomandibular, the quadrate, and the articular elements, the first of which

* In *Muræna Helena* the suspensorium forms an obtuse angle with the axis of the skull, though not so obtuse as in the frog. A strong ligament connects the outer side of the distal end of the quadratum with the maxillary bone, passing outside the lower jaw. If the posterior end of the ligament were ossified, it would correspond very nearly with the "quadratojugal" of the frog.

† Of course in a morphological sense. Whether they are more or less distant in actual space, is not the question.

becomes shortened in the *Amphibia*. In the oviparous abranchiate *Vertebrata* the cranium and the ramus are separated only by the quadratum and the articulare, the hyomandibulare having disappeared. Finally, in the mammal, the quadratum and the articulare are applied to new functions, and the ramus comes into direct contact with the cranium.

The operculum, suboperculum, and interoperculum appear to me to be specially piscine structures, having no unquestionable representatives in the higher *Vertebrata*. Much might be said in favour of the identification of the preoperculum with the tympanic bone; but there are many arguments on the other side, and at present I do not see my way to the formation of a definite conclusion on this subject.

In the preceding discussion of the structure of the osseous vertebrate skull, I have desired to direct your attention, more particularly, to the consideration of those fundamental bones, the determination of whose homologues throughout the vertebrate series is of the greatest importance for my present object. The presphenoid, ethmoid, mastoid, and petrosal are the Malakhoff and the Redan of the theory of the skull; and if anatomists were once agreed about their homologues, there would be comparatively little left to dispute about.

But besides the axial, inferolateral, and superior series of bones, there are other, less constant, elements of the cranial wall, forming a discontinuous superolateral series. These are the epiotic, the squamosal, the postfrontal, the prefrontal, and lacrymal bones. Of the two first-named of these bones I have already spoken sufficiently. The postfrontal exists only in Reptiles and Fishes, and is always situated between the frontal, alisphenoid, petrosal, and squamosal—the extent to which it is absolutely in contact with any one of these bones varying.

The prefrontal and lacrymal bones are always developed in or upon that lateral process of the ethmosphenoidal plate, which gives attachment externally to the palatopterygoid arch; consequently they lie at the anterolateral ends of the frontal, and have more or less close relations with it, the ethmoid and the palatine bones.

Finally, the nasal bones (or bone) never enter into the composition

of the walls of the skull, but have the same relation to the anterior and upper expanded edge of the prolonged lamina perpendicularis or body of the ethmoid, as the vomer or vomers have to its lower edge.

If the conclusions which I have laid before you are correct, the following propositions are true of all the bony skulls of *Vertebrata.*

1. Their axis contains at most five distinct bones, which are, from before backwards, the basioccipital, the basisphenoid, the presphenoid, the ethmoid, and the vomer; but any of these bones, except the basisphenoid, may be represented by cartilage, and they may anchylose to an indefinite extent; so that the number distinguishable as separate bones in any skull cannot be predicated. The craniofacial axis invariably presents the same regions, but the histological character of these regions may vary.

2. Their roof contains at most, leaving Wormian bones out of consideration, five bones (supraoccipital, parietals and frontals), or seven, if we include the epiotic bones in the roof. The number falls below this in particular cases, for the same reason as that given for the apparent variations in composition of the axis.

3. Their inferolateral wall contains at most six pair of bones (exoccipitals, mastoids, petrosals, alisphenoids, orbitosphenoids, prefrontals), whose apparent number, however, is affected by the same causes.

4. The axial bones have definite relations to the brain and nerves. The basioccipital lies behind the pituitary body, the basisphenoid beneath it, the presphenoid in front of it. In fact the pituitary body may be regarded as marking the organic centre, as it were, of the skull—its relations to the axial cranial bones being the same, as far as I am aware, in all *Vertebrata.*

The olfactory nerves pass on either side of the ethmoid, which bounds the cranial cavity in front, the greater part of its substance and that of the vomer being outside the cranial cavity.

5. The lateral bones have definite relations to the brain, nerves, and organs of sense. The exoccipital lies behind the exit of the par vagum; the mastoid lies in front of it; the petrosal lies behind the exit of the third division of the trigeminal; the alisphenoid lies in front of it; though either bone may, to a certain slight extent, en-

croach on the province of the other. The optic nerve passes out more or less in front of the alisphenoid, and behind, or through, the orbitosphenoid.

The organ of hearing is always bounded in front by the petrosal bone, which limits the anterior moiety of the fenestra ovalis.

The organs of smell always lie on each side of the ethmovomerine part of the axis.

The greater part, or the whole, of the petrosal lies behind the centre of the mesencephalon.

6. The attachment of the mandibular arch to the skull is never situated further forward than the posterior boundary of the exit of the trigeminal; consequently it cannot belong to any segment of the skull in front of the petrosal.

But if propositions of this generality can be enunciated with regard to all bony vertebrate skulls, it is needless to seek for further evidence of their unity of plan. These propositions are the expression of that plan, and might, if one so pleased, be thrown into a diagrammatic form. There is no harm in calling such a convenient diagram the 'Archetype' of the skull, but I prefer to avoid a word whose connotation is so fundamentally opposed to the spirit of modern science.

Admitting, however, that a general unity of plan pervades the organization of the ossified skull, the important fact remains, that many vertebrated animals—all those fishes, in fact, which are known as *Elasmobranchii*, *Marsipobranchii*, *Pharyngobranchii*, and *Dipnoi*—have no bony skull at all, at least in the sense in which the words have hitherto been used. In these *Vertebrata* the skull is either membranous or cartilaginous; or if ossified, the ossific matter presents no regular grouping around a few distinct centres.

Thus the cranium of the *Amphioxus* is nothing but a membranous capsule, whose walls are continuous with those of the canal for the spinal cord, and in whose floor lies a continuation of the notochord which underlies the spinal canal.

In the *Marsipobranchii* there is a marked increase in the capacity of the cranium as compared with that of the spinal canal, in correspondence with the decided differentiation of the cerebral masses; and, at the same time, the cranial walls have undergone a more or less extensive chondrification. The notochord terminates in the midst of the firm and solid cartilaginous plate which forms the posterior part

of the basis cranii, and which sends forward two processes, including a membranous interspace. The auditory capsules are enclosed within prolongations of the sides of the basilar plate; and just in front of and below them, the root of each process of the basal plate gives off a solid prolongation, which passes at first outwards and downwards, and then bends upwards and forwards, to rejoin the anterior part of the process of the basilar plate of its side. An inverted arch is thus formed, and the space included between its crura and the sides of the cranium, constitutes the floor of the orbit.

The posterior crus of the arch is divided into two, more or less distinct, pillars, the posterior of which supports the hyoidean arc; the mandibular arc appears to be absent.

The apertures whereby the cranial nerves make their exit are situated in the side-walls of the capsule, that for the vagus lying immediately behind the auditory capsule, while that for the trigeminal is immediately in front of the same organ. The olfactory nerves perforate the anterior walls of the cranial capsule; the optic, its lateral walls between them and the trigeminal.

The skulls of the *Elasmobranchii*, again, appear at first to be something quite different from either of these. The cranium is here a cartilaginous box, more or less incomplete and membranous above, and presenting on each side posteriorly a transverse enlargement, in which the auditory organ is contained; while anteriorly it expands into a broad plate, which on each side overhangs the olfactory sacs. The notochord and the membranous space have disappeared, or their traces only are visible in the base of the cranium, whose walls are, as it were, crusted with a multitude of minute plates of bone.

In the *Chimæræ* the inferolateral walls of the cranium pass into a cartilaginous arch-like plate which form the floor of the orbit, and whose posterior part, as in the *Marsipobranchii*, gives attachment to the hyoidean arch; besides which, a mandibular cartilage is connected with the condyloid surface developed from the crown of the arched plate.

In the Plagiostomes there is also an inverted suborbitar arch with a mandibular cartilage and a hyoidean apparatus, but the structure of the arch is different from what obtains in *Chimæra*.

The outer wall of that portion of the cranium which lodges the

auditory organ, in fact, furnishes an articular surface for a strong moveable peduncle, to which the hyoid arc is usually attached. At its lower end, however, this peduncle does not articulate with the mandibular cartilage, but is directly connected with a strong cartilaginous plate which forms the upper boundary of the gape, and is articulated anteriorly with the sides of the skull in front of the orbit. This plate bears the upper series of teeth, and bites more or less directly against the mandible, which is moveably articulated with a condyle furnished by its posterior extremity.

The upper plate is commonly, though, as I think, erroneously, regarded as the homologue of the maxilla and premaxilla in other fishes; the peduncle as the homologue of their whole suspensorium*.

The par vagum leaves the skull behind the auditory organ; the trigeminal passes out in front of it; and then its third division traverses the space enclosed between the peduncle, the upper plate, and the skull. The optic nerve passes through the lateral walls of the skull in front of the trigeminal, and the olfactory perforates its anterior boundary.

So brief and simple a statement of the characters of the skulls of these three orders of fishes, while it brings their diversities into prominence, also exhibits an amount of uniformity among them which is not a little remarkable. The exits of the great nerves have fixed relations to the auditory capsules, to the anterior boundary of the skull, and to the pituitary body. The inferior arc of the hyoid is constant (except in the *Pharyngobranchii*), and has always, speaking broadly, the same relative position with respect to the auditory capsule and the posterior crus of the suborbitar arch. The suborbitar arch itself is always present (except in *Pharyngobranchii*); its posterior crus is always attached to the cranium behind the third division of the trigeminal nerve, while the anterior is invariably fixed to that part of the skull which lies behind, or beside, the base of the olfactory capsule.

Thus the employment of the method of gradation alone exhibits a surprising uniformity in the organization of these lower forms of skull; and on comparing them with the higher forms, it seems obvious that, so far as it goes, their plan is identical with that of the latter; for the relations of the auditory organ to the par vagum and trige-

* See Note IV., on the suspensorium in fishes.

minal are the same in each; the posterior crus of the suborbitar arch answers to the suspensorium of *Teleostei*, its anterior crus to their palatopterygoid apparatus. But with all this, there are discrepancies in the structure of the skull itself, which would forbid too close an approximation between the bony and the unossified crania, if their adult forms alone were examined. The study of the development of the ossified vertebrate skull, however, eliminates this difficulty, and satisfactorily proves that the adult crania of the lower *Vertebrata* are but special developments of conditions through which the embryonic crania of the highest members of the subkingdom pass.

It is to Rathke's luminous researches that we are indebted for the first, and indeed, even now, almost the only, demonstrative evidence of this great fact. Twenty years ago that great and laborious embryologist worked out the early stages of the development of the skull in each class of the *Vertebrata*. Confirmed and adopted by Vogt and Bischoff, his conclusions have been feebly controverted, but never confuted; and my own observations lead me to believe that they are destined to take a permanent place among the data of biological science. Nothing is easier than to verify Rathke's views in an embryonic fish or amphibian; and as it matters not which of the higher *Vertebrata* is selected for the study of cranial development, I will state at some length what I have observed in the embryonic frog.*

Before the dorsal laminæ have united so as to enclose the primitive craniospinal cavity, the anterior portion of the floor of that cavity is bent downwards. The angle which the deflexed portion forms with the rest becomes less and less obtuse, until, when the dorsal laminæ have united and the visceral clefts have begun to appear, it constitutes a right angle.

On examining the floor of the craniospinal cavity at this period, it is seen that the notochord, at present formed by the aggregation of a number of yelk segments or embryo-cells, small in themselves, but larger than those of which the rest of the body is composed, ends in a point immediately behind the angular flexure.

The notochord has no sheath as yet, and is not in any sense prolonged into the deflexed portion of the floor of the craniospinal cavity.

When the visceral clefts first appear, they are best seen from the

* See Note V. for the development of the skull in other *Vertebrata*.

inner or pharyngeal aspect of the visceral wall. Five, of which the two anterior are the longest and about equal, while the others gradually diminish in length from before backwards, can be distinctly observed. They mark out the boundaries of a corresponding number of "visceral arches," and there is sometimes an appearance as of a sixth visceral arch behind the last cleft. A horizontal section shows that these arches differ in nothing but their relative size—in no other respect can one of them be distinguished from the other.

The anterior visceral cleft lies in a transverse plane, immediately behind the angular bend of the floor of the craniospinal cavity, or, as I shall henceforward term it, mesocephalic flexure. Consequently the posterior part of the first visceral arch passes into the future basis cranii close to the flexure.

The parts of the cerebrum are now distinguishable. It is bent in correspondence with the mesocephalic flexure, and its most projecting portion, or the angle of the bend, is the rudiment of the mesencephalon. The large rudiment of the pituitary body lies immediately in front of the flexure, and is therefore altogether anterior to the end of the notochord and to the posterior part of the first visceral arch. The rudiment of the eye lies at first altogether in front of the flexure, and therefore anterior to the root of the first visceral arch.

The auditory vesicles make their appearance on each side of a line which would cut the chorda a little behind its anterior termination. They are at first quite free and perfectly distinct from the walls of the cranium, which is in accordance with Remak's statement, that they are originally formed by the involution of the epidermic layer of the embryo. They long remain separate and easily detachable from the cranial walls.

Ten days after impregnation, larvæ with rudimentary external gills and colourless blood, still exhibited some traces of the mesocephalic flexure, but the angle formed by the anterior and posterior portions of the cranium was very obtuse; the base of the cranium had, in fact, undergone a gradual straightening. The rudiments of the cranial skeleton had made their appearance, and consisted, behind the mesocephalic angle, of a broad semi-cartilaginous plate enclosing the anterior end of the notochord, but not covering it above or below. It is not as yet adherent to the auditory sacs.

That part of the middle of the basis cranii which underlies the pituitary body is not converted into cartilage, but remains membranous, and may be called the "subpituitary membrane." The delicacy of this membrane is so great that it is easily torn, when the pituitary body seems, as Rathke originally supposed, to unite with the palatine mucous membrane. But that this is not really the case, is readily demonstrable in an embryo whose tissues have been sufficiently hardened with alcohol or nitric acid.

The cartilaginous basal plate gives off a prolongation on either side of the subpituitary membrane. This, the "cranial trabecula" (Schädelbalke of Rathke), passes forwards with a slight convexity outwards, and then turning inwards comes into contact with its fellow (from which, however, it is at first distinct), and spreads out into a broad, flat, elongated process, which I shall term the ethmo-vomerine cartilage.

Behind the eye and just in front of the auditory capsule (in the posterior part of the first visceral arch, therefore), a cartilaginous process lies, which is connected proximally with the root of the trabecula close to the basal plate, while at its distal end it sends a prolongation upwards to unite with the posterior end of the ethmo-vomerine cartilage. It then forms an arch, between which and the basis cranii is an interspace corresponding with, and lodging, the under surface of the large eyeball. The rudiments of the hyoid, mandibular and maxillary apparatus in larvæ at this stage are somewhat indistinct; and indeed not only in this, but in other respects, more instruction is to be derived from tadpoles which have advanced further.

In larvæ, with completely internal branchiæ and very short tubercles in the place of hind limbs, the notochord suddenly narrows between the auditory capsules to hardly more than half its preceding dimensions, and then gradually tapers off, to what appears to be a rounded end, a short distance from the anterior boundary of the basal plate. On very careful examination, however, a delicate process (which may by possibility be nothing but a cavity in the cartilage) can be traced from it very nearly to the margin of the basal plate. But there is no continuation whatsoever, either of the notochord itself or of its sheath, into the subpituitary membrane, which is now composed of delicate connective tissue, and from its extreme thinness and transparency would exhibit the least trace of such a

prolongation. And I speak the more confidently on this point, because the delicate process of the notochord or cavity in the cartilage, to which I have referred, contains opaque unchanged vitelline granules, and is therefore particularly conspicuous. The basal cartilage is still divided by the notochord into two lateral moieties, which are only united by a short band of cartilage in front of the end of the notochord. It sends off from its outer side a cartilaginous process, which envelopes the auditory capsule externally, but leaves on its inner side a wide aperture for the entrance of the auditory nerve. The oval auditory capsules thus formed have their long axes directed outwards and forwards.

The trabeculæ are still better developed than before, but instead of remaining distinct anteriorly, they have become fused together into a single trapezoidal cartilage, which may be termed the ethmo-presphenoidal plate. This plate, as it were, divides anteriorly into two flat, elongated and somewhat divergent processes, which are concave downwards and end in truncate extremities. Fibrous tissue connects the ends of these ethmovomerine processes with a crescentic cartilaginous plate which supports the horny upper jaw of the tadpole.

The posterior crus of the palatosuspensorial, or suborbitar, arch is not yet united with that portion of the cranial wall which encloses the auditory capsule; but fort he rest the same description applies to it which has already been given of the palatosuspensorial arch and its appendages in more advanced tadpoles. In this state, the roof, and all the lateral walls of the cranium, but that part into which the auditory capsule enters, are membranous.

If the skull of the larval frog just described, be laid open and the exit of the nerves observed (fig. 9), it will be seen that the par vagum makes its way out by a foramen situated immediately behind the auditory capsule; that the third division of the trigeminal leaves the cranium in front of the auditory capsule, passing over the posterior crus of the palatosuspensorial arch; and that the optic traverses the membranous walls of the skull between this and the olfactory nerve, which perforates the anterolateral region to enter the olfactory capsules. The latter are situated wide apart, on each side and in front of, the broad ethmo-presphenoidal cartilage and the anterior crus of the palatosuspensorial arch, and are even a little overlapped by the edges of the ethmovomerine processes.

In the further course of development, the trabeculæ approximate and elongate, so as to obliterate the subpituitary membrane, and form with the enlarged basal cartilage, the ethmoid cartilage and the ethmovomerine cartilages, the continuous cartilaginous craniofacial axis. A histological metamorphosis into cartilage is undergone by the roof of the occipital region of the skull, but in front of this it remains membranous; so that in the adult frog (in which this cartilaginous framework persists), the skull, when deprived of its bony matter, presents an anterior fontanelle. The ethmovomerine cartilages diverge still more, and form the broad mass whose lateral cavities shelter the olfactory sacs in the adult frog. If, bearing in mind the changes which are undergone by the palatosuspensorial apparatus, and which have already been described, we now compare the stages of development of the frog's skull with the persistent conditions of the skull in the *Amphioxus,* the Lamprey, and the Shark, we shall discover the model and type of the latter in the former. The skull of the *Amphioxus* presents a modification of that plan which is exhibited by the frog's skull, when its walls are still membranous and the notochord is not as yet imbedded in cartilage. The skull of the lamprey is readily reducible to the same plan of structure as that which is exhibited by the tadpole, while its gills are still external and its blood colourless. And finally, the skull of the shark is at once intelligible when we have studied the cranium in further advanced larvæ, or its cartilaginous basis in the adult frog.

Thus, I conceive, the study of the mode in which the skulls of vertebrate animals are developed, demonstrates the great truth which is foreshadowed by a careful and comprehensive examination of the gradations of form which they present in their adult state; namely, that they are all constructed upon one plan; that they differ, indeed, in the extent to which this plan is modified, but that all these modifications are foreshadowed in the series of conditions through which the skull of any one of the higher *Vertebrata* passes.

But if these conclusions be correct, the first problem which I proposed to you,—Are all vertebrate skulls constructed upon a common plan?—is solved affirmatively.

We have thus attained to a theory or general expression of the laws of structure of the skull. All vertebrate skulls are originally alike; in all (save *Amphioxus*?) the base of the primitive cranium

undergoes the mesocephalic flexure, behind which the notochord terminates, while immediately in front of it, the pituitary body is developed; in all, the cartilaginous cranium has primarily the same structure,—a basal plate enveloping the end of the notochord and sending forth three processes, of which one is short and median, while the other two, the lateral trabeculæ, pass on each side of the space, on which the pituitary body rests, and unite in front of it; in all, the mandibular arch is primarily attached behind the level of the pituitary space, and the auditory capsules are enveloped by a cartilaginous mass, continuous with the basal plate between them. The amount of further development to which the primary skull may attain varies, and no distinct ossifications at all may take place in it; but when such ossification does occur, the same bones are developed in similar relations to the primitive cartilaginous skull. But the theory of the skull thus enunciated is not a 'vertebral theory'; one may have a perfectly clear notion of the unity of organization of all skulls without thinking of vertebræ.

So much for the first problem before us. I now proceed to the second question, which was, you will recollect, Given the existence of a common plan of organization of all vertebrate skulls; is this plan the same as that of a spinal column?

To deal properly with this question, we must know what is the plan of organization of a spinal column, and that can be learnt only by a careful study of its development, as well as of its adult modifications. Indeed, the latter are unintelligible without a knowledge of the former.

It is impossible to form a clear conception of the essential nature of the process of development of a spinal column, or to compare it with that of the skull, unless we analyse very carefully, and distinguish from one another, the successive steps of that process*.

1. The primary changes of form exhibited by the blastoderm in the region of the spinal column, are, in all the *Vertebrata* whose development has yet been studied, precisely the same. Two ridges, the "laminæ dorsales," bounding a narrow elongated groove, rise up and eventually unite with one another so as to enclose a cavity—the neural canal. External to the junction of the laminæ dorsales

* See Note VI. for the details of the development of the spinal column in *Vertebrata* generally.

with the blastoderm, the latter is converted more or less completely into the "laminæ ventrales," which become incurved, unite, and eventually enclose the visceral cavity.

A transverse section of the embryo in this state shows a very thin and narrow median plate, separating the neural canal above, from the hæmal or visceral canal below, and passing on each side into thickened masses of blastoderm, which give rise to the laminæ dorsales on the one hand, and to the laminæ ventrales on the other.

For convenience of description, I shall term the median plate the "diaphysial plate," and the lateral ridges the "paraphysial thickenings."

2. The primary histological differentiations, which take place in the rudimentary spinal column just described, are the same in all *Vertebrata.*

A long filament, composed of indifferent tissue, makes its appearance in the middle of the diaphysial plate, and constitutes the notochord, or chorda dorsalis.

Next, the substance of the paraphysial thickenings undergoes a certain change of tissue at regular intervals, so that they acquire a segmented appearance; solid, broad, darker masses of blastema lying opposite one another in each paraphysial thickening, and being separated by clear, narrow interspaces.

These segments are what the Germans term "Urwirbel," or "primitive vertebræ;" a somewhat misleading name, as they are in every way distinct from what are commonly understood under the name of "vertebræ," even if we use that word in its broadest signification. Professor Goodsir's terms of *Somatomes* for the segments and *Metasomatomes* for their interspaces, appear to me to be well worthy of adoption as the equivalents of these "Urwirbel."

3. The next step in the development of a vertebral column, is the histological differentiation of the somatomes. Leaving out of consideration the epithelial and other minor tissues, it may be said that each somatome gives rise to (*a*) epiaxial muscles, (*b*) a nerve and its ganglion, (*c*) the blastema for a vertebral centrum and its neural and hæmal arches, and (*d*) possibly hypaxial muscles; while the metasomatome becomes for the greater part of its extent an "intermuscular septum."

It is unnecessary for my present purpose to trace out particularly

the development of any of these parts, except the centrum and its arches.

The blastema, which is specially intended for these parts, appears, in a distinct form, first, in the paraphysial thickenings, and then extends inwards above and below, so as gradually to enclose the notochord in a sheath, while, externally, it passes in the posterior half of each somatome, upwards into the neural arches, and downwards into the hæmal arches.

4. In some *Vertebrata* the spinal column never gets beyond this stage, nor even so far; but for the present it will be well to confine our attention to those which become completely ossified. In these chondrification is the next step. The blastema of the centra and its prolongations becomes converted into cartilage, but not continuously. On the contrary, at points corresponding with the intervals between every pair of metasomatomes, or with the middle of each somatome, the cartilage is replaced by more or less fibrous tissue. As a consequence, the cartilaginous sheath of the notochord is now divided into regular segments, which alternate with the somatomes, so that each metasomatome abuts upon the middle of one of these cartilaginous vertebral centra.

In every centrum it is necessary to distinguish three tracts or regions:—1. A diaphysial region immediately surrounding the notochord. 2. Two paraphysial regions lying in the paraphysial thickenings. The paraphysial regions give rise to the cartilaginous neural and hæmal semi-arcs, which are primitively continuous with them; so that all parts of the vertebra form one connected whole.

The neural semi-arcs eventually unite in the middle line, and ordinarily send a prolongation upwards from their junction. The hæmal semi-arcs also tend to unite below, but in a somewhat different manner.

5. The last step in the development of the vertebra is the differentiation of its various parts from one another, and their final metamorphosis into their adult form. The notochord, which primitively traversed the centra and the intercentra (intervertebral ligaments, synovial membranes, or the like, between the centra), becomes more or less completely obliterated.

The distal, larger part of the hæmal semi-arc is commonly distinguished from its proximal smaller part, by the conversion of its

cartilage into osseous or other tissue, and thus the semi-arc becomes separated into a rib and an articular surface or process, for the head of that rib, to which last the term *Parapophysis* may be conveniently restricted.

In the dorsal vertebræ of many *Vertebrata,* the neural semi-arc sends out a process, the *Diapophysis,* which is eventually met by a corresponding outgrowth of the rib, its so-called tubercle, and the two become firmly connected together.

When ossification occurs, it is a very general, if not invariable rule, that an annular deposit around the notochord takes place in the centrum. I term this the *Diaphysis* of the vertebra. In some fishes a distinct centre of ossification appears in each paraphysial region, and this may be termed the *Paraphysis* of the vertebra.

In mammals each end of the vertebra ossifies from a distinct point, and constitutes a central *Epiphysis* of the vertebra; and in many *Vertebrata* a part of the under surface of a centrum ossifies separately as a distinct *Hypophysis.* It is another very general, if not invariable rule, that a distinct centre of ossification appears in, or on, each neural semi-arc or *Neurapophysis,* and passes upwards, into the spine or *Metaneurapophysis*; downwards, to unite sooner or later with the diaphysis, or diaphysis and paraphysis; and outwards into the diapophysis.

It is doubtful whether the paraphysis appears as a distinct osseous element in any *Vertebrata* above the class of fishes, in very few of which even, is it distinguishable in the adult state. Consequently in the higher *Vertebrata* the paraphysial region is ossified, either from the diaphysis or from the neurapophysis, or from both; and a suture exists for a longer or shorter time at the point of junction of the neural and central ossifications. I will term this the *Neurocentral suture.* Its position is no certain or constant indication of the nature of the parts above or below it, for it may vary in the same vertebral column from the base of the neurapophysis, to the junction of the paraphysial with the diaphysial region of the centrum.

The number of the centres of ossification in each distal portion of the hæmal semi-arc may vary greatly; the uppermost is called a *Pleurapophysis,* the lower, *Hæmapophyses* and *Met-hæmapophyses.*

Besides these primary centres of ossification of a vertebra, there are others of less constancy. Thus the ends of the metaneurapo-

physes, diapophyses, and zygapophyses in many *Mammalia* are ossified from distinct centres; and in the caudal region of many of the higher *Vertebrata*, outgrowths of the centra unite below to enclose the caudal vessels, and ossify as distinct apophyses.

If the development of the skull be now compared with that of the spinal column, it is found that (1) the very earliest changes undergone by the blastoderm in each are almost identical. The primitive groove extends to the extremity of the future cranial cavity; its lateral walls are continuous with the laminæ dorsales, and these pass into laminæ ventrales, also continuous with those of the spinal region. The laminæ dorsales of the head become the cranial walls and enclose the cerebrum—the continuation of the myelon; the laminæ ventrales give rise to the boundaries of the future buccal and pharyngeal cavities.

2. But at this point the identity of the skull with the spinal column ceases, and the very earliest steps in histological differentiation exhibit the fundamental differences between the two. For, in the first place, in no instance save the *Amphioxus*, has the notochord as yet been traced through the whole of the floor of the cranial cavity. In no other embryo has it been yet seen to extend beyond the middle vesicle of the cerebrum, or in other words, beyond the level of the rudiment of the infundibulum and pituitary body.

In the second place, the division into somatomes, in all known vertebrate embryos, stops short at the posterior boundary of the skull, and no trace of such segmentation has yet been observed in the head itself.

3. Apparently as a consequence of these fundamental differences, the further course of the development of the skull is in many respects very different from that of a vertebral column. Chondrification takes place continuously on each side of the notochord, and beyond it, the two trabeculæ cranii, unlike anything in the spinal column, extend along the base of the cranium. No distinct cartilaginous centra, and consequently no intercentra, are ever developed. The occipital arch is developed in a manner remotely similar to that in which the neurapophysial processes are formed; but the walls of the auditory capsules, which lie in front of them, and which give rise to some of the parts, most confidently regarded as neurapo-

physes by the advocates of the current vertebral theories of the skull, are utterly unlike neurapophyses in their origin.

So, if we seek for hæmal semi-arcs, we find something very like them, arising from the substance of the basis cranii beneath the auditory cartilage; but there is none connected with the occipital cartilage, and none with the rudiment of the alisphenoid. The palato-pterygoid cartilage might be regarded as the hæmal semi-arc of the presphenoidal region, though the grounds for so doing are not very strong; but the premaxillary cartilage is something quite without parallel in the spinal column.

4. The mode of ossification of the skull, and the ultimate arrangement of its distinct bony elements, are at once curiously like, and singularly unlike those presented by the spinal column. The basi-occipital is ossified precisely after the manner of a vertebral centrum. Bony matter is deposited around the notochord, and gradually extends through the substance of the cartilaginous rudiment of the part.

The combined basi- and pre-sphenoid in *Pisces* and *Amphibia* is an ossific deposit, which takes place on the under surface of the basal cartilage in front of the basioccipital, and extends thence completely beneath the pituitary interspace as far as the ethmoid. It might be paralleled by the subchordal ossification in the coccyx of the frog, or by the cortical ossification of the atlas in many higher *Vertebrata,* if it really underlay a portion of the notochord; but at the very utmost the notochord only extends into its posterior extremity.

In some of the higher *Vertebrata,* as the snake, the osseous basi-sphenoid arises in the substance of its cartilaginous rudiment, while the osseous presphenoid underlies its cartilage. In others, both bones appear to arise directly in their cartilaginous forerunners. But nothing can be more irregular than the mode of ossification of the presphenoid, ethmoid and vomer in the vertebrate series, or less like the very constant and regular course of ossification of true vertebral centra.

With respect to the ossification of the lateral and superior constituents of the skull, the development of the exoccipital and supraoccipital does, without doubt, present a very close analogy to that of the separate pieces of the neural arch of some vertebræ in, *e.g.*, a crocodile. The alisphenoids and orbitosphenoids follow in the train of the exoccipitals; but I know not where in the spinal

column we are to find a parallel for the double parietals and frontals. But waiving this difficulty, and supposing, for the sake of argument, as was supposed by Oken, that the basisphenoid, alisphenoid and parietals, the presphenoid, orbitosphenoids, and frontals represent the elements of two vertebral centra and neural arches, what is to be made of the petrous and mastoid bones?

The difficulty has been eluded by terming the petrosal a "sense-capsule," the mastoid a "parapophysis." But I apprehend that neither of these explanations can be received for a moment by those who are acquainted with the development of the skull, or with the true homologues of the bones in question in the vertebrate series, or who think that scientific terms should always possess a well-defined and single meaning.

What, in fact, is the origin of the petrous and mastoid bones? There is much reason for believing (according to Remak's late observations) that the membranous labyrinth is primarily an involution of the sensory or epidermic layer of the blastoderm; but however this may be, it is quite certain that the auditory organ is, primarily, altogether independent of the walls of the skull, and that it may be detached without causing any lesion of them, in young embryos.

It is also quite certain that this membranous labyrinth becomes invested by a coat of cartilage, continuous with the cranial wall; but I do not know that there is evidence, at present, to enable one to say positively, whether this cartilaginous auditory capsule is formed independently around the labyrinth, and then unites with the cranium; or whether it is an outgrowth from the cranial walls, which invests and encloses the labyrinth. If the latter be the case, a consistent vertebral theory of the skull must account for all the bones developed out of the auditory capsule; if the former, it must exclude them all, as parts of an extra-vertebral sensory skeleton.

Now the bones developed in the capsule are, in front, the petrosal; behind, the mastoid; above, the epiotic. The first-named bone is admitted, by the most zealous advocates of the vertebral theory, to be a neurapophysis, in all oviparous *Vertebrata*. Hence they are also bound to admit that, for three centra below and three neural spines bounding the cranial cavity above, there are four pairs of neural arches. More than this, I do not see how it is to be denied that the true mastoid is the morphological equivalent of the petrosal;

and in that case there would be five neurapophyses to three central and three neural spines. Furthermore, it is precisely to these two superfluous elements that the only two clear and obvious hæmal arches, the mandibular and hyoid, are attached.

I confess I do not perceive how it is possible, fairly and consistently, to reconcile these facts with any existing theory of the vertebrate composition of the skull, except by drawing *ad libitum* upon the *Deus ex machinâ* of the speculator,—imaginary "confluences," "connations," "irrelative repetitions," and shiftings of position—by whose skilful application it would not be difficult to devise half a dozen very pretty vertebral theories, all equally true, in the course of a summer's day.

Those who, like myself, are unable to see the propriety and advantage of introducing into science any ideal conception, which is other than the simplest possible generalized expression of observed facts, and who view with extreme aversion, any attempt to introduce the phraseology and mode of thought of an obsolete and scholastic realism into biology, will, I think, agree with me, not only in the negative conclusion, that the doctrine of the vertebral composition of the skull is not proven, but in the positive belief, that the relation of the skull to the spinal column is quite different from that of one part of the vertebral column to another.

The fallacy involved in the vertebral theory of the skull is like that which, before Von Bär, infested our notions of the relations between fishes and mammals. The mammal was imagined to be a modified fish, whereas, in truth, fish and mammal start from a common point, and each follows its own road thence. So I conceive what the facts teach us is this:—the spinal column and the skull start from the same primitive condition—a common central plate with its laminæ dorsales and ventrales—whence they immediately begin to diverge.

The spinal column in all cases becomes segmented into its somatomes; and, in the great majority of cases, distinct centra and intercentra are developed, enclosing the notochord more or less completely.

The cranium never becomes segmented into somatomes; distinct centra and intercentra, like those of the spinal column, are never developed in it. Much of the basis cranii lies beyond the notochord.

In the process of ossification there is a certain analogy between

the spinal column and the cranium, but that analogy becomes weaker and weaker as we proceed towards the anterior end of the skull.

Thus it may be right to say, that there is a primitive identity of structure between the spinal or vertebral column and the skull; but it is no more true that the adult skull is a modified vertebral column, than it would be, to affirm that the vertebral column is a modified skull*.

While firmly entertaining this belief, however, I by no means wish to deny the interest and importance of inquiries into the analogies which obtain between the segments, which enter into the composition of the ossified cranium, and the vertebræ of an ossified spinal column. But all such inquiries must start with the recognition of the fundamental truths furnished by the study of development, which, as our knowledge at present stands, appear to me to be summed up in the following propositions:—

1. The notochord of the vertebrate embryo ends in that region of the basis cranii which ultimately lies behind the centre of the basisphenoid bone.

2. The basis cranii is never segmented.

3. The lamina perpendicularis of the ethmoid has the same morphological value as the presphenoid.

4. The petrosal has the same morphological value as the mastoid; if one is not an integral part of the skull, neither is the other.

5. The nasal bones are not neurapophyses.

6. The branchial arches have the same morphological value as the hyoid, and the latter as the mandibular arc.

7. The mandibular arc is primitively attached behind the point of exit from the skull, of the third division of the fifth nerve.

8. The premaxilla is originally totally distinct from the palatomaxillary arcade.

9. The pectoral arch is originally totally distinct from the skull.

Starting on this basis, it might not be difficult to show that the perfectly ossified skull is divisible into a series of segments, whose *analogy* with vertebræ is closer the nearer they lie to the occipital region; but the relation is an analogy and not an affinity, and these cephalic sclerotomes are not vertebræ.

* I feel sure that I met with this phrase somewhere, but I cannot recollect its author.

NOTES.

I.—*On the Mastoid in Birds.*

The true mastoid of the bird seems hitherto to have escaped notice.

Hallmann says (*l. c.* p. 33), "In the disarticulated skulls of chickens, I examined the share taken by the different bones in the formation of the labyrinth, by introducing bristles into the semicircular canals, and I found in the proper *petrosum* (into which the facial and acoustic nerves enter, and which contains the cochlea) the anterior crus of the anterior canal (I term the upper one thus for ready comparison with reptiles) and of the external canal; in the supraoccipital, the upper (=posterior) crus of the anterior canal, and the upper end of the posterior canal; and in the exoccipital, the lower crus of the posterior, and the posterior of the external canal. In other words, the distribution of the canals is as in the scaly *Amphibia.* For the rest, in birds as in mammals, and probably in all *Vertebrata,* the membranous semicircular canals are formed connectedly in the cartilage, and the bony parts only gradually invest them. Hence, when the chick's skull is too young, but very little of the posterior canal is to be found in the supraoccipital, which in fact contains somewhat less of the posterior canal than of the anterior, and thereby departs from reptiles and approximates mammals.

"At a certain period also, an interval filled with cartilage, through which the semicircular canals shine, is found in the bird's skull between the supraoccipital, the parietal, the squama temporis, and the exoccipital. I see this clearly in the skull of a young *Dicholophus cristatus* (No. 5605, B. M.). In the skeleton of a young *Colymbus cristatus* (No. 7172, B. M.), I find that this interval is, on the right side, almost filled up by a small bony plate, which has not as yet combined with the surrounding bones. This appears to me to be a separate *pars mastoidea,* which however combines very early with the exoccipital. In the skull of a young goose (fig. 3) (No. 3507, B. M.), this distinct piece (*e* between *s*, *r*, *t* and *l*) is still better shown. Subsequently it is altogether indistinguishable from the exoccipital."

I have endeavoured to show, however, that the true mastoid of the bird is to be sought elsewhere; and at any rate the bone described by Hallmann has not those relations which he himself considers essential for a mastoid. It appears to me, that the distinct ossification he mentions is the epiotic bone, which has not yet combined with the surrounding parts.

II.—*On the influence of the share taken by the squamosal in the Vertebrate Skull.*

In discussing the homologies of the bones of the skull of the crocodile, Cuvier (Ossemens Fossiles, t. ix. p. 163) states that "the squamosal and zygomatic bone becomes more and more excluded from the cranium as we descend in the scale of quadrupeds, so that in Ruminants it is rather stuck upon the skull than enters into the composition of its walls;" and it is by this argument mainly that the great anatomist justifies his identification of the quadratojugal of the crocodile with the squamosal,

or rather with the zygomatic portion of that bone in mammals (*l. c.* p. 171).

Professor Owen (Principes d'Ostéologie Comparée, p. 55) adopts Cuvier's argument, and pushes it further, endeavouring to show that the disappearance of the squamosal, and as he supposes of the petrosal, from the interior of the skull in *Reptilia*, is sufficient to account for that retrogression of the alisphenoid behind the exit of the fifth nerve, which is the necessary consequence of his identification of the true petrosal with the alisphenoid.

It seems strange that Cuvier should have advanced so weak an argument as that which I have cited; for assuredly Ruminants are not very low in the mammalian scale, nor are they those mammals which most nearly approach reptiles or birds. We must seek these among rodents and monotremes, in both of which the squamosal enters largely into the composition of the cranial walls.

This is particularly the case in that especially reptilian mammal the *Echidna*. As to birds, it can still less be said that their squamosal disappears from the interior of the skull. Köstlin says on this point ('Bau des knöchernen Kopfes,' p. 206), "The squamosal contributes a small surface to the ridge, which separates the anterior cranial fossa from the middle one. It is here applied above against the parietal, anteriorly against the anterior, and posteriorly against the posterior sphenoidal ala*, and seems in all birds to appear at this point in the cavity of the skull. In the goose its extent is far smaller than in the fowl. The actual size of the ala temporis, however, surpasses that of its inner surface by a great deal. In this respect birds are analogous to the *Cheiroptera*, *Insectivora*, and a few *Marsupialia*, where only a small portion of the squama temporis projects into the cranial cavity. Still more do they resemble the seals, in which this part is entirely enclosed by the parietal and ala temporis, and so is completely separated from the petrosal." Köstlin is in error, however, in assuming that the squamosal is visible in the interior of the skull of all birds, for as we have seen above, such is not the case in the ostrich.

The struthious skull then affords an important test of the value of Professor Owen's argument. If, as he supposes, the disappearance of the squamosal from the interior of the skull causes the alisphenoid to pass behind the exit of the trigeminal, this retrogression ought to have taken place in the ostrich. Nothing of the kind has occurred, however, the trigeminal foramen being a '*trou de conjugaison*' between the alisphenoid and the petrosal. It does not even traverse the middle of the alisphenoid, as in the sheep.

It is unnecessary to discuss the effect of the disappearance of the petrosal, as I have endeavoured to prove that it does not disappear in the lower *Vertebrata*.

III.—*Connexions of the tympanic membrane in Birds.*

According to Köstlin (*l. c.* p. 216), the tympanic membrane of birds is

* Alisphenoid and petrosal, *mihi*.

stretched upon a fibrocartilaginous frame, which is ordinarily attached to the squamosal, exoccipital, basisphenoid, and quadratum. In many gallinaceous birds this frame does not come into contact with the quadratum at all. From these circumstances, and from the fact that the quadratum of birds articulates with the lower jaw and the jugal arch, which is never the case with the tympanic of mammals, Köstlin concludes, with great justice, that the quadratum is not the homologue of the mammalian tympanic.

IV.—*On the modifications of the palatosuspensorial arch in Fishes.*

I have very briefly stated my views on this subject in the Quarterly Journal of Microscopical Science for October 1858, hoping at that time to enter more largely upon the subject in this place. But the present Lecture and its notes already occupy so much space, that I must reserve a full statement of what I have to say respecting the palatosuspensorial apparatus of fishes for a future occasion.

V.—*On the development of the Cranium.*

In confirmation of the views which I have adopted, as to the primary uniformity of plan of all vertebrate crania, I subjoin an abstract of Rathke's most valuable account of the development of the skull in *Coluber natrix* *, which contains much incidental information relating to the development of the skull in *Vertebrata* in general. Vogt's observations on *Coregonus* and *Alytes*, and my own on *Gasterosteus*, *Rana* and *Triton*, are in entire accordance with those of Rathke, so far as the primitive structure of the basis cranii is concerned.

The differences between the basis of the skull and the vertebral column in the earliest embryonic condition are,—

1. That round that part of the chorda which belongs to the head, more of the blastema, that is to be applied, in the spinal column, to the formation of the vertebræ and their different ligaments, is aggregated than around the rest of its extent, and—

2. That this mass grows out beyond the chorda to form the cranial trabeculæ.

The lateral trabeculæ at their first appearance formed two narrow and not very thick bands, which consisted of the same gelatinous substance as that which constituted the whole investment of the chorda, and were not sharply defined from the substance which lay between them and at their sides, but seemed only to be two thickened and somewhat more solid, or denser, parts of that half of the basis of the cranium, which lies under the anterior cerebral vesicle.

Posteriorly, at their origin, they were separated by only a small interval, equivalent to the breadth of the median trabecula, and thence swept in an arch to about the middle of their length, separating as they passed forwards; afterwards they converged, so that, at their extremities, they were separated by a very small space, or even came into contact. Altogether they formed, as it were, two horns, into which the investing

* Entwickelungsgeschichte der Natter, 1839.

mass of the chorda was continued forwards. The elongated space between them, moderately wide in the middle, was occupied by a layer of softer formative substance, which was very thin posteriorly, but somewhat thicker anteriorly. Upon this layer rested the infundibulum; and in front of it, partly on this layer, partly on the trabeculæ, that division of the brain whence the optic nerves proceed, and further forwards the hemispheres of the cerebrum. Anteriorly, both trabeculæ reached as far as the anterior end of the head, and here bent slightly upwards, so that they projected a little into the frontal wall of the head, their ends lying in front of the cerebrum. Almost at the end of each horn, however, I saw a small process, its immediate prolongation, pass outwards and form, as it were, the nucleus for a small lateral projection of the nasal process of the frontal wall.

The middle trabecula grows, with the brain, further and further into the cranial cavity, and as the dura mater begins to be now distinguishable, it becomes more readily obvious than before, that the middle trabecula raises up a transverse fold of it, which traverses the cranial cavity transversely*. The fold itself passes laterally into the cranial wall; it is highest in the middle, where it encloses the median trabecula, and becomes lower externally, where it forms, as it were, a short ala proceeding from the trabecula. With increasing elongation, the trabecula becomes broader and broader towards its free end, and, for a short time, its thickness increases. After this, however, it gradually becomes thinner, without any change in its tissue, till, at the end of the second period, it is only a thin lamella, and after a short time (in the third period) entirely disappears.

In mammals, birds, and lizards, that is, in those animals in general, in which the middle cerebral vesicle is very strongly bent up and forms a protuberance, while the base of the brain exhibits a deep fold between the infundibulum and the posterior cerebral vesicle, a similar part to this median trabecula of the skull is found.

In these animals, also, at a certain very early period of embryonic life, it elevates a fold of the dura mater which passes from one future petrous bone to the other, and after a certain time projects strongly into the cranial cavity. Somewhat later, however, it diminishes in height and thickness, as I have especially observed in embryos of the pig and fowl, until at last it disappears entirely in these higher animals also, the two layers of the fold which it had raised up coming into contact. When this has happened, the fold diminishes in height and eventually vanishes, almost completely.

The two lateral trabeculæ, which in the snake help to form the anterior half of the basis of the skull, attain a greater solidity in the second period, acquire a greater distinctness from the surrounding parts, and assume a more determinate form, becoming, in fact, filiform, so that the further forward, the thinner they appear. They increase only very little in thickness, but far more in length, during the growth of the head. Altogether anteriorly, they coalesce with one another, forming a part

* What Rathke terms the 'middle trabecula,' appears to be only very indistinctly developed in *Fishes* and *Amphibia*.

which lies between the two olfactory organs and constitutes a septum. As soon as these organs increase markedly in size, this part is moderately elongated and thickened, without however becoming so dense as the hinder, longer part of the trabeculæ. The prolongations into the lateral projections of the nasal processes, which now proceed from the coalesced part in question, also become but little denser in texture for the present, though they elongate considerably.

The lateral parts and the upper wall of the cranium, with the exception of the auditory capsules or of the subsequent bony labyrinth, remain merely membranous up to the end of the second period, consisting in fact only of the cutaneous covering, the dura mater, and a little interposed blastema, which is hardly perceptible in the upper part, but increases in the lateral walls, towards the base of the skull.

The chorda vertebralis reaches, in very young embryos of the snake, to between the auditory capsules, and further than this point it can be traced neither in the snake nor in other *Vertebrata*, at any period of life, as manifold investigations, conducted with especial reference to this point, have convinced me.

At the beginning of the third period, the basal plate chondrifies, at first leaving the space beneath the middle of the cerebellum membranous; but this also eventually chondrifies, and is distinguished from the rest of the skull only by its thinness.

Lateral processes grow out from the basal cartilage just in front of the occipital foramen, and eventually almost meet above. They are the exoccipitals.

The two lateral trabeculæ, parts which I have also seen in frogs, lizards, birds, and mammals, chondrify at the beginning of the third period. At first, they pass, separate from one another throughout their whole length, as far as the frontal wall, on entering which they come into contact; are more separate posteriorly than anteriorly, and present, in their mutual position and form, some similarity with the sides of a lyre. But as the eyes increase, become rounder, and project, opposite the middle of the trabeculæ, downwards towards the oral cavity, the latter are more and more pressed together, so that even in the third period they come to be almost parallel for the greater part of their length. Anteriorly, however, where they were already, at an earlier period, nearest to one another, they are also pressed together by the olfactory organs (which have developed at their sides to a considerable size), to such a degree, that they come into contact for a great distance and then completely coalesce; they are now most remote posteriorly, where the pituitary body has passed between them*, so that they seem still to embrace it. Anteriorly, between the most anterior regions of the two nasal cavities, they diverge from their coalesced part as two very short, thin, processes or cornua, directed upwards, and simply bent outwards.

" It has been seen above that the median trabecula does not chondrify,

* The pituitary body, however, as Rathke now admits, does not pass between the trabeculæ, and is developed in quite a different manner from that supposed in the memoir on *Coluber*.

but eventually disappears; in its place, a truly cartilaginous short thick band grows into the fold of dura mater from the cartilaginous basal plate.

"Where the pituitary gland lies, there remains between the lateral trabeculæ of the skull a considerable gap, which is only closed by the mucous membrane of the mouth and the dura mater. But there arises in front of this gap, between the two trabeculæ, as far as the point where they have already coalesced, a very narrow, moderately thick, and anteriorly pointed streak of blastema, which, shortly before the end of the third period, acquires a cartilaginous character, and subsequently becomes the body of the presphenoid*.

"Altogether anteriorly, however, where the two trabeculæ have coalesced, there grows out of this part, from the two cornua in which it ends, a pair of very delicate cartilaginous plates. At the end of the third period both plates acquire a not inconsiderable size, take the form of two irregularly formed triangles, and are moderately convex above, concave below, so as to be on the whole, shell-shaped. The nasal bones are developed upon these, while below them are the nasal cavities, and the nasal glands with their bony capsules.

"The alæ or lateral parts of the two sphenoids do not grow like the lateral parts of the occipital bone out of the basis cranii, whose foundation is formed by the cephalic part of the chorda, but are formed separately from it, although close to it, in the, until then, membranous part of the walls of the cranium.

"The alæ of the presphenoid (orbitosphenoids), which are observable not very long before the termination of the third period, appear as two truly cartilaginous (though they never redden), irregular, oblong, plates of moderate thickness, lie in front of the optic foramina, at the sides of the lateral trabeculæ of the skull, ascend from them upwards and outwards, and are somewhat convex on the side turned to the brain, somewhat concave on the other. The alæ magnæ (alisphenoids) are perceptible a little earlier than these. They are formed between the eye and the ear, and also originally consist of a colourless cartilaginous substance; they appear at the end of the third period as irregular four-sided plates, lie at both sides of the anterior half of the investing plate of the chorda, ascend less abruptly than the alæ orbitales, and are convex externally, internally concave.

"The upper posterior angle of each elongates, very early, into a process, which grows for a certain distance backwards, along the upper edge of the auditory capsule, and applies itself closely thereto.

"The auditory capsules, or the future petrous bones†, chondrify, as it would appear, the earliest of all parts of the skull: the fenestra ovalis arises in them by resorption.

"The *ossification* of the snake's skull commences in the basioccipital, or at any rate, this is one of the first parts to ossify. At a little distance from

* Compare with these statements, the figures and descriptions given above of the embryonic cranium in *Gasterosteus* and *Rana*.

† It will be found from Rathke's statements, further on, that the future petrous bone only represents a portion of each auditory capsule.

the occipital foramen, there arises a very small semilunar bony plate, whose concave edge or excavation is directed forwards; thereupon the bony substance shoots from this edge further and further forwards, until at length the bony plate has the form of the ace of hearts. Its base borders the fontanelle in the base of the skull, which lies under the anterior half of the third cerebral vesicle, while its point is contiguous to the occipital foramen; for the most part it is very thin, and only its axis (and next to this its whole posterior margin) is distinguished by a greater thickness. The cephalic part of the chorda can be recognized in the axis of this bony plate up to the following period. It passes from the posterior to the anterior end of the bony plate, where it is lost, and is so invested by the bony substance of the plate, that a smaller portion of the latter lies on the upper side of the chorda, a larger portion beneath it. On this account it forms, on the upper side of the plate, a longitudinal ridge, which subsequently becomes imperceptible by the aggregation of matter at the sides. On one occasion, however, I saw, in an embryo which was almost at term, a similarly formed and sized bony cone, which, through almost its entire length, appeared merely to lie *on* the body of the basi-occipital, since it had only coalesced with it below."

The nucleus and sheath of the cephalic part of the chorda become gradually broken up and the last trace of them eradicated, as the ossification of the basioccipital proceeds, like the nucleus and sheath of the rest of the chorda wherever a vertebral body is developed*.

The articular condyle is not yet formed. The exoccipitals ossify through their whole length and breadth.

The body of the basisphenoid is formed between the above-mentioned posterior fontanelle of the basis cranii and the pituitary space, 'therefore far from the cephalic part of the chorda.' It ossifies by two lateral centres, each of which forms a ring round the carotid canal. The alisphenoids ossify in their whole length and breadth; the orbitosphenoid only slightly, and the presphenoid not at all. The premaxillary bone arises as an azygos triangular cartilage between the cornua of the anterior ethmo-vomerine plate. It ossifies from a single centre.

"The auditory capsule, or the future petrosal bone, may, even at the end of this period, be readily separated from the other part of the cranial wall, and still consists for the most part of cartilage. On the other hand, the triangular form, which it had before, is not inconsiderably altered, since it greatly elongates forwards, and thus, as it were, thrusts its anterior angle further and further forwards, and becomes more unequal-sided. At the lower edge, or the longer side of it, about opposite to the upper angle, at the beginning of this (third) period, or indeed somewhat earlier, a diverticulum of the auditory capsule begins to be formed (the rudimentary cochlea), and developes into a moderately long, blunt, and hollow appendage, whose end is directed downwards, inwards and backwards,

* In the stickleback it has appeared to me that the wall of the anterior conical termination of the notochord in the basis cranii becomes ossified, or at any rate, invested by an inseparable sheath of bony matter, just in the same way as the 'urostyle' is developed in the tail.

and also consists of cartilage. Close above, and somewhat behind this appendage, however, there appears, at about the same time, a small rounded depression, in which the upper end of the auditory ossicle eventually rests; and somewhat later, an opening appears in this depression which corresponds with the fenestra rotunda of man. Very much later, namely, towards the end of this period, the auditory capsule begins to ossify. Ossification commences in a thin and moderately long, hook-like process, which is sent forwards and inwards from the lower hollow diverticulum of the cartilage, and unites with the basisphenoid. From this point it passes upwards and backwards, and, for the present, extends so far that, at the end of this period, besides that process, the diverticulum in question and about the anterior third of the auditory capsule itself, are ossified. Later than at the point indicated, an ossific centre appears at the posterior edge of the auditory capsule, where it abuts against the supra- and ex-occipitals, but extends from hence by no means so far forward as to meet that from the other point. The middle, larger part of the auditory capsule, therefore, for the present, remains cartilaginous.

"In the beginning of the fourth period, a third ossific centre arises in the upper angle of the capsule, whereupon all three grow towards one another. But the mode of enlargement and coalescence of these bony nuclei is very remarkable. They do not unite with one another in such a manner as to form a continuous bony capsule for the membranous part of the labyrinth, but are permanently separated by cartilagino-membranous and very narrow symphyses. On the other hand, one coalesces, in the most intimate manner, with that edge of the supraoccipital which is nearest to it, so that even in the more advanced embryos, this bone and it form a moderately long oblong plate, each end of which constitutes a small, tolerably deep, and irregularly-formed shell, containing a part of the anterior or upper semicircular canal. The second bony centre becomes anchylosed with the anterior edge of the lateral part of the occipital bone, and also forms a small, irregularly-shaped, but longish scale, which contains the deeper or lower part of the posterior crus of that semicircular canal, and besides this, the lower sac, or representative of the cochlea of the auditory labyrinth. The remaining bony mass of the auditory cartilage, however, includes the greater part of the membranous portion of the labyrinth, and is the largest. The same phenomenon, viz. that the petrosal bone breaks up, as it were, into three pieces, of which two coalesce with the occipital bone, occurs also, according to my observations, in *Lacerta agilis*, and probably takes place in like manner, if we may conclude from the later condition of the petrous bone to the earlier, in *Crocodilia* and *Chelonia*. A squama temporis and a mastoid are, as I judge, never formed in *Ophidia*."

Yet what is the osseous mass which eventually coalesces with the exoccipital but the mastoid? I have indicated above what I believe to be the true ophidian squamosal.

VI.—*On the development of the Ossified Vertebral Column.*

The concise statement of the general nature of this process which I

have given above, is based, partly on the observations of Vogt, Rathke, and Remak, and partly on my own. As great misunderstanding seems to me to have prevailed on this head, I have put together in the present note, all the most important evidence I have been able to collect on this highly interesting subject, accompanying it with a running commentary. I have done this the more willingly, as the accounts of the mode of development of vertebræ in general, in our own language, which I have met with, are strangely meagre.

Development of the Spinal Column in Fishes.

1. *Blennius viviparus* (Rathke, 'Bildungs- und Entwickelungsgeschichte des *Blennius viviparus.*' 1833).

The surface of the notochord hardens, and acquires a fibro-membranous consistence, while its inner substance becomes glassy and transparent, so that the notochord is separated into sheath and contents, as in the lamprey. A segmentation next takes place in the sheath. "At successive intervals it increases, more and more, in density and solidity, acquires in places almost the constitution of cartilage, and there thus arise a great number of successive, very fine, narrow rings, which are connected by much narrower, far less solid, but also far less transparent and more whitish-coloured parts, like sutures."

When this segmentation has commenced, "a number of cartilaginous, very short, thin and rod-like processes, which run in pairs from each member of the vertebral column, where its upper side passes into the two outer ones, appear, pass upwards in the walls surrounding the spinal marrow, and enclose its lower cords. At first, therefore, each pair of processes are separated by a considerable interval throughout their entire length. Subsequently their upper ends approximate (increasing in length, and at the same time accommodating themselves to the curve of the spinal marrow, and bending round it) more and more closely, till they, at last, meet above the spinal marrow, and soon after this has happened, coalesce into an arch.

"Contemporaneously with these processes, and in the same way, there arise from the vertebral column (though only from its hinder half, or that which constitutes the foundation of the tail) a number of other processes similar in form and structure, which spring from the junction of the under with the lateral faces of the column. These take the opposite direction to the preceding, tend to enclose the great caudal vessels, and unite in pairs into arches, which lie in a series and correspond with the vertebral segments."

From the segments of that part of the vertebral column which lies between the tail and the head, there grow out, in corresponding places to those in which the crura of the inferior arches take their origin from the vertebral segments of the tail, and in the same manner and at the same time, many cartilaginous processes, which attain, however, only a very slight length, and also take a transverse direction. They might be regarded as lateral pieces of the transverse processes of the higher animals; but it is more probable that they correspond with the ribs of other *Vertebrata.*

"All these processes are connected with the sheath, but not with the core of the notochord."

As development advances, the ring-like segments increase in breadth, length and thickness; at the same time they become somewhat cartilaginous, and then ossify. Each widens somewhat more at its ends than in the middle, and so appears a little contracted in the centre. It is only after birth that such an internal thickening takes place as to interrupt the cavity of the vertebral centrum.

The sheath of the notochord is originally of one texture throughout, but the smaller portions, which lie between the vertebral centra, assume a fibrous texture, contemporaneously with the appearance of the latter. The included substance of the notochord loses its peculiar dense and elastic character, becomes first gelatinous, then grumous, and finally resembles a thick serum.

The crura of the upper and lower vertebral arches (in the tail) unite in pairs, and their points of union grow out into spinous processes.

The ossification of the processes which arise from the vertebræ commences at the point of junction of the process with the centrum. "A small bony point arises, which appears to belong to both centrum and process, and from whence ossification extends into both. In each vertebral centrum, therefore, as well of the tail as of the trunk, ossification proceeds from different and distant points."

2. *Cyprinus blicca* (Von Bär, 'Untersuchungen über die Entw. d. Fische.' 1835).

"At the end of the first day the notochord is covered by something which surrounds it like thin plates; these are the developing bodies of vertebræ. It is clearly observable that these bodies of vertebræ are not undivided rings surrounding the notochord, but that they consist of many pieces united by sutures. This condition also is persistent in the sturgeons. The body of the vertebra, therefore, is formed of the coalescence of many pieces, and a lateral suture seems to indicate that these processes are elongations of the previously observed upper and under vertebral arches."

Von Bär imagines that the unconstricted part of the notochord gives rise to the intervertebral ligaments.

From these observations of Rathke and Von Bär, it would appear as if the annular ossifications which surround the notochord arose by the coalescence of ossific centres, primarily developed at the junction of the apophyses with the centra. My own observations on *Gasterosteus*, however, show, like those of Vogt on *Coregonus*, that the centra ossify from distinct rings deposited immediately round the notochord; and I am very strongly inclined to believe that the corresponding primary annular diaphyses of the vertebræ in *Cyprinus* and *Blennius* have been overlooked.

3. *Coregonus palea* (Vogt, 'Embryologie des Saumones,' 1842, p. 104 *et seq.*).—"But it is necessary to distinguish carefully between what we call vertebræ in the adult fish, that is to say, those osseous or cartilaginous pieces intended for the support of the whole body, and more particularly of the spinal cord, and such vertebral divisions as we find in embryos. These

last are the general fact, the expression of a constant law according to which all the *Vertebrata* are developed. The vertebræ of adult fishes, on the other hand, are solid rings, whose presence depends on the type peculiar to each species; consequently their form, and the substance of which they are composed, vary in almost every species.

"The vertebral divisions * appear very early in the *Coregonus*—almost at the same time as the notochord; and when the dorsal groove begins to close, they are fine lines, caused, as it would appear, by a greater accumulation of embryonic cells, which, like transverse septa, traverse the entire mass as far as the notochord. These divisions extend forwards, as far as the neighbourhood of the auditory vesicles, but there never exists the smallest trace of them in the head itself. At first they are visible only in the middle of the body; by degrees they move forwards, as far as close to the ear, and backwards, towards the tail, as far as it is formed; but they invade its extremity only when it has attained its full length relatively to the body. At first these lines are all straight and perpendicular to the axis of the chorda; but by degrees, and in proportion as development advances, they become oblique and bend, forming an angle whose apex is directed forwards, and corresponds exactly to the median line of the notochord."

They eventually become the intermuscular septa. Vogt goes on to say,—"The typical structure of the *Vertebrata,* then, consists solely in these rings of separation, which are formed around a notochord, and nowise in the development of a distinct head, or of other solid pieces of the skeleton, such as osseous or cartilaginous vertebræ," illustrating his case by the *Amphioxus.*

Each osseous vertebra corresponds to two vertebral segments, namely, to the half of that which precedes, and to the half of that which follows a metasomatome; for it is where the latter reaches the notochord, that the centra and arches take their origin. The centra arise as a double ring of cartilage, internal and external to the sheath of the notochord. The intervertebral spaces always correspond with the middle of the interval between two intermuscular septa, each of which is consequently inserted into the middle of a centrum, while the superior and inferior arches are developed in their plane. They are ossified only long after the centra, which arise as bony rings around the notochord. The intervertebral ligaments are formed from the sheath of the notochord.

4. Prof. Owen (Principes d'Ostéologie Comparée, 1855, p. 184) affirms that "In osseous fishes the centrum is ordinarily ossified from six points, of which four begin in the bases of the two neurapophyses and of the two parapophyses, but the terminal concave plates of the centrum are ossified separately."

It is not stated on what fish the observations on which this latter assertion is based were made. Prof. Williamson has already shown ("On the Development of the Scales and Bones of Fishes," Phil. Trans. 1851) that it is inconsistent with the structure of the adult vertebra; it is not supported by any of those writers who have directly observed the develop-

* Metasomatomes, their interspaces being the somatomes.

ment of the vertebræ of Teleostean fishes, and it is negatived by the observations of Vogt just cited and by my own.

Development of the Spinal Column of Batrachia.

1. *Anura* (Dugès, 'Recherches sur les Batraciens,' 1835).—In the first period the notochord appears to be divided transversely into "rondelles" or vertebræ, but these are not real divisions; they are appearances produced by the intersections of the muscles which surround the notochord, and of the transverse vascular branches which accompany each pair of nerves when it leaves the medulla. [Dugès thus describes the somatomes.]

In the second period, cartilaginous processes, adherent to the notochord, appear in pairs and enclose the medulla. There are as many of these processes as vertebræ will in future exist, and two crests even make their appearance, to form the walls of the coccygeal canal. The apophyses are at first little tubercles; they then bifurcate; one branch becomes the transverse process, the other the neurapophysis with its zygapophyses. In *B. fuscus, A. obstetricans, punctatus,* and *Hyla,* where these vertebræ ossify, "two clouds" of ossific matter make their appearance in each vertebra, "as distant from one another as they are from the lateral masses or apophyses," and eventually unite above the notochord so as to form a quadrate ossific centre. This quadrate mass enlarges, but remains concave, not only above, but also in front and behind, and especially below, where it forms a semi-canal or groove, in which the notochord is lodged. The groove is gradually filled up, the notochord undergoing a contemporaneous atrophy, and becoming eventually reduced to a mere ligament. The intervertebral masses are formed altogether independently of the notochord.

In *Rana,* on the other hand, the primitive centre of ossification of the body of the vertebra is a ring completely enclosing the chorda; in other respects the development of the spinal column resembles that just described.

2. Müller (Vergleichende Anat. d. Myxinoiden, 1835) remarks, "that the formation of the primitive elements of the skull (which are different from the secondary osseous ones) takes place in the higher animals, constantly in the same way, is much to be doubted, since variations of the fundamental plan obtain in the vertebral column. In many *Batrachia,* as *Cultripes provincialis,* and *Rana paradoxa,* the bodies of the vertebræ arise only out of the upper primitive vertebral elements. I found, indeed, in the larva of *Rana paradoxa,* on the under part of the circumference of the chorda dorsalis, a cartilaginous band which was especially well developed posteriorly, in front of the ossification of the coccygeal spine, and was continued, thinner, along the under surface of the chorda, for half the length of the future vertebral column. This cartilaginous band had no fellow, but on the contrary, was thickest in the middle. In the caudal part of the chorda it diminished until it gradually disappeared, so that the inferior arches surrounding the caudal vessels were merely fibrous productions of the external sheath of the chorda. But this inferior cartilaginous band on the chorda of the larva of *Rana paradoxa* disappears in the greater

part of the spinal column, and merely a part of it ossifies to become the basilar part of the coccyx, which Dugès was acquainted with, as well as with the two vertebræ of the coccyx above the chorda: the basilar bone is not a body of a vertebra, but coalesces subsequently with the inferior circumference of the coccygeal vertebræ. In these frogs, the coccyx is the only part which arises from both upper and lower vertebral elements; all the other vertebræ arise in *Cultripes* and *Rana*, merely from the upper primitive vertebral elements, which in the course of ossification become divided into arches and central portions. It is only the ossifications of the coccyx which, in these frogs, completely enclose the chorda, since that part is eventually composed of two pairs of vertebræ, and a long basilar piece, whose sutures are retained even in the adult *R. paradoxa*" (p. 130).

3. *Alytes.*—With respect to the development of the vertebræ in *Alytes obstetricans,* Vogt (Entwickelungsgeschichte der Geburtshelferkröte, 1842) states that cartilaginous rings appear in the sheath of the chorda, as rudiments of the centra. Contemporaneously with these the cartilaginous neural arches are developed in the wall of the canal of the medulla; nothing is said as to the mode of ossification.

4. In both *Rana temporaria* and *Triton,* I find that the diaphysis of the vertebra arises as a saddle-like patch, upon, and in immediate contact with, the dorsal surface of the notochord; the layer of osseous matter is at first exceedingly thin, and gradually extends round the notochord until in most of the frog's vertebræ, and in all of those of the *Triton,* it forms a complete ring. The osseous deposit in the arches is quite distinct, and has, in the frog, the form of a thin bony sheath investing their cartilaginous basis. The diaphysis of the sacral vertebra remains open below long after the others, and after its neural arch is completely ossified.

5. The development of the coccyx of the anourous *Batrachia* has been well described by Dugès (*l. c.* p. 108). The two neural arches originally formed in this region ossify and unite above the spinal cord, and at the same time two osseous centra, which very soon coalesce with them, are formed. These centra are incomplete arcs, open below, where they embrace the notochord. A long cartilaginous plate, however, arises on the ventral surface of the notochord, extending backwards far beyond the level of these posterior coccygeal vertebræ. It ossifies, and eventually becomes anchylosed with the bodies of the coccygeal vertebræ to form the coccyx. Such is the substance of Dugès' views, which, as has been seen, have been confirmed in all essential points by Müller.

Prof. Owen, however, gives a very different account of the matter.

"The vertebræ of the tail of the larvæ of the *Anura* are seen distinctly only in the aponeurotic stage. When chondrification occurs, the operation of absorption and coalescence takes place, and two long neurapophyses only are established on each side; the ossification of these plates extends into the fibrillar sheath of the rest of the coccygeal notochord, and when the perishable parts of the tail of the larva have been absorbed, and the fore- and hind-legs are developed, they constitute by their connation the elongated, osseous coccygeal style, often hollow, of the anourous *Batrachia.*" (Principes d'Ostéologie, 1855, p. 186.)

Prof. Owen does not state on what anourous batrachian his observations were made, nor does he notice the wide discrepancy between his views and those of Dugès. I have carefully studied the development of the coccyx in the common frog, and my observations are in entire agreement with those of Dugès. Nothing can be more clear than the primitive entire independence of the inferior cartilaginous plate, which by its ossification constitutes the major part of the coccygeal style, from the two neurapophyses and the rudimentary diaphyses which correspond with them.

Development of the Spinal Column of Reptilia.

1. *Ophidia* (Rathke, 'Entw. der Natter,' 1839).—"Quadrate plates of a more solid substance than the rest of the blastema appear on each side of the notochord, in the middle of the body, where they are at first largest, diminishing in size backwards and forwards. At first they extend neither into the dorsal nor into the ventral plates of the embryo.

"These plates increase in length, and those of each pair grow towards one another above and below. Each plate grows out into two branches above and below. The inner branches lie in close contact with the notochord, and coalesce with those of the opposite side, so as to form rings which eventually become the bodies of the vertebræ. The upper outer branch extends into the wall of the neural canal, and, eventually uniting with its fellow, forms the neural arch. The lower outer branch extends into the ventral wall and becomes the rib."

The ring grows both externally and internally, so as to constrict the notochord (which softens and acquires a grumous consistence), and then becomes converted into bone, so that the notochord is surrounded by a series of bony rings. The notochord takes no part in the formation of the articular intervertebral surface, which is an apophysis and not an epiphysis; the neural arches ossify much later than the centrum, from a single point in the middle of each. The inferior processes are outgrowths of the substance of the vertebra, and in the caudal region are, from the first, double.

"On each side of the body of the vertebra, where the ribs and the vertebral arches radiate from it, the condensed blastema of which it originally consists, grows out slowly, but considerably, and becomes developed (gradually undergoing chondrification) into a plate, whose largest surfaces are vertical, which increases more in length than in breadth and thickness, and which gradually drives the rib and vertebral arch further away from the axis of the vertebral column. At the end of this period it is almost as thick as the length of the vertebra; it then appears, when viewed from in front or behind, as a short irregular oblong, one of whose angles passes into the body of the vertebra, and one of whose shorter sides is turned outwards and upwards. From this side passes one crus of the vertebral arch, while from the side which is turned outwards and downwards, at least in most of the vertebræ, a rib is developed, so that these processes are connected with the originally existing part of the body of the vertebra, only mediately, by the plate in question.

"The crura of the vertebral arch ossify from their middle towards both

ends, the process commencing very soon after the ossification of the centra; but after ossification has begun in them, these originally filiform parts widen into broad oblong plates, which, in the greater part of the body, come into contact only towards the end of the period, and in the tail and hindermost part of the body, only in the following period.

"The ribs ossify far later, and also from the middle towards the end. Before, however, an ossific centre is developed in them, the cartilage, of which the rib now for the most part consists, becomes articulated with the rest of the vertebra. The plate, lastly*, which forms the union between a rib, a vertebral arch and a vertebral body, and subsequently forms a part of the body of the vertebra, ossifies only in the following period.

"The relation of the ribs to the bodies of the vertebræ, therefore, is originally just the same as that of the crura of the vertebral arches to them: just as one of these crura, does each rib arise as an outward growth of that vertebral body which is the first formed of all these parts; whilst, however, the crura of the vertebral arches are directed upwards in order to enclose the spinal cord, the ribs grow downwards to enclose the viscera of organic life, and in this way their greater length in the snake, and a multitude of other *Vertebrata*, is explicable.

"The transverse processes of the caudal vertebræ exhibit the same relations to the bodies of the vertebræ, as the ribs in the dorsal region. They arise in the same places as these; become, in like manner, removed, together with the crura of the arches, by lateral outgrowths of the body from its axis; and, before the ribs are articulated, they pass quite imperceptibly into the processes in question. The transition is the more remarkable, as in the snake the hindermost rib is split, in just the same manner as the transverse processes of the three or four succeeding caudal vertebræ.

"If we consider, in the first place, the relation of the parts in the adult snake, we find that the penultimate rib, near its upper or inner end, gives off from its upper side, a small process directed upwards and outwards; however, in the last rib this process is about a quarter as long as the remaining part of the rib, which lies external to and below it, so that the whole bone has the form of a two-pronged hay-fork, not yet fastened to a handle, and one of whose prongs is for the most part broken off. The same fundamental form is possessed by the transverse process of the first caudal vertebra; and the difference between it, and the rib which lies immediately before it, lies principally in this, that it is not, like the rib, articulated with its vertebra, that its upper half or prong is almost equal in length to the lower, and that, regarded as a whole, it is not half so long as the hindermost rib. The transverse processes of the succeeding caudal vertebræ have quite the same form as their predecessors, but gradually diminish in length backwards. As to the development of the ribs and transverse processes in question, they, like almost all the other ribs, are originally sent out as quite simple rays from the bodies of their vertebræ; very soon, however, there arises on the upper side and neck of the ray where it passes out from the vertebral body, an outgrowth which elongates

* Paraphysial cartilage.

more or less, also assumes a ray-like form, and has its free end directed outwards. Thus a fork is produced, whose one prong is more or less thicker than the other."

Rathke suggests that the accessory ribs of many fish are probably developed in this way, the upper prong becoming articulated with the lower.

"The two or three anterior subvertebral processes in the tail are, and remain, quite simple, like the similar processes of the cervical and many dorsal vertebræ. The two halves of the others, which arose as separate lateral processes, remain permanently distinct.

"All the newly commencing inferior processes arise as paired outgrowths of the bodies of the vertebræ."

"The ribs are not less outgrowths (*ausstrahlungen*) of the bodies of the vertebræ than the crura of the arches, as I can say from my investigations on fishes, snakes, lizards, birds, and mammals; even when the bodies of the vertebræ are completely chondrified, the ribs form a connected whole with them, but subsequently they become articulated, and are thereby essentially distinguished from the crura of the vertebral arches. In some animals the articulation takes place and remains close to the bodies; in others it takes place also close to the bodies, yet, afterwards, a process grows out between the rib and the body, by which the rib is more or less thrust out; this is the transverse process; where it occurs, the rib is, in all cases, at first united only with it; sometimes, however, a process grows out from the rib (the so-called head with its neck), by which it becomes immediately attached to the body of the vertebra itself, so that it is doubly united with the body. In many cases the rib may also become articulated at some distance from the body, and thus break up into rib and transverse process."

"As respects the ribs of the higher *Vertebrata*, together with their transverse processes, the development of the snake teaches us, that although they are subsequently seen to be in close connexion with the crura of the vertebral arches, they grow out, not from the base of these arches, but far from them, out of the bodies of the vertebræ themselves; where they have arisen, however, each lateral half of the body of the vertebra increases in thickness, in such a manner that it acquires an ala, which drives the crus of the vertebral arch and the rib further and further from the axis, until at length it appears as a common trunk for both, and therefore may easily deceive one into supposing, that the rib is given off from the base of the crus of the arch, and is a process from it."

2. *Lacertilia* (Rathke, 'Ueber die Entwick. d. Schildkröten,' 1848, pp. 65–67).—In the lizards (as in the snakes), the osseous centra of the vertebræ appear as rings, which do not so closely embrace the notochord (as they do in fishes and Batrachia), being separated from it by a layer of cartilage.

3. *Chelonia* (Rathke, 'Schildkröten,' p. 65–67).—In the *Chelonia* two bony rings arise, the one on the outer surface of the cartilaginous basis of the centrum, the other close to the notochord; the rings thicken and eventually coalesce; the ossification of the arches takes place quite independently of that of the centrum. The notochord takes no essential part

in the formation of the articulations between the vertebræ, but runs like a thread through them.

Development of the Spinal Column of Birds.

In the Chick (Remak, 'Untersuchungen über die Entw. die Wirbelthiere,' 1855).—The upper and middle (sensory and motor) layers of the germ coalesce to form the axial plate (primitive streak); the lateral halves of this plate thicken and leave between them a groove, the primitive groove; immediately below the groove, and parallel with it, the notochord appears in the axis of the motor layer. The sensory layer of the axial plate is now the medullary plate; from it the nervous centre is developed; the motor layer is termed by Remak the 'Urwirbel-platte,'—the primitive vertebral plate.

The primitive vertebræ (Urwirbel, somatomes) first appear in the dorsal part of the embryo, as opaque portions of the substance of the primitive vertebral plates, which extend from the sides of the chorda into the lateral plate, or that thickened part of the motor layer with which the primitive vertebral plates are immediately continuous. These primitive vertebræ are the result of a sort of segmentation of the motor layer; they acquire a cubical form, and are separated by clear, narrow, interspaces (metasomatomes).

At the beginning of the third day the ventral surface of each primitive vertebra has an almost square shape with rounded angles; the transverse section is no longer square but three-sided, the upper and outer faces having merged into one convex face; the surface turned towards the medullary canal is four-sided and a little concave.

The inferior internal edge of the primitive vertebra grows out towards the notochord, and having reached its outer side, it divides into two lamellar processes, which, coalescing with those of the other side, surround the notochord and constitute the blastema of the vertebral column.

The dorsal layer of the primitive vertebra becomes converted into muscle, and forms a segment of the dorsal muscles; the anterior portion of its substance, beneath this, becomes the spinal ganglion; the posterior, the rudiment of the neural arch and rib. The latter extends backwards beyond the boundary of its primitive vertebra into the region of the next, so that it appears to be divided by the clear line of separation into a larger anterior and smaller posterior portion.

The axial portion of the vertebral column does not become segmented in correspondence with the divisions between the primitive vertebræ, but midway between them, so that the lines of separation between the primitive vertebræ correspond with the centres of the permanent vertebræ, each of which may thus be said to be formed by the coalescence of the posterior half of the axis of one primitive vertebra with the anterior half of the next following.

Rathke, 'Schildkröten,' p. 66.—In birds, the centra commence as bony rings which closely encircle the chorda, and lie internal to the general cartilaginous mass of the vertebra. The bony substance extends inwards and constricts the notochord, outwards it permeates the vertebræ.

"In the caudal vertebræ, and perhaps in all the cervical vertebræ, the bony substance of the rings extends gradually to the surface of their centra. In the dorsal vertebræ, on the other hand, there is formed at the fifteenth day, independently of these rings, a broad though thin bony plate on the upper, and a second on the lower face of the centrum, with which the substance of the ring, as it extends, coalesces. The notochord at the eighteenth day may be seen traversing the intervertebral articular cavities like a thread."

Development of the Spinal Column of Mammalia.

Rathke, 'Schildkröten,' pp. 66, 67.—"In the pig and sheep the bony substance is deposited immediately around the notochord, in such a manner that, at first, as in birds, it forms a narrow and thin ring, from which it passes partly towards the surface, partly towards the ends of the separate centra, and after a time reaches the surface, but not the ends. To complete the centra, there arise in the latter two special disks of bone for each vertebra, which afterwards apply themselves to the previously ossified middle part, and wholly coalesce with it.

"In pig-embryos of 1 in. to 1 in. 3 lines in length, the notochord ran straight through the already existing rudiments of the intervertebral ligament like a delicate filament" (p. 77).

Bischoff, in his various works, shows that the earliest changes in the vertebral column of mammals are the same as in birds.

"In like manner as in the *Mammalia* and birds, the ribs in the *Chelonia* grow out as simple rays from the neural arches (*Bogenschenkeln*) of the vertebræ, quite close to their bodies. Very close to the places where they have arisen, however, the ribs of birds and mammals send, as a rule, a process downwards and inwards, which increases more or less in length and thickness, enlarges somewhat at its free end, and becomes closely applied thereby to one, or two, bodies of vertebræ, by the intermediation of the articular capsule which now becomes formed. This process is the neck and head of the rib."—Rathke, *Schildkröten*, p. 97.

"The cervical transverse processes of birds and *Mammalia* attain their forked form in quite a different manner from the ribs, namely, by the coalescence at one end, of what are properly two transverse processes which have grown out of the vertebra, while, on the other hand, a rib has become forked, because, though originally a perfectly simple ray, it has sent out a secondary process from one of its ends."

It results from the observations which have just been detailed, that with certain real or apparent exceptions which have been duly noted, there is a very great uniformity in the mode of development of the vertebral column in all *Vertebrata*.

The primary processes up to, and inclusive of, segmentation or division into somatomes, appear to be the same in all; and there is every reason to believe that the somatomes become differentiated in the same general way *. There seems to be no difference, save in degree, in the chondrifi-

* The relations of the ganglion to the rudiment of the rib and neural arch and segment of the dorsal muscles in the mouse's embryo are the same as in that of the bird.

cation which takes place in the immediate neighbourhood of the notochord and in the neural and hæmal arches. The intercentra correspond in fishes, as in *Amphibia* and birds, to the middle of each somatome; and it does not appear that they are, to any appreciable extent, produced by the metamorphosis of the notochord.

When ossification takes place, the diaphysis appears as a ring, or part of a ring, in immediate contact with, or very close proximity to, the notochord, which it usually embraces completely, though in the rare case of some *Amphibia*, only partially. The diaphysis then increases inwards so as to constrict the notochord, and outwards, so as to invade the centrum more and more. A distinct ossification is commonly formed in each neural arch, and one or more others in each hæmal arch.

In the higher or abranchiate oviparous *Vertebrata* there would seem to be no other centres of ossification in the vertebra than the five just mentioned, except those of the terminal epiphyses. In fishes, on the other hand, a distinct centre—which might be termed the *paraphysis*—is occasionally found in the paraphysial portion of the centrum.

The dorsal vertebræ of a young carp exemplify this structure remarkably well. The diaphysis is represented by an annular osseous ring, which surrounds the notochord and gives off a vertical median process or plate, and two inferolateral plates, which unite the hollow bony cones into which the osseous ring dilates in front and behind. The ossified neurapophyses expand into wedge-like lower ends, which embrace the vertical plate of the diaphysis, and whose apices come into contact with its annular part.

A thick cuneiform mass is interposed between the base of the neurapophysis and the inferolateral plate of the diaphysis on each side. The outer surface forms part of the general contour of the vertebra, and is not produced into a distinct process, though it represents a parapophysis, and gives attachment to the broad head of the distinctly ossified rib. The outer half of the mass is ossified as a distinct paraphysis; the inner in the young carp is still cartilaginous. In the adult the whole wedge-like paraphysial portion of the centrum is ossified; but, instead of becoming united with the diaphysis and neurapophysis, it is anchylosed with the rib, and seems to form its head. In the pike, the paraphysis, more or less produced into a parapophysis, remains distinct from both rib and diaphysis, and the latter occupies a very much larger share of the whole vertebra.

As I have said above, no distinct ossific centre appears to be developed in the paraphysial region of the centrum in any of the abranchiate *Vertebrata*; but it becomes ossified partly by the encroachment of the neurapophysial, and partly by that of the diaphysial, ossifications.

These two ossifications may coalesce so as to leave no trace of their primitive distinctness, as in *Ophidia, Lacertilia,* and birds; or as in mammals, *Crocodilia, Chelonia,* and many extinct reptiles, they may remain for a long time, or permanently, separated by a suture, which may be termed the "*neurocentral suture.*"

It is very commonly assumed that this neurocentral suture is a sort of

morphological landmark, and that it always indicates the boundary between the neurapophysis on the one hand, and the diaphysis or osseous centrum on the other; so that any process which is given off from the vertebra above the suture, is supposed to arise from the neurapophysis, while those given off below it only, are said to arise from the centrum.

It is only necessary to cite a few facts, which may be readily verified, however, to show that the neurocentral suture is of no value as a test of the nature of the parts above and below it.

In man and in the pig, the heads of the ribs, whether dorsal or cervical, are (as Retzius has well pointed out) articulated above the neurocentral suture; and therefore, if we accept the ordinary definition, not to the centrum at all, but to the neurapophysis. Furthermore, if we accept the ordinary view, the "inferior transverse processes" in the neck of these animals are not parapophyses, but second diapophyses, inasmuch as they arise from the neurapophyses, and not from the centrum.

The "transverse processes" of the lumbar vertebræ are usually given off above the neurocentral suture, and are therefore called "diapophyses." In a young Dugong, in the Museum of the Royal College of Surgeons, I find that, in the two hinder lumbar vertebræ, these transverse processes are given off below the neurocentral suture.

In the *Echidna* the head of every rib is attached to the centrum, or below the neurocentral suture; and in the neck, this suture lies between the upper and lower transverse processes.

Thus, if we follow out logically the view that the neurocentral suture indicates the boundary between the neurapophyses and the centrum, and if we accept the current definition of diapophyses and parapophyses, we arrive at the conclusion, that, in the cervical region, man and the pig have vertebræ with two diapophyses and no parapophysis, while the monotreme has a parapophysis and a diapophysis on each side; that, in the dorsal region, the ribs in man and the pig are connected only with the neurapophyses, while in the monotreme they articulate only with the centra; that the transverse processes of the anterior lumbar vertebræ of the dugong are diapophyses, while those of the posterior ones are parapophyses!

The crocodile and some extinct reptiles, such as the *Ichthyosaurus*, whose ribs are throughout attached to the centra, afford still more striking instances of the confusion which would be produced by taking the neurocentral suture as a morphological boundary.

Müller has argued that it is a distinctive character of fishes to have the ribs attached to the centra of the vertebræ or to parapophyses, and his views have been adopted by other anatomists; but the ribs of the *Ichthyosaurus* and of the *Echidna* are as completely and solely attached to their centra as those of any fish; so that if we merely take the facts furnished by anatomy, the doctrine that there is anything peculiarly piscine in the attachment of the ribs to the centra only, falls to the ground.

But it may be urged that the connexion of the head of the rib with the centrum, in mammals and the higher *Vertebrata*, is secondary, that with the diapophysis being the primary and essential one. This is a very

widely current doctrine, and it is sanctioned by Rathke, as we have seen in the passages quoted above, though they are not quite consistent with one another. It is with great hesitation that I venture to contravene the distinct statements of so eminent and accurate an embryologist as Rathke, but my own observations lead me to precisely the opposite conclusions.

In the spinal column of embryos of the mouse 7-8ths of an inch long, for instance, I find that the posterior dorsal vertebræ (A. fig. 10) have no diapophyses, and that the ribs have no tubercles, but that their heads pass directly into the cartilaginous substance of the centrum. Further forwards (B, C) both diapophysis and tubercle become more and more developed, until at length they come into contact and articulate in the ordinary way. Finally, in the cervical region (D) the rib and the diapophysis are, even in this early stage, confluent.

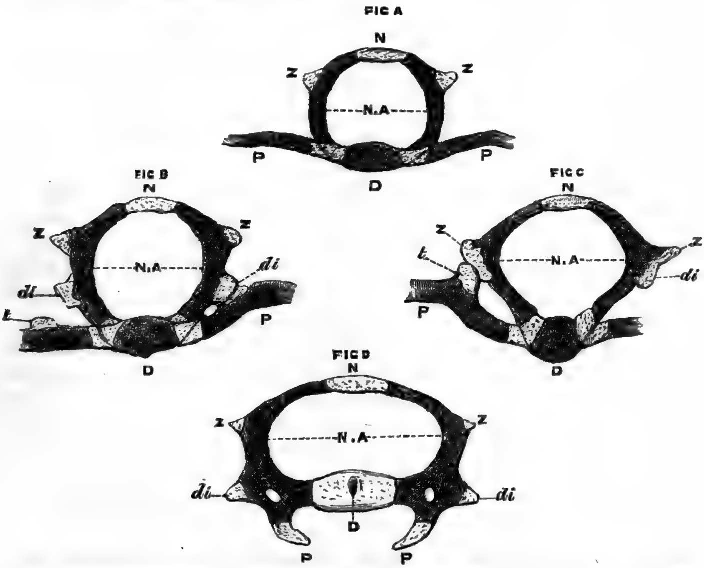


Fig. 10.—Vertebræ of an embryonic mouse $\frac{7}{8}$ths of an inch long. A. The penultimate dorsal vertebra. B. A middle dorsal; and C′. an anterior dorsal vertebra. D. A posterior cervical vertebra. N. Metaneurapophysis or neural spine. N.A. Neurapophyses. Z. Zygapophysis. *di.* Diapophysis. P. Rib or pleurapophysis. *t.* Its tubercle. D. Diaphysis. The ossified parts are shaded, the cartilaginous, dotted.

These facts are, I conceive, wholly at variance with the supposition that the primary connexion of the ribs is with the diapophyses. On the other hand, they teach that the ribs in the mammal are, as in the fish, primarily continuous with the centra of the vertebræ—a result which is in perfect accordance with the ordinary embryological relations of the higher and lower animals.

The hæmapophysial cartilage passes off from the paraphysial cartilage

on the one hand, just as the neurapophysial cartilage is given off from it on the other.

If the hæmapophysial cartilage becomes divided into rib and process, at some little distance from the centrum, the rib is said to be attached to a parapophysis; and I believe that the only consistent definition that can be given of a parapophysis is, that it is developed from the proximal end of a hæmapophysial arch.

If the paraphysis is merged in the general body of the vertebra, and the rib becomes distinctly articulated close to it, the attachment of the rib is said to be to the centrum.

If the paraphysis is bent upwards, so as to pass insensibly in direction into the neurapophysis, and becomes ossified in continuity with the latter, the head of the rib is said to be attached to the neurapophysis; although, in truth, the head of the rib, or at any rate the proximal end of the hæmapophysial arch of which that rib is a part, always retains, so far as we have any evidence, its primitive connexion with its vertebral centrum, into whatever new ones it may enter.

If the neurocentral suture does not define the lower limits of a neurapophysis, and if the true definition of a parapophysis is that given above, it is obvious that our nomenclature of the parts of the dorsal and lumbar vertebræ throughout the vertebrate series, requires a thorough revision.

To this subject I hope to return on a future occasion.

VII.—I subjoin the views of Vogt, and the criticisms of the late great anatomist Johannes Müller upon them, as the best means of exhibiting their relation to those I have advocated.

"If we ask ourselves what we mean by vertebræ, the primary segments of the still indifferent tissue round the chorda, which arise in all vertebrate embryos, are the first things to suggest themselves. These persist only in the lowest grades of vertebrate animals, while in the higher they disappear, in consequence of the more and more complete development of secondary organs, especially of the extremities; so far as we are able to trace these segments, so far is there a formation of vertebræ.

"But there is at once a difficulty, when we endeavour to find these segments in the rudiment of the skull of any vertebrate embryo. It is true that many inflexions may be observed which appear to correspond with such vertebræ, but unfortunately these do not appear in the same places in different embryos; and besides, these inflexions and curvatures of the base of the skull are not in the least similar to the sharply and clearly defined intervals between the primary vertebræ. The first of these intervals is always formed behind the auditory vesicles, and lies therefore between the occiput and the first cervical vertebra; further forwards, as has been said, no such interval is discoverable. But in the Cyclostome fishes, which represent this embryonic condition, no vertebral divisions of the skull are discernible; in fact we have in the Myxinoids, only the chorda with its sheath and muscular and cutaneous vertebral rings, which are repeated up to the skull, but there cease. The skull of the Myxinoids, like that of the higher cartilaginous fishes, cannot by any amount of violence be forced under the vertebrate type. In the skull, then, the

primary vertebral segments are wanting. However, they might be obliterated by the early development of the organs of sense, or by the aberrant development of the brain.

"But there remains a second means of discovering the cranial vertebræ, by examining the solid cartilaginous and bony basis of the skull; though here also we meet with insuperable difficulties. As the primitive type of the more solid bodies of the vertebræ, we have everywhere cartilaginous rings arising out of the sheath of the chorda, and deposited around its nucleus. Whether they arise as lateral halves or as entire rings, whether they embrace the chorda completely or only above or below, is a matter of no essential moment. But are such cartilaginous rings deposited around the chorda, discoverable in the skull? They will be sought for in vain unless it be in the last, occipital, cranial vertebra; in this we still find all the characters of a vertebra—the investment of the chorda, the chondrification in the sheath of the chorda. But the chorda does not pass into the so-called first and second cranial vertebræ; it invariably ends, as Rathke justly states, between the auditory capsules, and never passes into the body of the second cranial vertebra, let alone that of the first. The lateral cranial trabeculæ, which bear the two anterior cranial vertebræ, can by no possibility be regarded as centra of vertebræ, since in this case, the characteristic feature, the being traversed by the chorda, is entirely absent. Again, these lateral trabeculæ are continued uninterruptedly forwards, below the first division of the brain, showing no trace of a median division. But in what part of the vertebral column has it ever been seen that two vertebræ arise united and afterwards divide?

"It has therefore become my distinct persuasion that the occipital vertebra is indeed a true vertebra, but that everything which lies before it *is not fashioned upon the vertebrate type at all*, and that all efforts to interpret it in such a way are vain; that therefore, if we except that vertebra (occipital), which ends the spinal column anteriorly, there are no cranial vertebræ at all."—Vogt, *Entw. d. Geburtshelferkröte*, pp. 98–100.

"Vogt, and in the present work Agassiz also, contest the justice of the theory of the composition of the skull of several vertebræ, and will only admit an occipital vertebra, because the embryonic chorda, according to Vogt's investigations, extends no further in the skulls of fishes and *Amphibia*. In this, in my opinion, too much stress is laid upon a single result of embryological investigation. That, however, the chorda in the frog's larva extends beyond the base of the occiput, further than where the slight trace of the basioccipital is ultimately formed, I have myself seen. Even the anterior part of the vertebral column of the Rays shows that the chordal system, out of which, according to my own and Vogt's observations, only the central part of the fishes' vertebra proceeds, may be abortive, whilst the cortical part of the vertebra, which arises in quite a different way, is at its maximum of development. In a longitudinal section of the anterior part of the vertebral column of a Ray, it is seen that the central parts of the vertebræ, in the axis of the vertebral column, or those parts which are developed from the chordal sheath alone, become finer and finer anteriorly (although the column still exhibits vertebral divisions), and

at last cease entirely, without reaching the anterior end of the vertebral column. On the other hand, *Branchiostoma lubricum* shows us the opposite extreme; the chorda passes beyond the anterior end of the skull, beyond the mouth and the eyes, far into the extremest end of the snout.

"This remarkable fact, first observed by Sundevall, was very surprising to me, since in consequence of my studies up till that time, I regarded the existence of three vertebræ in the proper cerebral cranium as certain, at least I considered the assumption of a fourth ethmoidal vertebra to be uncertain and undemonstrated.

"For now I saw at once, that it was undoubtedly possible that the cephalic vertebral column might extend further forwards. There need not always be three cranial vertebræ developed in the head; in birds, reptiles, and fishes, the most anterior vertebra is abortive, and is even entirely wanting in some families; but, in the *Mammalia* and man, three cranial vertebræ are without exception discoverable in the basis cranii, either in the fœtus, or in many cases even in young or middle-aged animals—the occipitale basilare, sphenoideum basilare, posterius and anterius; these also occur in fish. How far the chorda primitively extends in *Mammalia* is not yet made out; but even although it should not reach through the whole basis cranii, this, from the reasons which have been stated, would be no good argument."—Joh. Müller, *Bericht.* cclxviii–ix., Müller's *Archiv*, 1843.

"Report of the Joint Committee of the Royal Society and the British Association, for procuring a continuance of the Magnetic and Meteorological Observatories." With Appendix, containing Letter of General Sabine to the Committee. Communicated by order of the President and Council.

At the Meeting of the British Association which was held at Dublin, in August 1857, a resolution was adopted, proposing the continuance of the system of magnetical observations which was commenced under the auspices of the Royal Society, and of the British Association, in 1840; and a Committee, consisting of the President of the Association, the Rev. Dr. Robinson, and Major-General Sabine, was appointed, to request the cooperation of the President and Council of the Royal Society in the endeavour to attain this object, and to take, in conjunction with them, such steps as may appear desirable for that end.

The Committee thus appointed accordingly held a meeting in London, on the 5th of November last, at which it was agreed to recom-

mend that hourly observations, for not more than five years, should be undertaken at certain stations in the British Colonies; and a letter was addressed to the President of the Royal Society, asking for his cooperation, and that of the Council of the Society, in endeavouring to attain that object.

This application was favourably received by the Council of the Royal Society; and on the 10th of December, 1857, the following resolution was adopted in reference to it:—

"That Sir John Herschel, the Astronomer Royal, the Dean of Ely, and Dr. Whewell, be appointed a Committee, to cooperate with the Committee appointed with this view by the British Association, and to take, in conjunction with them, such steps as may be necessary, including, if it be thought desirable, an application to Government."

In consequence of this resolution, a correspondence took place among the members of the two Committees, which having resulted, it is believed, in a general agreement as to the course to be adopted, the joint Committee so acting in cooperation met at Leeds on the 24th of September, and in the first instance proceeded to inquire into the nature and scientific value of the results which have already been secured by the system of observation hitherto carried out, at the observatories maintained by the Government, at the joint recommendation of these two bodies, with a view to forming a distinct opinion whether they are such as to merit being regarded as a reasonable, and, what may be called, a remunerative return for the labour and thought bestowed upon them, and the very considerable expenditure of the public money incurred by them. In so doing, they have limited their views to the results, as compared with the expenditure, in the British Colonial Magnetic Observatories only, without taking into consideration those deducible from observations made under foreign auspices; and they find that, at the cost of an expenditure which may be reckoned at about £400 per annum (exclusive of the cost of instruments, outfit, and publication) for each of the several observatories at St. Helena, Toronto, Hobarton, and the Cape of Good Hope during the respective continuance of each, the accumulated observations, so far as they have yet been discussed, have produced the following results, which they consider as satisfactorily established by the discussion:—

In the first place, the mean state of the several magnetic elements for each of the stations, as reduced to a fixed epoch, has been obtained with a precision of which nothing previously done has afforded any example—emulating, in this respect, the exactness of astronomical determinations, and competent to serve as a fixed point of departure to the latest ages; and this for each of the elements in question—the dip, the declination, and the intensity of the magnetic force.

Secondly, that at each station, the rate of regularly progressive secular change in all the three elements above mentioned has been ascertained with a degree of precision which contrasts strongly with the loose and inaccurate determinations of former times.

Thirdly, that the laws of the diurnal, annual, and other periodic fluctuations in the values of these elements, as exhibited at each station, have been established in a manner and with a decision to which nothing hitherto executed in any branch of science, astronomy excepted, is comparable; and that the results embodied in the examination of these laws have laid open a view of magnetic action so singular, and so utterly unexpected, as to amount to the creation of a new department of science, and the detection of a completely novel system of physical relations: for that, in the first place, the systems of diurnal and annual magnetic changes have each been separated into two perfectly distinct and physically independent systems,—the one, at any particular station, holding its course according to laws depending solely on the sun's hour-angle at the moment of observation, and his meridian altitude at different seasons,—the other, comprehending all those movements which, under the name of magnetic storms, or "irregular disturbances," have hitherto presented the perplexing aspect of phenomena purely casual, capricious in amount and in the particular occasions of their occurrence when regarded singly, has been shown, by these discussions, to be subject in its totality to laws equally definite with the others, though more dependent for their application on peculiarities of local situation. As regards the first of these systems of fluctuation, they find it demonstrated:—

That the sun's regular action on the magnetism of the globe is determined by a law of no small complexity and intricacy, but which, nevertheless, has been traced with precision and certainty, and shown to be referable, in the first place, and for one of its arbitrary coefficients, to the geographical situation of the place of observation with

respect to a certain line or equator on the earth's surface, which cannot yet be precisely traced for want of sufficiently numerous stations (but which seems to approach to the line of least intensity, and is very far from coinciding with the geographical equator),—and in the next, and for its other influential cause, to the fact of the sun's having north or south declination; so that the whole diurnal change in any one of the elements, and at any station, is made up of two portions, one of which retains the same sign, and a constant coefficient all the year round; the other changes sign, and varies in the value of its coefficient with the annual movement of the sun from one side of the equator to the other.

That, consequently, for a station on the magnetic equator (so defined), the *mean* amount of diurnal change is *nil*, when taken over the whole year, but that on any particular day in the year it has a determinate magnitude, which passes through an annual periodicity, with opposite characters in opposite seasons. And that for a station in middle latitudes the mean diurnal fluctuation is not *nil*, but such as during every part of the year to exhibit an easterly deviation in the morning hours, and a westerly in the evening hours, for stations north of the magnetic equator, and *vice versâ* for those south of it; but that the amount of this deviation, or the amplitude of the diurnal fluctuation, varies with the seasons, being exaggerated or partially counteracted by the alternate conspiring and opposing influence of the sun's declination during the summer and winter seasons.

As regards the irregular disturbances, though arbitrary and capricious in extent and in the moments when they may be expected, individually, they nevertheless obey, with great fidelity, the law of averages when grouped in masses, and treated separately from those of the former class. So handled they are found to conform in their average effect, at each of the twenty-four hours of the day, and on each day of the year, to the very same rules as regards the sun's daily and annual movement, with one remarkable point of difference, viz. that their hours of maxima and minima are not identical with those of the regular class, but that each particular station has, in this respect, its own peculiar hours, analogous to what is called the "establishment" of a port in the theory of the tides. And that in consequence, the superposition of these two systems of diurnal fluctuation gives rise to a series of compound variations analogous to the

superposition of two undulations having the same period but different amplitudes, and different epochal times; and that by attending to this principle, many of the most complex phenomena, such as that of a double maximum and minimum, with the occurrence of a nightly as well as a daily movement, are explained in a satisfactory manner.

The discussion of the observations already accumulated has further brought into view, and in the opinion of your Committee fully established, the existence of a very extraordinary periodicity in the extent of fluctuation of all the magnetic elements, and in the amplitude and frequency of their irregular movements especially, which connects them directly with the physical constitution of the sun, and with the periodical greater or less prevalence of spots on its surface—the maxima of the amount of fluctuation corresponding to the maxima of the spots, and these again with those of the exhibitions of the Aurora Borealis, which appears also to be subject to the same law of periodicity; a law, which as it does not agree with any of the otherwise known solar, lunar, or planetary periods, may be considered as, so to speak, personal to the sun itself. And thus we find ourselves landed in a system of cosmical relations, in which both the sun and the earth, and probably the whole planetary system, are implicated.

That the sun acts in influencing the earth's magnetism in some other manner than by its heat, seems to be rendered very probable by several features of this inquiry, and the idea of a direct magnetic influence exterior to the earth, is corroborated by the discovery of a minute fluctuation in the magnetic elements, having for its period not the solar but the lunar day, and therefore directly traceable to the action of the moon. The detection of this fluctuation by M. Kreil, from a discussion of the Prague observations, has been confirmed by the evidence afforded by those of our Colonial observatories, and appears to be placed beyond all question by the recent deductions for the horizontal force and the declination extending over three years of observation at the Cape of Good Hope, which General Sabine has submitted for your Committee's inspection, and in both which the fluctuations in question emerge in a very satisfactory manner, and one calculated to give a high idea of the precision of which such determinations are susceptible, when it is considered that the total amplitude of oscillation due to this cause in the direction of the Cape-needle is only about 16″ of angle.

Your Committee, looking at this long catalogue of distinct and positive conclusions already obtained, feel themselves fully borne out in considering that the operation in a scientific point of view has proved so far eminently remunerative and successful, and that its results have fully equalled in importance and value, as real accessions to our knowledge, any anticipations which could reasonably have been formed at the commencement of the inquiry.

Having satisfied themselves of the great and important value of the results already obtained, independent of the dormant interest as respects future discussion which the mass of observations accumulated continues to possess, and which it remains for future theoretical combinations to elicit,—your Committee next turned their attention to the question, whether and to what extent the maintenance of some or all of the old Colonial observatories, or the establishment of new ones for a limited term might be expected,—first to give additional certainty and precision to the determinations already obtained,—and secondly, to elucidate points imperfectly made out, and more especially geographical relations which determine the greater or less amount of discordance between the epochal hours of the regular and irregular diurnal changes—relations which no doubt involve the causes of the irregular fluctuations themselves (causes at present involved in the greatest obscurity),—and to obtain indications of the points in the earth's surface at which the forces producing them originate.

As regards the general question as to the desirableness of some continuation of the observations, it seems hardly to be referred to our consideration as a Committee—the resolutions came to both by the British Association and by the Council of the Royal Society, in the appointment of their respective Committees of cooperation, indicating an opinion already conclusively formed on the part of both bodies to that effect. They have felt it due to themselves, however, to come to an independent conclusion on that point, and having done so with perfect unanimity, on the grounds already adduced, and the expectations for the future which those grounds justify, they next address themselves to the consideration of the two points above indicated, and to the important questions—first, whether to recommend the continuance or resumption of the establishments at the former

stations, or the selection of new ones; and secondly, with how *few* new or revived establishments, with how limited a scale as to extent and expense, and with how short a period as to the minimum term of their duration, the expectation of these advantages being secured could be compatible, and finally to fix upon the stations most desirable.

As regards the first point referred to, viz. the more complete establishment of the laws themselves, and the giving of greater numerical precision to their expression, the Committee are of opinion that the laws themselves are not likely to be subverted or contradicted by a larger series of observations at any station for which they have once been shown to prevail; but that every new station differing much in geographical situation from the former, in which they might be found verified, with or without supplementary modifications, would undoubtedly add strength to the induction by which they have been concluded. Additional numerical precision on the other hand would only be attained by a continuance of observations at former stations, and is not a point of sufficient importance, in their opinion, to be entitled to any weight in opposition to considerations in favour of change,—while in the one important case in which such additional precision is especially desirable,—that of the solar period,—such additional precision will be acquired ultimately as a matter of course by continued observation at any one of the existing permanent observatories, of whose business magnetic observation forms a part as well as by any amount of Colonial establishments.

It is therefore mainly in the elucidation of obscure and difficult physical points, and in the probable extension of our knowledge of the geographical and other conditions on which the irregular disturbances depend, that our hope of advantage from further observation consists; our conviction being that, without special observations at well-selected stations (selected, that is, with a view to these objects), there is little or no prospect of further progress. The *general character* of the magnetic phenomena may be considered as secured from loss; but the great problem remains unresolved,—the local influences are yet to trace, and the only means of tracing them must consist in varying the position of our stations, so as to embrace great differences in geographical situation, and in conformity with such indications as can be gathered from our present experience. The magnetic esta-

blishments permanently existing in Europe and America are confessedly inadequate to afford the requisite information. The stations which have occurred to your Committee as most eligible, would be, Vancouver Island, Newfoundland, the Falkland Isles, Bermuda, Ceylon, Shanghai or some locality in China, and Mauritius; but they are fully aware that to demand from the national purse the institution of observations at all these points, would be more than is warranted by any pressing necessity, and ought therefore not to be insisted on. Among them, the principal in point of interest (for reasons which will be presently mentioned), are Vancouver Island, Newfoundland, the Falkland Isles, and Pekin or some near adjacent Chinese station; and the Committee consider that much valuable information would accrue from observations sufficiently prolonged at these, to which, therefore, they would be understood to limit their recommendation. In regard to the length of time over which they would desire to see the observations extended, they consider five years (being about half the solar period, and as being also sufficient to give a fair grasp of the secular change of the magnetic elements) as a period both in consonance with that which has been accorded on former occasions, and in some sort designated by the nature of the case.

The reasons which induce them to give a preference to these over the rest of the stations enumerated are as follows:—Between Toronto and Point Barrow the difference of the epochal hours of the irregular diurnal fluctuations is such as to amount to a complete opposition of phases, a circumstance which goes far to point out the latter station as being in the immediate neighbourhood of the origin of those irregular disturbances. Should observations be established at the two stations now proposed, there is every reason to hope, as will appear from a document drawn up at the request of the Committee by General Sabine, and with his permission appended to this Report, that the observations at Toronto, which have been partially re-established since 1855, would, with a view to cooperation for this especial purpose, be wholly resumed on a fitting application to the Colonial legislature. And, in addition to this, should an application, made to the Norwegian Government for the establishment, during the same period, of an observatory at the North Cape prove, successful (which there is every reason to hope, such an application made on a former occasion having been well received, and having ultimately failed owing

only to a want of attention to some point of diplomatic form in its mode of communication), we should then have a chain of stations in high northern latitudes, the results obtained at which being severally brought into comparison with those already procured at Point Barrow, and with each other, could hardly fail of bringing out some very positive conclusion.

As regards the proposal for a station at the Falkland Isles, it is presumed, from the general course of the magnetic line of minimum intensity, that this station will prove, in analogy to the Cape of Good Hope, and in contrast with the northern stations recommended, to have the character of an equatorial, or approximate equatorial, station; and in respect to that proposed in China, that it will complete and carry round the globe the chain of northern middle latitude stations, —the intermediate links being supplied by the Russian observatories, and by those which it is hoped may be established at the North Cape and at Toronto. As regards the Falkland Isles and Newfoundland, it should be noticed that there exist considerable facilities and conveniences for the comfortable establishment of an observatory there; and in respect of the other two, it may be remarked, that they are both points of great present interest, and that a determination of the meteorological as well as magnetic peculiarities of both would be important. The affections of a telegraphic wire by electric discharges in the nature of Aurora Borealis have already attracted attention, and produced confusion in the ordinary use of such wires, and constitute one of the motives for inquiry into the nature and laws of the so-called magnetic storms. It may also be observed that, in reference to the anomalistic equation of the sun's magnetic intensity, or the effect of its annual approach and recess due to the ellipticity of the earth's orbit, the influence of local temperature upon the observations requires to be eliminated, in order to bring this effect into evidence, by a combination of the results obtained at stations whose seasons are opposite.

In reference to the important consideration of keeping down as much as possible the outlay consequent on the establishment of these observatories, your Committee have given attention to the question whether it be desirable to continue, as heretofore, the printing of the observations *in extenso*—a measure resulting in the production of vast and costly volumes, and entailing a great amount of laborious super-

intendence. They consider that the form of the observations remaining unaltered, and the principles of their reduction being now rendered familiar, this would not be necessary, provided the original observations were registered in triplicate, and the copies separately deposited in different and secure custody for preservation and occasional reference when required, and provided that sufficient and well-digested abstracts of their reduced results were published. One series of observations, however, they consider must be excepted from this alteration of system,—those of a continuous nature, made on term days, FOUR of which per annum they desire to see still kept up,—and those taken on occasions of magnetic storms, when continuous observation is substituted for that on the regular hourly intervals; for the treatment of such observations is still a matter of scientific inquiry; and to render them available, in comparison with others, the complete register is indispensable.

Your Committee cannot but contemplate a revival of active interest and cooperative participation in the system of observation on the part of our Colonial and of Foreign Governments, when once it shall become known that the subject is resumed by our own Home Government in the manner recommended. On this subject they beg to refer to General Sabine's reply to their inquiries, already alluded to, which places in a distinct point of view the expectations which may justly be indulged on that score. In reference, moreover, to the personal and material establishment at each of the Government observatories, this document contains a summary of what is needed, and of what ought to be applied for.

And this leads your Committee to a point which they consider of such importance to the success of the whole proceeding, that they cannot help embodying their opinion on it in this Report. It is of little avail to accumulate observations unless their effective and complete reduction be provided for, and the assurance obtained that when reduced they will undergo such discussion and scientific treatment as shall elicit from them the laws of the phenomena of which they are the records. The zeal and ability with which the present Superintendent of the Government Magnetic and Meteorological Observatories has hitherto executed this task, if extended to the new series now called for, would afford that assurance in its fullest extent; and they earnestly trust that this will not be lost sight of in the

arrangements to be made in carrying out their proposals, if adopted.

There is another point to which your Committee consider their attention ought to be paid simultaneously with the establishment of the proposed observatories: it is that of the extension of Magnetic Surveys of the districts in their immediate neighbourhood, with a view to fixing the situation and direction of the iso-magnetic curves within some considerable adjoining area (in the case of the Falkland Isles—that of the whole group).

On this point the following remarks by General Sabine, in a communication addressed by him to us in reply to certain inquiries which we considered it right to make of him, are in the opinion of your Committee, conclusive in deciding them to recommend that provision be also made for the execution of such surveys, collaterally with the observations at the fixed stations.

"Recent observations in North America, discussed in the 'Proceedings of the Royal Society' for January 7th, 1858, have made known that the general movement of translation of the isoclinal and isogonic lines, which from the earliest observations have been progressing from west to east, has within a few years reached its extreme eastern oscillation, and that the movement in the reverse direction has already commenced; we live therefore at an epoch in the history of terrestrial magnetism, which we have reason to believe will be regarded hereafter—when theory shall have more advanced—as a highly important and critical epoch. The geographical position of the maximum force in the Northern Hemisphere appears to have reached its extreme easterly elongation, and from this time forth may be expected to move for many years to come towards the meridian which it occupied in Halley's time, accompanied by a corresponding change in the positions and forms of the isodynamic, isoclinal, and isogonic lines in North America: a careful determination of the absolute values and present secular change of the three elements at this critical theoretical epoch, at stations situated on either side of the American continent, and nearly in the geographical latitude of the maximum of the force, would furnish therefore data for posterity, of the value of which we may have a very inadequate appreciation at present. I may refer to the discussion prefixed to the third volume of the Toronto Observations, to show that the means and methods with which

we are conversant are adequate for the purpose ; and I may indicate Vancouver Island and Newfoundland as colonies well-suited for establishments of the same nature as those of which the efficiency has been proved."

As regards the instrumental means to be employed, the Committee believe that the consideration of the subject would be more fitly undertaken by the Royal Society, who will probably think it right to appoint a special Committee, as was done on the former occasion, to consider it maturely, and to report upon it. Well as in general the instruments employed in the British Colonial Observatories have performed, it may be desirable to consider whether they could not be improved, by diminishing considerably the size of the magnetic bars employed. Small bars indicate more certainly the rapid magnetic changes; they may be hardened more perfectly, and therefore vary less in their magnetic condition with changes of temperature; they admit of more perfect protection from the effects of disturbing aërial currents; and, finally, the instruments may be constructed at less expense, and may be grouped together in a smaller and less costly building.

The joint Committee therefore have finally agreed to the following resolutions, which they submit for approval to their respective appointing bodies:—

1. That it is highly desirable that a series of Magnetical and Meteorological observations, on the same plan as those which have been already carried on in the Colonial Observatories for that purpose, under the direction of Her Majesty's Board of Ordnance, be obtained, to extend over a period of not more than five years, at the following stations:—

1. Vancouver Island.
2. Newfoundland.
3. The Falkland Isles.
4. Pekin, or some near adjacent station.

2. That an application be made to Her Majesty's Government, to obtain the establishment of observatories at these stations for the above-mentioned term, on a personal and material footing, and under the same superintendence as in the observatories (now discontinued) at Toronto, St. Helena, and Van Diemen Island.

3. That the observations at the observatories now recommended,

should be comparable with, and in continuation of, those made at the last-named observatories, including four days of term observations annually.

4. That provision be also requested at the hands of Her Majesty's Government, for the execution, within the period embraced by the observations, of magnetic surveys in the districts immediately adjacent to those stations, viz. of the whole of Vancouver Island and the shores of the strait separating it from the main land,—of the Falkland Isles,—and of the immediate neighbourhood of the Chinese observatory (if practicable), wherever situated,—on the plan of the surveys already executed in the British possessions in North America, and in the Indian Archipelago.

5. That a sum of £350 per annum, during the continuance of the observations, be recommended to be placed by Government at the disposal of the General Superintendent, for the purpose of procuring a special and scientific verification and exact correspondence of the magnetical and meteorological instruments, both of those which shall be furnished to the several observatories, and of those which, during the continuance of the observations for the period in question, shall be brought into comparison with them, either at Foreign or Colonial stations.

6. That the printing of the observations *in extenso* be discontinued, but that provision be made for their printing in abstract, with discussion; but that the term observations, and those to be made on the occurrence of magnetic storms, be still printed *in extenso*; and that the registry of the observations be made in triplicate, one copy to be preserved in the office of the General Superintendent, one to be presented to the Royal Society, and one to the Royal Observatory at Greenwich, for conservation and future reference.

7. That measures be adopted for taking advantage of whatever disposition may exist on the part of our Colonial Governments, to establish observatories of the same kind, or otherwise to cooperate with the proposed system of observation.

8. That in placing these resolutions and the Report of the Committee before the President and Council of the Royal Society, the continued cooperation of that Society be requested in whatever ulterior measures may be requisite.

9. That the President of the British Association be requested to act in conjunction with the President of the Royal Society, and with the Members of the two Committees, in any steps which may appear necessary for the accomplishment of the objects above stated.

10. That an early communication be made of this procedure to His Royal Highness The Prince Consort, the President elect of the British Association for the ensuing year.

Appendix.

Letter from General Sabine to the Committee.

St. Leonard's, June 26, 1858.

My dear Sir,—You wish me to state for the consideration of the Committee, what *specific* measures for the continuance and extension of magnetical researches appear to me suitable to the present state of that branch of science, and at the same time sufficiently moderate and reasonable to justify the expectation that the portion of them which requires it may receive the sanction of Her Majesty's Government.

For this purpose it may be convenient to divide the subject generally under three heads, and to consider separately what may be expected,—

1st. From our own Government.
2nd. From our own Colonies.
3rd. From foreign Countries.

1st. *From our own Government.*

The establishment, for a limited period, of observatories in the three colonies,—

Vancouver Island,
Newfoundland,
The Falkland Islands,

on a similar plan to those which were established at Toronto, St. Helena, and Hobarton, and which have now ceased, having accomplished their objects. The "personnel" at each of these three observatories to consist of an officer, four non-commissioned officers, and one private, either of the Artillery or of the Engineers, with the same extra pay and allowance for incidentals as was the case in the observatories which have terminated. The instruments, both absolute and differential, to be of the same description as before, with of

course such modifications and improvements as experience has suggested. The instruments to be prepared at Kew, and the directors of the three observatories to be instructed there. The system of observation to be hourly; Sundays, Christmas days, and Good Fridays excepted. The time to be employed to be mean astronomical time at the station, both for magnetism and meteorology. The observatories to be maintained until five complete years of observation are obtained. The number of term-days in each year to be reduced from twelve to four.

The public departments whose sanction will be required, are, the Treasury for the expense, and the General Commanding in Chief for the selection and appointment of the officers and non-commissioned officers. In addition to the officer for each observatory, a fourth officer will be required as an assistant to the General Superintendent, in carrying on, under his direction, the details of correspondence with the observatories. Total, four officers, twelve non-commissioned officers, and three privates.

2nd. *With reference to our own Colonies.*

As soon as the sanction of Government has been obtained for the observatories already named, the Governors of British Guiana, Mauritius, and Melbourne may be written to, suggesting that a communication should be addressed from each of those colonies to the Secretary of State for the Colonies, expressing the desire which is felt in the colony to participate in the proposed systematic researches, stating what facilities can be afforded, and what portion of the expense can be borne by the colony itself, and requesting to be placed in official communication with some suitable authority for the preparation of instruments, and for furnishing such instructions and advice as may be required.

The accompanying letter from Lieutenant Governor Walker, of British Guiana, to M. Sandeman (which has been just forwarded to me), will show how ready that colony is to take its part, and that it waits only for that measure of countenance and encouragement which it reasonably expects, and ought to receive, from the mother country. At Mauritius, a Meteorological Society, formed by the colonists themselves, is most actively and usefully employed in tracing out, by means of the logs of merchant vessels, the phenomena of the storms by which navigation in that vicinity is troubled; and is making most

pressing appeals for an authorization which should enable them to add researches in magnetism to those in meteorology; having on the spot, in Captain Fyers, of the Royal Engineers, a person who would make an admirable director of such an establishment. At Melbourne, a proposition for a magnetic observatory and survey is now before the local government; means are abundant, but instruments and direction are wanting. Mr. Jeffery, so well trained in the Hobarton Observatory, is in that country, and is desirous of such employment, as M. Neumayer is of the survey. Melbourne would be a most important station for a magnetic observatory, as we might expect from it the verification or otherwise of the increase in the magnetism of the earth, at the period of the year when she makes her nearest approach to the sun.

Other colonies might follow the example; but in regard to these three, we are justified in expecting that, with suitable measures of encouragement, we should have thoroughly efficient establishments, carrying out the system of observation in its completeness.

3rd. *With reference to Foreign Countries.*

A proposition has recently been made to the Netherlands Government, by Dr. Buys Ballot (who fills the same official position in Holland that Admiral FitzRoy does in this country), for the establishment of a magnetic observatory in the Dutch Colony of Batavia; and Dr. Buys Ballot has written to inquire whether, in the event of the proposition being acceded to, two sets of instruments, one for Batavia and the other for Utrecht, could be prepared at Kew. An intimation that the British Government was about to resume and extend its magnetical researches, might be expected to have a favourable influence on the success of Dr. Buys Ballot's proposition, and might also lead to the adoption of our colonial system of observation in its full extent at the Batavian observatory.

The importance of the North Cape of Europe as a magnetical station, especially with reference to the connexion between the Aurora and the magnetic disturbances, has already been noticed in a preceding letter from me (that of May 13). The instruments which were prepared some years ago at the expense of the Royal Society, and were intended to be presented to the observatory at the North Cape (should one be established there by the Norwegian Government), are still in existence, and with a few modifications

might be applied to their original purpose. The re-establishment of magnetic observatories in the British Colonies might furnish a favourable occasion for reviving, in concert with M. Hansteen, a proposition for an observatory at the North Cape, which seems to have failed on the former occasion rather from an accident than from any real difficulty in the matter itself.

M. Secchi, of the Observatory of the Collegio Romano at Rome, has recently been supplied from Kew, at the expense of the Papal Government, with a complete equipment of magnetical instruments, similar to those which have done such good work in the British Colonial establishments. With the encouragement derived from the revival of active measures here, M. Secchi might hope to obtain the aid which he desires, in the way of temporary assistance, to enable him to carry out the complete system of observation, for which he is already provided with the instrumental means.

The concert which has prevailed between Russia and England in magnetic operations, gives reason to hope that renewed activity here might so far strengthen M. Kupffer's hands, as to enable him to carry out efficiently the hourly system of the three elements, at one at least of the stations in Eastern Siberia, the probable importance of which can scarcely be overrated.

A meteorological observatory has recently been instituted at Havanna, and the director, M. Poey, has proposed to the Cuban authorities the purchase of magnetical instruments, to be prepared at Kew, and a sufficient increase of assistants to provide for observations to be made with them. M. Poey is active and intelligent, and has recently visited the principal magnetic institutions in Europe. He would not fail to avail himself of the support which his proposition would derive from the measures which might be taken here.

The Regents of the Smithsonian Institution at Washington, have agreed to allot a portion of their funds to a magnetic observatory; but neither the instruments nor the system of observation have yet been determined: their decision might probably be hastened by the knowledge that active measures are in progress here.

Viewing the necessity of resorting to means of securing and ascertaining a precise correspondence between the magnetical and meteorological instruments which may come to be used in these operations, or in cooperation with them in other quarters, as well as their exact

and scientific adjustment, as also of securing a self-registered series of photographic delineations of the solar spots during its continuance —it is proposed that during the continuance of the observatories, an annual sum, not exceeding £350, should be taken on the estimate, and placed at the disposal of the General Superintendent for these purposes.

There is a point referred to in a former letter which will require the attention of the Committee. It is the question, whether the observations of the proposed observatories should be printed *in extenso*, or in abstract accompanied by a discussion of the principal results.

I remain, my dear Sir, faithfully yours,

EDWARD SABINE.

Sir John Herschel, Bart.

Papers read.

I. "On the Changes produced in the proportion of the Red Corpuscles of the Blood by the administration of Cod-Liver Oil." By Theophilus Thompson, M.D., F.R.S. Received April 30, 1858.

Influenced by a conviction that the peculiarities essential to any disease are often associated with characteristic changes in the blood, and that the efficacy of many remedies depends on their power of modifying such conditions of the blood, I have made it an object to take opportunities of ascertaining some particulars regarding the composition of that fluid in certain diseased conditions prior to the use of remedies, and also in patients affected with similar maladies, but whose symptoms under the employment of medicines have been materially ameliorated. In reference to the inquiry, the proportion of red corpuscles would appear to be a circumstance of special significance, and to this question I have directed my chief care. In pursuing the investigation, my attention has been particularly directed to cases of phthisis, as there was in this way afforded to me the greatest opportunity of comparing the largest amount of analogous instances.

On the 27th of April, 1854, I had the honour of presenting to the

Royal Society a short communication descriptive of the changes produced in the blood by the administration of cod-liver oil and cocoa-nut oil, and advanced the conclusion deduced from chemical analysis, that any favourable result derived from the administration of these oils is associated with an increase in the proportion of red corpuscles. Having availed myself of opportunities for continuing the investigation, I beg to present the results to the Society.

The chief particulars observed are exhibited in a tabular form, but it may be desirable briefly to detail the most important circumstances in some of the instances adduced.

In two of the patients no oil had been given; both were men, the subjects of advanced consumption. In one of them, Edward D., the proportion of red corpuscles was only 98·20 in 1000 of blood. In the other, D. D., similarly affected, it was 119·64.

When cod-liver oil was administered, but failed to produce any favourable effect on the general condition of the patient, a similar spareness in the proportion of blood-corpuscles was observable. Thus, for example, James H., a man affected with phthisis in the third stage, had taken the oil for four months, but notwithstanding gradually lost strength and weight. His proportion of blood-corpuscles was 114·39, still less than in D. D., a patient to whom no oil had been administered.

With these cases it is instructive to contrast others in the same period of disease, to whom cod-liver oil had been administered with manifest advantage.

Sarah Warren, aged 26, with a consumptive cavity in each lung, under a few months' course of the oil gained $10\frac{1}{2}$ pounds in weight, and in other respects materially improved. The proportion of corpuscles was found on analysis to be 145·68.

In Edwin P., aged 28, affected with a similar condition of both lungs, who improved considerably and gained 4 pounds in weight, under two months' continuance of the oil treatment, the red corpuscles existed in nearly the same proportion as in the female last mentioned, being 145·56.

Thomas N., a patient with a consumptive cavity (indicated amongst other signs by cracked pipkin sound on the left side), having taken the oil for some months, gained 13 pounds in weight. The proportion of red discs was 157·78. It may be interesting to

mention, that in this individual, the fibrine, although considerable in amount, was not associated with fat, and that the corpuscles of his blood examined under the microscope exhibited numerous chains or rouleaux.

The results of analysis in less advanced stages of the disease are still more striking.

Sarah M., a girl aged 16, having the peculiar clicking sound, and other evidences of the second stage—that of softening tubercle (on the left side)—had taken cod-liver oil for six months with most satisfactory results. The corpuscles were in remarkably high proportion, namely 172·56. It is an interesting fact, that in this instance the remarkable hum in the jugular veins, often found as a symptom in anæmia (but as I think wrongly regarded as peculiar to that condition), was present.

In Henry B., the subject of phthisis in the second stage on both sides, after a short course of oil, the proportion was 144·76.

The highest proportion of corpuscles which I have found in any of my consumptive patients was 174·34. It occurred in S. P., a man aged 36, who had the disease in a very early stage accompanied with the variety of wavy inspiration, which I believe to indicate the existence of a considerable amount of freely expanding lung around the parts consolidated by tubercular deposit. He had taken oil for only two months, but had during that period gained about a stone in weight.

Another man, Thos. C., also in the first stage, and with the same symptom, wavy inspiration, but with a greater extent of dull percussion (and other signs of more disease than S. P.), had also taken oil for two months with advantage, and the proportion of corpuscles was 144·45, whilst Thos. B., in whom the physical signs of disease were still slighter, but in whose expectoration, examined with the microscope, the presence of lung-tissue sufficiently demonstrated the nature of the disease, the blood contained a proportion of corpuscles as high as 165·90. This man had taken cod-liver oil (with only one short interruption) for twelve months.

The fourteen cases recorded in this communication with a view to the question under consideration are arranged in the following Table :—

No.	Name.	Stage of disease.	Time during which oil was given.	Gain or loss in weight.	Proportion of red corpuscles.
				lbs.	
1.	Edward D....	3rd.	None given.	......	98·20
2.	David D....	3rd.	None.	......	119·64
3.	James H. ...	3rd.	4 months.	− 7	114·39
4.	Sarah Warren	3rd, on both sides	5 months.	+10½	145·68
5.	Edwin P. ...	3rd, do. do.	6 weeks.	+ 4	145·56
6.	Thomas N....	3rd.	Some months.	+13	157·78
7.	Sarah M. ...	2nd.	6 months.	+ 3	172·56
8.	Henry B. ...	2nd, on both sides	14 days.	No change.	144·76
9.	George P. ...	1st.	7 weeks.	+13	174·34
10.	Thomas C. ...	1st.	2 months.	+ 6	144·45
11.	Thomas B. ...	1st.	12 months.	......	165·90
12.	Martha W....	1st.	3 weeks.	......	142·62
13.	Mary D.......	3rd.	4 months.	+10	84·83
14.	Sarah W......	3rd.	Some months, ozonized oil.	+ 6	162·07

The analyses were made by Mr. Dugald Campbell, Analytical Chemist to the Consumption Hospital, and were conducted as follows:—

The blood was placed in a small beaker half-full, fitted with a cork, and was conveyed carefully so as to keep the serum free from the blood-corpuscles. The beaker containing the blood, after standing twenty-four hours, was weighed.

The serum, perfectly free from red corpuscles, was drained from the coagulum with very great care, and the latter being transferred on to bibulous paper, after standing for the space of four or five hours, was weighed. Two separate portions of coagulum, of from 35 to 40 grains, were again weighed, one for the moisture, and the other for the fibrine. The moisture was then entirely removed by means of a water-oven; from 24 to 36 hours being found necessary for accomplishing this object, the completion of which was ascertained by the capsule, with its contents, ceasing to lose weight. The portion for the fibrine was removed into a bason, treated with cold water by maceration, until the fibrine appeared to be perfectly colourless, when it was removed on to a previously weighed filter, again washed, and carefully but not too highly dried. The filter and fibrine were next put into a test-tube, and, in order to separate the fat, digested, first with ether and again with alcohol, twice or thrice, then dried by means of a water-bath, and weighed. This weight, less that of the filter,

represents the fibrine; and these data being obtained, the amount of blood-corpuscles, and that of fibrine in 1000 parts of the blood, are determined by a simple calculation.

The general correspondence between the instances adduced in the present communication and those of my former contribution is sufficiently obvious; the proportion of red corpuscles in patients to whom oil had been successfully administered exceeding that ascertained to exist in the first stage of the disease, in those to whom this medicine had not been given. It is commonly stated, and the remark is in harmony with my own observations, that the proportion of red corpuscles is usually less in women than in men. To this rule the seventh case in the Table furnishes an exception, and it was further remarkable from the fact that a murmur could be heard in the jugular vein; a phenomenon commonly attributed to spareness of blood-corpuscles, but in this instance associated with more than the average amount.

The case numbered thirteen in the tabular analyses would seem directly opposed to the conclusion to which the other observations tend, but the patient had suffered from repeated attacks of spitting of blood so extreme as to place her life in jeopardy. This profuse hæmorrhage would naturally increase the poverty of the circulating fluid, and thus counteract to a great extent the apparent influence of the remedy.

In the fourteenth case ozonized oil had been administered.

The rapid reproduction of red corpuscles implied in these observations, suggests inquiries of special interest; but I purposely abstain from any attempt to explain the mode by which it is effected.

II. "Further Observations on the Power exercised by the Actiniæ of our Shores in killing their prey." In a Letter to W. Bowman, Esq., F.R.S., dated Oct. 25, 1858. By R. M'Donnell, M.D. Communicated by Mr. Bowman. Received Oct. 27, 1858.

Dear Sir,—In the course of last winter I had the honour, through your kindness, of making a communication to the Royal Society "On the Power exercised by the Actiniæ of our Shores in killing their prey;" allow me now, through the same medium, to correct the

view which I was at that time led to adopt, that this power is due to electrical influence.

In the communication alluded to, the idea of these creatures being electrical, was based on the fact, that when the nerve of a frog's limb, prepared after the manner of Matteucci's galvanoscopic frog, is seized by the tentacles of an actinia, contractions of the muscles promptly ensue. It was admitted, however, that all attempts to produce deflection of the galvanometer-needle had failed, and this being the very doubtful state of the question, I ventured to look forward to the pleasure of making another communication on the subject when I had had further opportunities of examining the Actiniæ in health and vigour.

I have now had these opportunities, and have found that the most delicate electrometers are unaffected by these animals, and I conceive that by the following simple, and indeed obvious experiments, all idea of the Anemones of our coasts being electrical may be set aside.

A galvanoscopic frog's limb having been prepared, with the nerve as long as possible, it is laid on a piece of perfectly clear glass, so that the nerve hangs over the edge. The pendent nerve is lowered into the water containing an *Anthea*, and the nerve is brought in contact with *a single one* of the long tentacles of this creature; immediately vigorous contractions follow in the muscles of the limb, and if everything be left undisturbed, these twitchings will continue for some minutes after the nerve is withdrawn.

If, however, a thread be tied round the nerve, below the point where the tentacle of the Anthea had touched it, all twitchings at once cease. If the portion touched by the tentacle be snipped off, all twitchings also cease. Having thus repeatedly observed that contact between the nerve and a single tentacle was followed by muscular contractions, which at once ceased as soon as the portion of the nerve which had been in contact with the tentacle was removed, it occurred to me to try the effect of applying to the nerve a single tentacle removed from the body of an Anthea. I therefore had recourse to the following experiment:—The hind leg of a frog is separated from the body, the sciatic nerve dissected out carefully, so that the nerve be not crushed or injured, and the thigh cut away. The limb with the nerve thus dissected out as long as

possible, is to be laid on a plate of clean glass; a silk thread is tied round the base of one of the tentacles of an Anthea, and the tentacle snipped off. The mere tentacle separated from the animal to which it belonged is drawn gently across the nerve, or laid upon it, at the upper part: immediately muscular contractions follow in the leg. These contractions cease at once if the portion of the nerve touched by the tentacle be cut off. There can, it seems, no longer be any doubt that the muscular contractions are excited, not by electricity, but by irritant action of the urticating organs of the Anthea, which being more powerful in this respect than other Anemones, has been chosen for experiment, although other varieties give similar results.

I now see I was in error in supposing that the effect produced on the frog's limb by the Actiniæ could be transmitted along a wire. I presume that in preparing the experiment alluded to, which I performed in the open air, at the sea-side, some of the irritant materials of the Anemones, which I had possibly handled, had been brought by my fingers in contact with the nerves, and I was thus deceived.

I am very happy, however, that I am myself the first to perceive and correct this error.

I remain, &c.,

ROBERT M'DONNELL.

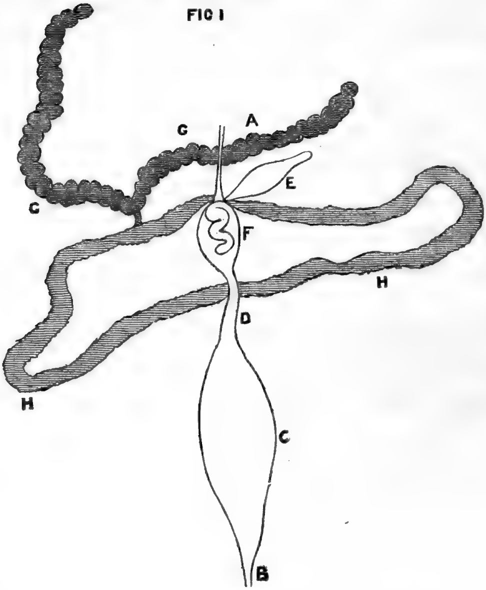
W. Bowman, Esq., F.R.S., &c. &c.

III. "On the Digestive and Nervous Systems of *Coccus hesperidum*." By JOHN LUBBOCK, Esq., F.R.S., F.L.S., F.G.S. Received Oct. 4, 1858.

In the early part of last spring I began to investigate the anatomy of this interesting little insect, with the intention of studying only the organs connected with the development of the ova and pseudova. It soon, however, became evident that the structure of the intestinal canal, on the one hand, had been entirely misunderstood by those who had previously examined it; and on the other, that the nervous system, far from being similar in all specimens, varied in the most extraordinary manner. It is therefore proposed in the present communication to give a very brief description of the digestive organs and of the nervous system.

Intestinal Canal.
(Magnified 30 diameters.)

FIG I



Ramdohr and Leydig are the only two naturalists, so far as I know, who have published any original remarks on this subject.

Ramdohr says, "Die Speiseröhre kurz und enge. Der Magen vorn ein wenig erweitert, lang und völlig durchsichtig, so dass man die dunkeln Contenta darin sieht. . . . . Der Dunndarm ist leer, etwas weiter als der Magen, durchsichtig, bisweilen faltig. . . . Die Gallgefässe fehlen, wenigstens konnte ich nicht die geringste Spur davon entdecken." This description, however, has reference to *Chermes Alni*.

According to Leydig (Zeitschr. f. Wiss. Zool. V. TAB. I. fig. 1), the canal in *Coccus hesperidum* consists of a short œsophagus, a large stomach, and a long intestine. Into this intestine open four glands. Rather behind the middle of it are situated the two large, yellow hepatic glands, and in front of these open, on one side, a free, slightly curved cæcum, and on the other, a shorter cæcum coiled up and enclosed in a pyriform sac, which is continued into a tube, whose end is attached to the skin. This description is a singular mixture of truth and error, and Professor Leydig is so careful an observer that it

was long before I could convince myself that he had made such a series of mistakes. His descriptions of the separate parts are indeed correct (though in my specimens the hepatic glands (G, G) were proportionally larger than in his figure), but he has entirely misunderstood the relations of the different organs.

The true œsophagus (fig. 1 A) is rather long and extremely narrow. It corresponds, I believe, to the tube *f* in Leydig's figure, which he considers as an appendage to the intestine. Following the œsophagus comes the pear-shaped bag (fig. 1 F), with its remarkable cellular, contorted, internal gland. Then there is a very short intestine (D, ilium) opening into the rectum (C), which Leydig has described as the stomach. The rectum is often found filled with fluid, as Leydig figures it, and varies in shape in different specimens; it contracts at its posterior end into a narrow tube, B (the œsophagus of Leydig), which opens into the vent on the upper side of the body.

At the anterior end of the pear-shaped crop or stomach are attached, besides the œsophagus, the two ends of the recurrent intestine (H), and the cæcum (E), which is generally swollen at its base, and is perhaps the equivalent of the sucking stomach.

The recurrent intestine is considered by Burmeister and Lacordaire to be part of the ventriculus, but in all insects the Malpighian vessels open into the duodenum, or, when this is wanting, into the ilium, close behind the pylorus; and as in the Homoptera they are attached to the recurrent intestine, it seems improper to consider this as part of the ventriculus. If the recurrent intestine be cut, a number of large cells, some with daughter-cells, exude from it.

According to Burmeister, the Malpighian vessels are never less than four in number; and according to Lacordaire, when there are only two, they are always attached by both ends; but in *C. hesperidum* there are but two, and they are attached only at one end.

It seems to me evident that M. Leydig must have detached the whole canal from the skin, and, in doing so, ruptured the recurrent intestine. In this case it would be very natural for him to regard the free end of the longer part as the anus. The large rectum he has evidently mistaken for the stomach, and the vent for the mouth. There would then remain the œsophagus, which he has correctly described as going to the skin.

I have repeatedly dissected out the intestinal canal without rupturing the recurrent intestine; and it may be observed that the structure of the whole digestive organs, as now described, is in accordance with that of the other Homoptera, which would not be the case if M. Leydig is correct.

M. Ramdohr examined *C. Alni*, but his description can hardly be correct, since it is scarcely possible that nearly allied species can differ so entirely in the arrangement of such important organs. Unless *Coccus Alni* does differ very much from *C. hesperidum*, he has made the same mistakes as M. Leydig, with the addition of having misunderstood or overlooked the hepatic glands, which perhaps he may have mistaken for ovaries.

The intestinal canal of *C. persicæ* is formed on the same type as that of *C. hesperidum*.

Nervous System.

I do not propose to give a detailed account of the nervous system, and only allude to it in order to mention the great variations observed in different specimens. Figs. 2–9 represent different forms of the nervous system in *C. hesperidum*, and fig. 10 that of *C. persicæ*: in all the objects are magnified 60 diameters.

Leydig rightly describes the subœsophageal portion of the ganglionic column as being reduced to a large mass (fig. 2, &c. A), situated close behind the mouth. This ganglion generally emits, besides the commissure, three large nerves on each side, and its hinder extremity is continued into a still larger nervous column (C), which passes backward for rather more than ·014 of an inch without throwing off any branches. It then divides, and after a while each of the divisions again subdivides, so as to give off a rich plexus of nerves to the posterior part of the body.

The posterior pair of nerves (fig. 2, &c.) always throws off on its inner side, and not very far from its origin, a nerve (F) which I once traced and found to unite with one of the nerves derived from the main central chord. This nerve (F) is always present, but the point at which it leaves the main nerve (B) is very variable, being sometimes as much as ·014 of an inch from the subœsophageal ganglion, sometimes quite close to it. Indeed, in more than one instance it arose from the ganglion itself, and not from the nerve B (fig. 3).

FIG. 2

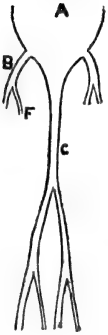


FIG. 3

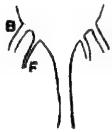


FIG. 4

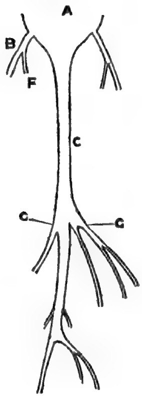


FIG. 6

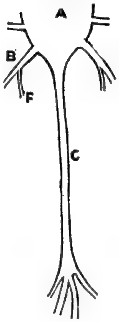


FIG. 5

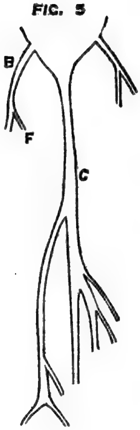


FIG. 7

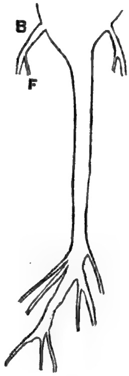


FIG. 9

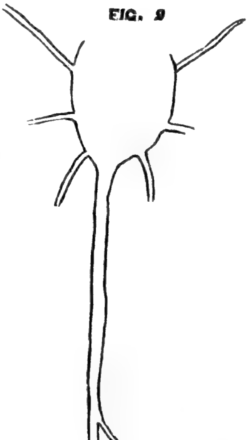
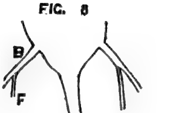


FIG. 10

FIG. 8



In the divisions also of the central stem there are very great variations, which it would be endless to describe in detail. Perhaps the arrangement most generally met with, and that which I am inclined to regard as the type, on account of its presenting the nearest approach to symmetry, is that the main central chord separates, at about ·014 from its origin, into two equal branches, and these again, after a course of about ·01, divide dichotomously (fig. 2). In such a case the division of F from B generally takes place at a considerable distance from the ganglion.

I have, however, not met with many specimens presenting even this very limited amount of symmetry and regularity.

In fig. 4 we see the two divisions (G, G) of the central chord C divide almost immediately and yet not symmetrically. In fig. 5 the chord C divides into two unequal divisions, the smaller of which passes along for more than ·014 before it redivides, while the larger branch divides into three at a point only ·006 from its origin. In fig. 6 the central chord, just before its division into two branches, throws off on each side a small branchlet; in fig. 7 this happens only on one side. Finally, figs. 4 and 7 present us with some instances in which more than four branches are given off close to the first division of the great chord C.

But even in the case which I have above described as most typical, the symmetry is not in fact so great as it would at first sight appear to be, because the nerves on the two sides are frequently not of the same size. Thus, in fig. 2 each of the two branches of the main central stem divides, it is true, into two secondary branches, one of which is smaller than the other, but the two lesser branches are both upon the right side. If then, as is probable, we are justified in concluding that in each animal the ultimate nervous fibrils are of somewhat equal size, that they compose the greater part of the nerve, and that the corresponding organs of the two sides of the body receive an equal amount of nerves, it is evident that some of the parts which on the left side are supplied by the large outer branch must on the right side be connected with the median branch.

We see, therefore, that not only is the branching of the nerves absolutely irregular, and that of the two sides entirely unsymmetrical, but even the number of main stems proceeding from the

ganglion is not always the same. This result has surprised me very much, since if any organs might have been expected to be almost invariable, I should have thought it would have been the nervous system. I believe that no parallel case has been described, nor do I even remember to have seen a description of any variation occurring in the larger nerves of any animal whatsoever. Considering, however, how great are the variations which occur here in the same species, it is evident that differences in the distribution of the nerves in nearly allied forms are in themselves no *proof* that such species were separately created.

Around the ganglionic masses are several large spherical bodies. These appear to be homologous with the "Zellenkörper," described by Leuckart as surrounding the supraœsophageal ganglion in the larva of *Melophagus*. He considers them also as homologous with similar organs which have been observed in the embryos of other insects by Heroldt and Kölliker*.

Dujardin (Ann. des Sci. Nat. 1850, 3rd sér. vol. xiv. p. 202) describes the supraœsophageal ganglion of the worker-ants as consisting of several isolated parts, and I was at first inclined to consider these spherical bodies as also merely isolated parts of the ganglionic mass, in favour of which view it may be urged that fewer nerves than usual appear to proceed from this mass. The contents of the spherical bodies, however, under the influence of reagents, present an appearance different from that of the supraœsophageal mass.

The subœsophageal ganglion is very richly supplied with tracheæ, derived from two large stems which are attached to the front angles, and ramify from thence all over the mass.

The supraœsophageal ganglion is a triangular mass with its apex behind. The two front corners terminate in large nerves.

The nervous system of *C. persicæ* differs but little from that of *C. hesperidum*, and offers the same extraordinary amount of variation. The two species, however, could be at once distinguished by the superior size of the subœsophageal ganglion in *C. persicæ*, in which species also the last pair of nerves (fig. 10) is given off more posteriorly, while both they and the central stem are considerably swollen at their origin, so as to give the hind margin of the ganglion a three-pronged outline.

* Die Fortpflanzung und Entwickelung der Pupiparen. Halle, 1858.

November 25, 1858.

W. R. GROVE, Esq., V.P., in the Chair.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read, as follows:—

President.—Sir Benjamin Collins Brodie, Bart.

Treasurer.—Major-General Sabine, R.A.

Secretaries.— { William Sharpey, M.D. / George Gabriel Stokes, Esq., M.A.

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—Henry Wentworth Dyke Acland, M.D.; Rear-Admiral Sir George Back, D.C.L.; The Rev. John Barlow, M.A.; Thomas Bell, Esq., Pres. L.S.; His Grace the Duke of Devonshire, M.A.; Edward Frankland, Ph.D.; John Peter Gassiot, Esq.; Philip Hardwick, Esq., R.A.; Arthur Henfrey, Esq.; Lieut.-Colonel Henry James, R.E.; Sir Roderick Impey Murchison, M.A., D.C.L.; John Percy, M.D.; Archibald Smith, Esq., M.A.; The Rev. William Whewell, D.D.; Charles Wheatstone, Esq.; The Lord Wrottesley, M.A.

Captain Boxer, R.A., was admitted into the Society.

Robert W. Bunsen, Louis Poinsot, and Carl Theodor von Siebold, having been severally balloted for, were elected Foreign Members of the Society.

The following communications were read:—

I. "Researches on the Phosphorus-Bases."—No. III. Phosphoretted Ureas. By A. W. Hofmann, Ph.D., F.R.S.

Received November 18, 1858.

The existence in the nitrogen-series of a well-defined group of diamines of the formula

$$\left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} N_2,$$

rendered it probable that the continued study of the phosphorus-,

arsenic-, and antimony-bases would lead to the discovery of the corresponding terms,

$$\left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}P_2,\qquad \left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}As_2 \text{ and } \left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}Sb_2,$$

which might be designated as diphosphines, diarsines, and distibines; and the further prosecution of this line of thought very naturally suggested the idea of searching for a group of intermediate compounds containing simultaneously nitrogen and phosphorus, nitrogen and arsenic, phosphorus and arsenic, &c., compounds expressed by the formulæ

$$\left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}NP;\qquad \left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}N\,As;\qquad \left.\begin{matrix}A_2\\B_2\\C_2\end{matrix}\right\}P\,As, \text{ \&c.},$$

which might be termed phosphamines, arsamines, phospharsines, &c.

Among the several processes likely to furnish this result, none appeared more promising than the reaction between a monamine and a monophosphine of opposite chemical characters.

In the conception of this idea, I have studied the deportment of cyanic acid and some of its derivatives with phosphoretted hydrogen and its homologues, in the hope of producing combinations similar in constitution to the ureas, but differing from these substances by containing phosphorus in the place of one equivalent of nitrogen.

The action of cyanate and sulphocyanide of phenyl, an account of which I have lately* submitted to the Royal Society, upon triethylphosphine, seemed to include the conditions for the realization of such compounds.

On bringing cyanate of phenyl in contact with triethylphosphine, a most lively reaction ensues; the mixture begins to boil, and the phosphorus-base is apt to be inflamed. On cooling, the liquid solidifies into a crystalline mass, which is insoluble in water, soluble in alcohol and ether, and crystallizes from the latter solvent in beautiful little square tables, tasteless, inodorous, and infusible at 100° C. On submitting this compound to analysis, I was surprised to find that it contained no phosphorus, and that it exhibited the composition of the original cyanate of phenyl, from which it differs so essentially in its properties. This substance is the *cyanurate of phenyl*, generated from the cyanate by simple transposition of the elements. The

* Proceedings, vol. ix. p. 274.

triethylphosphine participates only indirectly in the reaction; in giving rise to the transformation of the cyanate, the phosphorous body plays the part of a ferment, a comparison which is moreover suggested by the large proportion of cyanate over which the influence of a minute quantity of phosphorus-base extends. A glass rod moistened with triethylphosphine solidifies, almost instantaneously, a considerable quantity of the cyanate. The transformation of the cyanate under the influence of triethylphosphine, into cyanurate, although the principal phase of the reaction, is attended by other changes which I intend to examine more minutely by and by.

Very different results were obtained by substituting for the cyanate the sulphocyanide of phenyl. The reaction between this body and triethylphosphine is very violent, and frequently gives rise to the inflammation of the phosphorus-base. The mixture assumes a deep yellow colour, and often deposits splendid yellow needles on cooling; frequently, however, it remains liquid for hours and even for days, but suddenly solidifies, when touched with a glass rod, into a hard, yellow, crystalline mass. This substance is insoluble in water; it dissolves with the greatest facility in alcohol, hot or cold, likewise in warm, less so in cold ether. Recrystallization from boiling ether affords, in fact, the best means of procuring the new body in a state of purity. This end is likewise considerably facilitated, by allowing the sulphocyanide of phenyl to act upon the triethylphosphine in the presence of a considerable quantity of ether.

In the pure state the new compound presents itself in the form of well-defined prisms of uranium-yellow colour, which fuse at 61° C. They cannot be heated much beyond their fusing-point without being altered; at 100° C. they are entirely decomposed, evolving a most peculiar odour, which is also observed on evaporating the ethereal mother-liquor.

The new compound possesses the characters of a well-defined base. Quite insoluble in water, it dissolves in the most dilute acids, forming with some of them, such as hydrochloric and hydrobromic acid, beautifully crystallized saline compounds. From these salts the base may be separated again by cautiously adding either potassa or ammonia. The hydrochloric solution of the base yields with dichloride of platinum a yellow crystalline precipitate, sparingly soluble in water, insoluble in alcohol and ether.

Analysis of the yellow crystals, dried over sulphuric acid, led to the formula

$$C_{26}\,H_{20}\,N\,PS_2,$$

which shows that they are formed by the simple union of the two substances placed in contact:

$$\underbrace{C_{14}\,H_5\,NS_2}_{\text{Sulphocyanide of phenyl.}}+\underbrace{C_{12}\,H_{15}\,P}_{\text{Triethyl-phosphine.}}=\underbrace{C_{26}\,H_{20}\,N\,PS_2}_{\text{New compound.}}.$$

If we consider urea as a diamine derived from diammonia by the substitution of the diatomic molecule carbonyl $(C_2\,O_2)''$ for 2 equivs. of hydrogen,

$$\text{Urea } C_2\,H_4\,N\,O_2=\left.\begin{matrix}(C_2\,O_2)''\\ H_2\\ H_2\end{matrix}\right\}N_2,$$

—the simplest perhaps of the many views brought forward regarding the constitution of urea,—the new substance, which formation as well as chemical deportment essentially characterize as an analogue of urea, may be represented by the following formula:—

$$C_{26}\,H_{20}\,N\,PS_2=\left.\begin{matrix}(C_2\,S_2)''\\ (C_4\,H_5)_2\\ (C_{12}\,H_5)(C_4\,H_5)\end{matrix}\right\}NP;$$

that is, urea, the oxygen of which is replaced by sulphur, the hydrogen by ethyl and phenyl, and lastly, half the nitrogen by phosphorus.

The formation of this compound presents considerable interest, not only as an illustration of the remarkable persistence of the type urea, but also as furnishing the first unequivocal instance of the formation of ureas containing no longer any unreplaced hydrogen, the existence of which had as yet remained doubtful.

The new urea forms, as I have stated, a series of well-defined beautifully crystallized salts. Its solution in warm hydrochloric acid solidifies, on cooling, into a crystalline mass, which, when recrystallized from warm water, is obtained in splendid needles of a bright cadmium-yellow colour, often several inches in length. They are decomposed at 100° C., and must therefore be dried over sulphuric acid *in vacuo*. Analysis proved them to contain

$$C_{26}\,H_{20}\,N\,PS_2,\ HCl.$$

The solution of this salt yields with dichloride of platinum a bright yellow precipitate, which under the microscope is found to consist of

small lily-shaped crystals. Dried over sulphuric acid *in vacuo* it contains

$$C_{26}\,H_{20}\,N\,PS_2,\ HCl,\ Pt\,Cl_2.$$

The hydrochlorate yields also a precipitate with trichloride of gold; the salt is, however, rapidly blackened.

The hydrobromate, both in preparation and properties, resembles the hydrochlorate. Its composition is

$$C_{26}\,H_{20}\,NPS_2,\ HBr.$$

The urea readily combines with iodide of methyl and ethyl. The methyl-compound immediately separates in the crystalline form on mixing an ethereal solution of the urea with iodide of methyl; it is soluble in water, and crystallizes from a boiling solution in beautiful golden-yellow needles, containing

$$C_{26}\,H_{20}\,N\,PS_2,\ C_2\,H_3\,I.$$

The iodide, by the action of chloride of silver, may be converted into the chloride; this yields with dichloride of platinum a fine needle-formed salt, which may be recrystallized without decomposition. The formula of this platinum-salt is

$$C_{26}\,H_{20}\,N\,PS_2,\ C_2\,H_3\,Cl,\ Pt,\ Cl_2.$$

When treated with oxide of silver, the iodide furnishes a powerfully alkaline liquid, probably the corresponding base

$$\left.\left[(C_2\,H_3)\,\left((C_2\,S_2)''\,(C_4\,H_5)_3\,(C_{12}\,H_5)\,\underset{H}{N}\,P\right)\right]\right\}O_2.$$

Scarcely separated, however, this substance decomposes with liberation of sulphocyanide of phenyl, the oxide of methyl-triethylphosphonium remaining in solution. This salt is sufficiently characterized by the readily crystallizable octahedral platinum-salt.

I have not been able to obtain either the sulphate or the nitrate of the urea, probably on account of the great instability of the new substance.

On dissolving the base, even in dilute nitric acid, it is immediately decomposed with separation of sulphocyanide of phenyl, the triethylphosphine being oxidized. The same change is observed when one of the more stable salts, such as the hydrochlorate, is dissolved in a large quantity of water; the liquid soon becomes turbid from the elimination of oily globules of sulphocyanide of phenyl, and now contains the hydrochlorate of the phosphorus-base.

On adding ammonia to a salt of the urea, similar phenomena are

observed. From a concentrated solution, the base is separated without change; but when dilute and hot solutions are employed, the turbidity at first produced disappears, and after a few minutes beautiful crystals of phenyl-sulphocarbamide ($C_{14} H_8 N_2 S_2$)* make their appearance; at the same time the odour of triethylphosphine becomes perceptible.

With potassa the deportment is perfectly analogous, but the crystals formed after some time are diphenyl-sulphocarbamide (sulphocarbanilide, $C_{26} H_{12} N_2 S_2$) instead of phenyl-sulphocarbamide.

On adding to an ethereal solution of the urea a few drops of bisulphide of carbon, the liquid, when gently heated, assumes a deep crimson colour, and deposits, on cooling, the beautiful compound $(C_4 H_5)_3 P, C_2 S_4$, which I have described some time ago†. The mother-liquor yields on evaporation oily drops of sulphocyanide of phenyl.

The deportment of triethylphosphine with sulphocyanide of phenyl induced me to investigate the action of this body upon several other sulphocyanides. The substance which at once suggested itself for examination was sulphocyanide of allyl, mustard-oil. This compound reacts most powerfully with the phosphorus-base. On mixing the two bodies, a powerful evolution of heat takes place, and the mixture assumes a deep brown colour, but does not solidify either on cooling or on agitation. After several days' standing, however, very large well-defined crystals are deposited which unfortunately are contaminated with the brown colouring matter of the solution. I have not yet succeeded in getting them perfectly white, and have therefore not analysed them. Their formation, however, and their general characters leave no doubt that they are the corresponding allyl-compound,

$$C_{20} H_{20} N PS_2 = \left.\begin{matrix} (C_2 S_2)'' \\ (C_4 H_5)_2 \\ (C_4 H_5)(C_6 H_5) \end{matrix}\right\} NP.$$

Triethylphosphine has remained in contact with sulphocyanide of ethyl for more than a month without depositing any crystals. *A priori*, however, the formation of an urea under these circumstances was doubtful, since sulphocyanide of ethyl differs from the corresponding phenyl- and allyl-compounds, even in its deportment with ammonia and the monamines.

* Proceedings of the Royal Society, vol. ix. p. 276. † Ibid. p. 290.

In conclusion, it deserves to be mentioned that there appears to exist a similar series of arsenetted ureas. Triethylarsine, when left for some weeks in contact with sulphocyanide of phenyl, deposits small crystals of a body which I believe to be the arsenic-compound corresponding to the phosphorus-urea described in this paper. This body requires a more minute examination.

II. "On the Deflection of the Plumb-line in India caused by the Attraction of the Himalaya Mountains and the elevated regions beyond, and its modification by the compensating effect of a Deficiency of Matter below the Mountain Mass." By the Venerable Archdeacon PRATT. Communicated by Mr. STOKES, Sec. R.S. Received October 25, 1858.

(Abstract.)

The author begins by referring to his former paper, published in the 'Transactions' for 1855, in which he calculated the deflections caused by the mountain mass on the north of Hindostan, at three principal stations of the Great Arc, in the plane of the meridian, viz. Kaliana (lat. 29° 30′ 48″), Kalianpur (24° 7′ 11″), and Damargida (18° 3′ 15″). He made them 27″·853, 11″·968, and 6″·909 (or more correctly, as revised in the present Paper, 27″·978, 12″·047, and 6″·790); and showed that the comparison of these two portions of the arc—which, if it *be* elliptical, and if the amplitudes are accurately known, ought to give the exact ellipticity of the arc in question—gives an ellipticity of $\frac{1}{426}$, instead of the mean $\frac{1}{300}$.

2. He next states that the Astronomer Royal, in a subsequent communication (in 1855), suggests that there is most probably a deficiency of matter immediately below the mountains which will cause a *negative* deflection, and so compensate for the mountain attraction. Three objections are urged against this hypothesis, as stated by Mr. Airy. It requires (1) that the solid crust should be only about ten miles thick; (2) that the crust should be lighter than the lava on which it rests; (3) that wherever there is a protuberance upwards in mountain masses and table-lands, there must be a corresponding projection of the crust downwards into the fluid, which it is difficult to conceive, as the same reason which is used to show it would prove also that, where there are hollows above as in deep

seas, there must be corresponding hollows in the solid crust below filled up by the lava, and this would point out a law of varying thickness in the crust which no process of cooling could well produce.

3. The author considers, however, that if there be a compensating cause it must lie in this direction; and he puts forth the hypothesis of deficiency of matter in a new form. He supposes that the mountain mass has risen up in consequence of a slight expansion of the solid crust below through many miles of thickness, producing a slight attenuation from a considerable depth. He calculates formulæ and reduces them to tables to find the effect of this attenuation, and shows that the mountain attraction, modified by this attenuation, if it extend down through 100 or 300, or 500 or 1000 miles (the attenuation being uniform along each vertical line), will produce the following deflections:—

At Kaliana....	$1''\cdot538$, or	$6''\cdot872$, or	$10''\cdot912$, or	$16''\cdot779$.
„ Kalianpur..	$0''\cdot064$ „	$0''\cdot369$ „	$2''\cdot425$ „	$4''\cdot661$.
„ Damargida	$0''\cdot065$ „	$0''\cdot076$ „	$0''\cdot120$ „	$1''\cdot570$.

4. These four sets of deflections are then applied to correct the amplitudes of the two portions of the arc, and by their comparison to find the ellipticity, which is shown to be, in the four cases,

$$\frac{1}{216}, \frac{1}{280}, \frac{1}{286}, \text{ and } \frac{1}{385}.$$

So that although the hypothesis, if the depth of attenuation be about 100 miles, greatly reduces the deflection, it does not reduce the ellipticity to the mean value, which is attained only if the depth be somewhere between 500 and 1000 miles. There is little or no ground, therefore, for working with a mean ellipticity as is done in the Great Survey.

5. It is next pointed out that this theory will not explain the peculiarities of the Indian Arc under consideration; in which (according to Colonel Everest: see his volume for 1847, p. clxxvii), the upper portion has an excess in its amplitude, geodetically determined, of $5''\cdot236$, and the lower a defect of $3''\cdot789$. The presence of other disturbing causes near Kalianpur or Damargida, or both, is indicated by this; either in *visible* masses above, which ought to be accurately surveyed (as even small masses, if near enough, will produce the effect); or in *invisible* defects or excesses of matter

below, which it is impossible to discover, and therefore to estimate. The possibility, and even not small probability of such existing without our being able to detect and estimate them, throws an air of doubt and uncertainty over all geodetic operations, whenever it is necessary to know with precision the position of the vertical, freed from the influence of local disturbing causes. This *is* necessary for determining the curvature of the arc, that it may be used in both the problems of mapping the country with extreme accuracy, and of ascertaining the form of this part of the earth. A note is appended, illustrating the degree of influence which errors in the verticals and the ellipticities may have on the mapping.

6. The author next applies the results of his paper to ascertain the effect upon the plumb-line of an excess or defect of density, of only 1-100th part above or below the density required by the fluid-theory of equilibrium, and prevailing over wide-spread spaces in the interior of the earth. From the fact that specimens of rocks, even of the same description, found at the surface of the earth, vary considerably in density, he infers that it is not improbable that there may be as wide variations of density among the masses below, in addition to the variations arising from difference of distance from the centre of the earth and required by the fluid-theory of equilibrium. If this be the case, his calculation shows that his fears expressed in the last paragraph are not unfounded. The result of this part of the calculation is expressed in the following Table:—

Table of Deflections caused by a defect or excess of matter throughout a semicubic space of four millions of miles [*i. e.* 200 each way parallel to the surface, and 100 miles in the vertical], the mean density of the excess or defect being 1-100th part of the density of the earth at the depth of the centre of the cubic space.

Depth of the centre of the semi-cubic space.	Distance of the middle point of the space from the station, measured along the chord to the surface,				
	379 miles.	581 miles.	781 miles.	980 miles.	1173 miles.
50 miles.	1″·940	0″·835	0″·457	0″·248	0″·118
150 „	1·621	0·803	0·456	0·252	0·120
250 „	1·383	0·782	0·483	0·272	0·131
350 „	1·067	0·749	0·490	0·286	0·142
450 „	0·663	0·713	0·425	0·277	0·145

If the space be nearer to the station, or if the difference in density be more than 1-100th part, these deflections must be multiplied by a corresponding quantity.

7. The paper is concluded by a revision of some of the calculations in the former communication. The mass of the mountain region above the level of the plains is shown to be somewhat more than four-millionths of the mass of the earth.

III. "On the Thermal Effects of Compressing Fluids." By J. P. Joule, LL.D., F.R.S. &c. Received October 9, 1858.

(Abstract.)

The author in this paper gives an experimental demonstration of the correctness of Professor Thomson's formula, $\theta=\frac{\mathrm{T}ep}{\mathrm{JK}}$, where θ is the thermal effect, T the temperature from absolute zero, e the expansibility by heat, p the pressure, J the mechanical equivalent of the thermal unit, and K the capacity for heat. The fluids experimented on were water and oil, with the results tabulated below:—

	Temperature of the liquid.	Pressure applied in atmospheres.	Experimental result.	Theoretical result.
Water...	$1^{\circ}\cdot2$ Cent.	25·34	$-0^{\circ}\cdot0083$	$-0^{\circ}\cdot0071$
	5	25·34	0·0044	0·0027
	11·69	25·34	0·0205	0·0197
	18·38	25·34	0·0314	0·0340
	30	25·34	0·0544	0·0563
	31·37	15·64	0·0394	0·0353
	40·4	15·64	0·0450	0·0476
Oil	16	7·92	0·0792	0·0886
	17·29	15·64	0·1686	0·1758
	16·27	25·34	0·2663	0·2837

IV. "Note on Archdeacon Pratt's paper on the Effect of Local Attraction on the English Arc." By Captain Clarke, R.E. Communicated by Lieut.-Colonel James, R.E. Received June 30, 1858.

The following letter of Colonel James will explain the nature of this communication; the numerical statements, being not susceptible

of abridgement, are reserved for the Philosophical Transactions, in which the paper will appear.

"Ordnance Survey Office,
Southampton, June 29, 1858.

"In the valuable communication by Archdeacon Pratt 'On the Effect of Local Attraction upon the Plumb-line at Stations on the English Arc of the Meridian between Dunnose and Burleigh Moor,' which is published in part 1. vol. cxlvi. of the Philosophical Transactions, the data for the calculation are taken from vols. ii. and iii. of the Trigonometrical Survey, which were published in 1811. Since the publication of those volumes the triangulation has been extended over the whole of the United Kingdom, new bases have been measured, and all the calculations have been revised; and as considerable errors have been detected in the distances used as data by Archdeacon Pratt, I directed Captain Clarke to substitute the correct distances in the formulæ used by the Archdeacon, and recompute the ellipticity and the amount of the local attraction at the several stations. The accompanying paper contains the results; and you will see that we have simply made the numerical computations without presuming to alter the formulæ employed; in fact I have considered it almost a duty on our part to supply the labour of recomputation, the necessity for which was caused by the errors in our first published volumes."

"Henry James."

"G. G. Stokes, Esq., Sec. R.S."

November 30, 1858.

ANNIVERSARY MEETING.

The LORD WROTTESLEY, President, in the Chair.

Mr. J. G. Jeffreys reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including a balance of £27 5*s*. 8*d*. in the hands of the Treasurer, amounted to £3864 15*s*. 3*d*.; and the total expenditure during the same period, including a balance of £47 9*s*. due to the Society's Bankers, amounted to £4048 1*s*. 11*d*., leaving a balance due to the Treasurer of £183 6*s*. 8*d*.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following lists.

Fellows deceased since the last Anniversary.

On the Home List.

William Ayrton, Esq.
Rear Admiral Sir Francis Beaufort, K.C.B.
Robert Brown, Esq., D.C.L.
Edward Bury, Esq.
Alexander Caldcleugh, Esq.
Joseph Carne, Esq.
Sir Philip Crampton, Bart.
Edmund Davy, Esq.
Rev. Richard Dixon, M.A.
Sir James Fellowes, M.D.
Edward Griffith, Esq.
Thomas Charles Harrison, Esq.
Thomas Legh, Esq.
Sir James MacGrigor, Bart.
Sir George Magrath, K.H., M.D.
Ebenezer Fuller Maitland, Esq.
Thomas Lister Parker, Esq.
Hugh Lee Pattinson, Esq.
Very Rev. George Peacock, Dean of Ely.
Major-General Sir William Reid, K.C.B.
John Forbes Royle, M.D.
Richard Horsman Solly, Esq.
Thomas Tooke, Esq.
Benjamin Travers, Esq.
Charles Hampden Turner, Esq.
Dawson Turner, Esq.
Henry Warburton, Esq.

On the Foreign List.

Johannes Müller.

Withdrawn from the Society.

George Hunsley Fielding, Esq.

Fellows elected since the last Anniversary.

Thomas Graham Balfour, M.D.
Edward Mounier Boxer, Captain R.A.
Frederick Currey, Esq.
David Forbes, Esq.
Alfred Baring Garrod, M.D.
William Henry Harvey, M.D.
The Rev. Samuel Haughton.
Henry Hennessy, Esq.
David Livingstone, LL.D.
John Lubbock, Esq.
Right Hon. Sir John Pakington, Bart.
Henry Darwin Rogers, LL.D.
William Scovell Savory, Esq., M.B.
Warington Wilkinson Smyth, Esq.
The Right Hon. James, Lord Talbot de Malahide.
Lieut.-Col. Andrew Scott Waugh, B.E.
Thomas Williams, M.D.

Foreign Members elected.

Robert W. Bunsen.
Louis Poinsot.
Carl Theodor von Siebold.

The President then addressed the Society as follows:—

GENTLEMEN,

IN addressing you for the last time from this Chair, which by your favour I have now occupied for a period of four years, it affords me great gratification to be able to announce that all those measures which were rendered necessary by our removal to this site, are now completed, and we meet in an apartment which may be truly said to be worthy of a Society which for near 200 years has taken the lead in fostering a spirit of investigation into the laws of nature, and thus promoting the best interests of its country and of mankind.

I rejoice that our walls are once more adorned by pictures of some of the most eminent of the many distinguished men, who by their lives and discoveries have left an imperishable name to posterity, and shed a halo of glory over the whole human race. Even as amidst the ruins of Iona our great moralist felt his religious enthusiasm powerfully aroused, so may the sight of these portraits kindle in us and our successors an earnest desire to emulate the virtues of those whom they represent—that spirit of persevering research which achieved such brilliant success—that regard for truth which deems no sacrifice too great when her interests are at stake—that modesty, the never-failing companion of genius, which, slightly regarding results attained, is almost overpowered by the sense of what remains to be accomplished. When we look at these memorials of our predecessors, may we feel as the Romans of old, when they beheld the statues of their ancestors; they declared, "Cum majorum imagines intuerentur, vehementissime sibi animum ad virtutem accendi;" and yet, to use the words of the Grecian orator, *Παισὶ δ' αυ ἢ ἀδελφοῖς ὁρῶ μέγαν τὸν ἀγῶνα.* Great is the competition indeed, and few are the champions worthy to contend.

In a former Address I expressed a hope that the late Government would send an Expedition to the mouth of the Mackenzie River to continue those magnetical observations, which had been so perseveringly and successfully carried out by Capt. Maguire, and from which expedition important accessions to our knowledge of the magnetical laws were not unreasonably expected; but unhappily these hopes have not been realized. Our disappointment may possibly be traced, partly to the dislike which seems to prevail to anything

which can be designated as a renewal of Arctic voyages, and partly to the want of a due appreciation of the value of the researches proposed to be undertaken. On the objections arising out of this antipathy to Polar expeditions, I have said so much on a former occasion, that I will not again advert to them, further than to observe that there was no ground for believing that the dangers to which Arctic voyagers are sometimes, though rarely, exposed, would have been encountered in this expedition; and doubtless there were many gallant hearts eager to face any amount of peril in so worthy a cause. As to the second point, viz. the want of a due appreciation of the researches themselves, I think it impossible to believe that any one of average capacity and discernment would undervalue the importance of prosecuting inquiries of this character, were he familiarly acquainted with the history of scientific discovery. If our leading statesmen and legislators had perused with the same attention the records of the progress of science as many of them have devoted to the historical memorials of the two great nations of antiquity, can it be doubted that they would view these questions in a far different spirit?

When two powerful nations connected together by the tie of a common origin and a common language were informed that messages of mutual congratulation between their respective rulers had been transmitted by the electrical current, beneath more than 2000 miles of the deep waters of the vast Western Ocean, and that the two great continents of the globe had been brought, as it were, within speaking distance, a general sensation of delight pervaded their inhabitants, and there was no want of appreciation of such results of scientific research; but it were well for Science and for the cause of future discovery, if any great proportion of those who shared in this joy had been able to trace the slow and gradual processes by which such triumphs of human genius had been finally won. I have no hesitation in speaking of this wondrous achievement as a success, for it has been clearly shown that the work can be performed whenever public spirit and zeal shall supply the necessary funds.

There is a lesson then which many men have yet to learn, and it is most desirable that those especially, to whom the destinies of nations are given in charge, should learn it—a lesson which I may express in the words of a Report of the Parliamentary Committee

of the British Association, viz. "The most abstract scientific investigations have often led to most useful industrial applications; and observations and experiments, seemingly trivial and likely to lead to no useful result, have sometimes, after the lapse of years, been elaborated into discoveries which do honour to human nature."

Let us by way of illustration sketch some of the countless researches which preceded the invention of the Electric Telegraph. In 1729 Grey discovered that electricity could be transmitted through conductors to a distance. In 1747 it was transmitted through several miles of wire. In 1753 an anonymous writer in the 'Scots' Magazine' first suggested the idea of an electric telegraph. In 1800 the voltaic battery was invented. In 1802 it was discovered that the earth might be substituted for the return wire of a voltaic circuit. In 1820 Oersted discovered the mutual action of voltaic conductors and magnets, the foundation of the science of Electro-magnetism. In 1820, also, Schwiegger invented the electro-magnetic multiplier. In 1822 Ampère developed the laws of Electro-magnetism, and discovered many new facts, and Arago detected the action of a voltaic current on soft iron. In 1827 Ohm eliminated the laws of the voltaic circuit. In 1832 began the brilliant researches of Faraday, in which he discovered and enunciated the laws of voltaic and magneto-electric induction. In 1834 Wheatstone invented and practically applied a method of measuring the velocity of electricity in metallic wires. In 1835 Gauss and Weber employed their electric battery in establishing a communication between the Observatory of Göttingen and the University; and in July 1837 Wheatstone first tried his electric telegraph on the line of the London and Birmingham Railway. During all this period the voltaic battery was gradually improved and its powers vastly augmented by Daniell and Grove.

Again, the progress of Chemistry during the last sixty years affords abundant evidence of the advantages derived from the pursuits of abstract science, when viewed simply in their bearing upon the comfort and convenience of mankind.

At the close of the last century, the Swedish chemist, Scheele, made a series of experiments on the black oxide of manganese. To some this might have seemed a very unprofitable waste of time; but what was the result? Chlorine was discovered, a substance of the greatest importance in the arts. Berthollet, finding that this gas

changed the colour of the corks of the bottles in which it was confined, suggested its employment as a bleaching agent. This suggestion, successfully carried out by Tennant, produced a complete revolution in the staple trade of Manchester and Glasgow. The operations of the bleacher were shortened from several months to as many hours. The discovery of iodine was the result of a not very promising examination of the refuse of kelp liquors; and a laborious train of investigation into the laws of decomposition gave us chloroform. But what inestimable benefits have accrued to mankind from these discoveries! I need hardly speak of the vast alleviation of human suffering which is due to the guarded use of the latter substance; and the former, in addition to its medicinal value, is the basis of photography, one of the most beautiful and curious of modern arts, which not only adds to our rational enjoyments, but is extensively used in scientific researches, and ministers efficiently to the useful purposes of life.

Then, again, Professor Owen astonished his audience at Leeds, by relating how much agricultural wealth had been derived from the proper application of a single once-neglected fossil.

The hopes which I at one time entertained that the Government of Victoria would erect at Melbourne a four-foot reflector for the observation of the Southern Nebulæ, have not hitherto been fulfilled. The General Committee of the British Association have voted a sum of £200 towards carrying out the project, on condition that the remainder of the funds shall be supplied by the local government; and this course they have taken in testimony of the interest which they feel in the proposed measure, and of their desire to see it carried fully into effect. Our posterity will have just ground of complaint if the task be much longer postponed of surveying and delineating the forms and peculiarities of these southern nebulæ with instrumental means, equal to the work, and capable of supplementing what has been done and is now doing for the nebulæ visible in our own latitudes by the magnificent instrument, for which Science is indebted to your late President. The possession by this country of vast colonies in various parts of the globe, offering facilities for the erection of observatories and instruments for pursuing our researches into nature's laws under every variety of climate and position on the earth's surface, does seem to impose on a great and

thriving nation some obligation to utilize these advantages for the welfare of mankind at large. Truly something besides material gold is to be had there for the seeking; the precious ores of intellectual progress may there also be culled and stored away.

The history of the progress of astronomical science has already disclosed both the evil effects of neglecting these duties, and the benefits which are likely to accrue when they are properly fulfilled. When, in 1819, Encke computed the orbit of the comet of short period, which justly bears his honoured name, and announced its return in 1822, that return would not have been observed had not the zeal of a private individual, Sir Thomas Brisbane, established an observatory at Paramatta in Australia, where, and where alone, its reappearance was noted. Subsequently, the observatory at the Cape of Good Hope was established; but for many years, both there and in Australia, the instrumental means provided for the observation of comets were inadequate to the due discharge of the task. Now it is well known that the motions of the comet of Encke first suggested to astronomers the probability of the existence of some highly attenuated medium or ether pervading the planetary spaces, in which both planets and comets perform their revolutions. Every succeeding return of this most interesting though diminutive body has tended to confirm and strengthen that probability, which has now well nigh, if not entirely, assumed the proportions of a physical fact. But on each succeeding return of this wandering visitor, until 1855, the distinguished veteran, who watches its motions with all the interest and anxiety displayed by a fond parent towards a favourite child, has had cause to deplore the deficiency of the instruments in the Southern Hemisphere, to which the duty is necessarily assigned of continuing the observations after the comet has ceased to be visible in these latitudes. Before 1855, however, a more powerful equatoreal had been supplied to Mr. Maclear, the able astronomer at the Cape; and now, to use the words of Encke himself in his letter to the Astronomer Royal,—"In the year 1855, thanks to the skill of Mr. Maclear, and the beautiful 8½-foot equatoreal with wire micrometer, the observations are perfectly on a level with the European, which I take the opportunity of begging [you] to make known to Mr. Maclear with my best thanks. It is a real pleasure now constantly to see that the comet is observed at every return with an

accuracy, which, in the year 1819, there was no possibility of expecting." Now, it appears that these observations of 1855 fully confirmed the fact of the acceleration of the comet's mean motion, so often observed before; but they have done more than this, for they have been so well made, that Encke was enabled by their means to correct the elements of the orbit, and calculate an ephemeris for the return of the comet this year, taking into account the perturbations of Jupiter, with such consummate accuracy, that on the first evening on which, through the absence of moonlight, there was a hope of finding it, which was on the 7th of August, it was actually detected during the first half-hour's search in the place given by calculation, though presenting to the eye only a faint diffused nebulous mass of about one minute in diameter. Now, if the existence of this ether, or attenuated atmosphere, pervading the planetary spaces, should be conclusively established, of which there is now scarcely any doubt, it is a fact of the utmost physical importance. We may not indeed in the present state of science be able to foresee all the consequences which may flow from its establishment; but we know enough to perceive that the question has an important bearing upon several interesting modern speculations. The suggestion of this resisting medium is only one of many important services which this diminutive but most interesting comet has rendered to astronomical science, and will doubtless still continue to render. Mr. Maclear's equatoreal will now be turned to profitable account in following up the fine comet of Donati, which has just escaped from our view to present itself, but shorn of most of its former splendour, to the expectant gaze of southern astronomers. Those who had the good fortune to observe this comet on the night on which it approached Arcturus so nearly, will not easily forget the spectacle it then presented.

You are well aware that it was in our Colonies also that many of the magnetical observatories were established, in which for several years hourly observations were made of the three magnetical elements; and it may be confidently affirmed that from these observations most valuable results have been deduced, to which, however, as they were to some extent described by me on a former occasion, I will not further allude, than to inform you that in the opinion of your Council the time has now arrived, in which it is most expedient that

these observatories should be again established for a limited period, furnished with instruments from Kew, embracing all modern improvements. The propriety of pursuing this course has been carefully investigated by a Joint Committee of this Society and the British Association, of which Sir John Herschel acted as Chairman. They have reported strongly in its favour, and an application has accordingly been made to the Government by Prof. Owen, as President of the British Association, and myself, urging the re-establishment of these observatories at four stations, viz. Newfoundland, Vancouver's Island, the Falkland Isles, and at Pekin or some adjacent station. I cannot but add the earnest expression of my hope that the Government will be induced to regard with favour this proposal, for I well know that all the distinguished men who have made magnetical science their peculiar study, including the honoured name of Humboldt, are most earnest in desiring it. It would require more time than I can afford to bestow on the subject, were I to attempt even to hint at all the benefits which might flow from the complete elimination and elucidation of the magnetical laws; but the construction of correct and complete charts, showing the variation and the isodynamical and isoclinal lines at given epochs, is alone an object of transcendent importance to commerce and navigation. To this must be added the accurate establishment of the data on which are founded the methods adopted for ascertaining and correcting the deviations of the compass in iron ships; and besides, we must always bear in mind that the result of modern speculations seems to show that all the so-called imponderable agents, heat, light, electricity, and magnetism, are intimately connected by mysterious links; every accession to our knowledge of one has therefore an important bearing on the elucidation of all the others.

It would appear from a communication which has been received by your Council, that it has been in contemplation by the Government to erect, on the unoccupied site to the north of Burlington House, an office for the Commissioners of Patents. This is a mode of employing the large surplus income which is now derived from patent fees, to which no objection can be reasonably made; but the whole subject of the working of the patent laws must at no very distant date again undergo a searching investigation. It can never be tolerated

that inventors, to whom we owe such inestimable accessions to the conveniences and luxuries of life, should be subject to a tax peculiar to their class alone; and this must be the effect of the present law so long as fees are received from the patentees exceeding the amount which may be reasonably demanded for purposes in which they have themselves a direct interest, and the surplus carried to the account of the public Exchequer. At the same time I cannot but think that a small sum in addition might justly be required from every grantee of a privilege of this kind, to be carried to a fund for the promotion of the researches of abstract science; for assuredly there are none who derive more direct benefit from such investigations than those from whom the payment would be demanded; and that class have, and more especially in a country like this, opportunities of turning their talents to pecuniary account, which are altogether denied to the cultivator of abstract science, who forms his nest only to be seized by some more fortunate occupier, coming in like the bird in the fable, and appropriating to his own benefit the severe and long-protracted labours of the skilful and painstaking builder.

On reviewing once more the position of Science in this country, I find that the voluntary or independent system has been carried very far indeed. That is, a great many Societies have at various periods, from 1663, the date of this the most ancient, to the present time, been successively founded for the promotion and encouragement of science. Those who compose these various associations pay for the honour of enrolling their names as members, sums which must be considered large in reference to the means of some of the contributors; and many of those members in addition devote a considerable portion of their valuable time to scientific researches. These Societies, though contributing so much to the public weal, are yet for the most part entirely unrecognized by the Government, and, with certain trifling exceptions, receive no kind of public aid in prosecuting their valuable labours; but, on the other hand, by a kind of hap-hazard arrangement, strongly characteristic of our national character, the Government has from time to time organized various bodies, which, though with few exceptions not recognized by any act of the legislature, are yet invested to a certain extent with an official character, and obtain greater consideration from the Government in consequence. Such, for example, are the

Board of the Trustees of the British Museum ; the Board of Visitors of the Royal Observatory of Greenwich, which has an official connexion with the Board of Admiralty; the various officers to whom the management of matters connected with science or art is committed by the Council of Education, Board of Trade, or Military or Naval authorities. Now if we contrast this system of ours with that of our neighbours in France and some other continental states, we shall find that there are marked and striking differences between them. In France there is the Institute, the members of which not only do not pay out of their own resources for the honour they enjoy, but are invested with high privileges and great official consideration, and receive stipends from the State, at the price no doubt of an amount of Government interference with their proceedings which our countrymen might probably hesitate to submit to. I believe that in France the members of the Institute, as such, are regularly consulted on the important scientific questions which must present themselves for solution from time to time in the course of civil and military administration. In England, on the other hand, the various voluntary or private Societies established for the promotion of science have never been recognized as advisers of the Government, or consulted as a rule ; though undoubtedly the Councils of this and other Societies have been often applied to for advice, and more frequently perhaps of late years than formerly; but the various Boards and officers above referred to are not only invariably consulted on those matters which are consigned to their management, but there are few instances on record in which their advice has been neglected, or the measures recommended by them have failed to be carried into effect.

In instituting any comparison of the relative merits of these two modes of proceeding, it is clear that, irrespective of the results obtained, the decision of the question as to which is to be preferred, must depend greatly on the mode by which the referees of the Executive in the two countries are appointed; for it may be fairly assumed that both contain men well qualified, both by their extensive attainments and natural and acquired abilities, to give good advice to their respective Governments. Now the French Institute undoubtedly comprehends most able and highly distinguished men, enjoying an established and world-wide reputation, and well qualified to give sound advice to their Government on questions of science. This is

undoubtedly true, though it may be equally true that there are circumstances in the working of the French system, dependent partly on the immense value of the privileges conferred by the election, which have a tendency to arrest the wholesome progress of science in that country. In England, so far as an experience of nearly forty years can enable me to form an opinion, I should say that there is great impartiality and discrimination exhibited in the selection of men to fill high posts in the various Scientific Societies there established, and I apprehend there are few instances in which meritorious and successful cultivators of the various departments of science, if resident in the metropolis, have failed in obtaining their share in the government and administration of those learned bodies. The constitution of the Board of Visitors of the Greenwich Observatory also is worthy of all praise. It has contributed in some degree doubtless to raise that establishment to the high point of eminence which it now deservedly occupies; and that constitution has called forth the warm eulogiums of the veteran French astronomer Biot, than whom there can be few more competent judges. On the other hand, the mode of appointing the Trustees of the British Museum is defective in the extreme. They are nominally elected by the Trustees themselves, but the Trustees, who have been themselves elected, are by an absurd regulation excluded from the body of electors. The officers of the Council of Education and the Board of Trade, and the Military and Naval Boards are appointed by the Government or Military authorities, and the nominations are thus subject to all the incidents of appointments of this class.

The blots in our system seem to be, first, that there is a great want of combined action between the various communities representing Science;—an evil, which might possibly be remedied by some joint representation of the whole; and secondly, that the Societies instituted for the promotion of the various branches of science, though containing among their members and governing bodies those men who have been impartially selected as pre-eminent in their various walks, are not officially recognized in any way as authorities, or appealed to except occasionally and by accident, whenever some member of the Administration may happen to perceive that their counsel might advance the object in view, and be profitable to the State. Moreover, it seems never to have occurred either to the

Government or Parliament, that the materials exist out of which a Board may be formed which might be expected to give wholesome advice on scientific questions, take on themselves a share of the Government responsibility, and save the country from the bad consequences which now flow either from neglecting to take counsel, or from the careless and indeterminate way in which it is sometimes sought and obtained.

There are other points connected with the important questions to which I have shortly adverted, for which I must refer you to the twelve resolutions of your Council of January 1857; and also to the Report of the Parliamentary Committee to the British Association of 1855, already referred to, in which the subject is discussed and examined.

I anticipate much advantage to science from the circumstance that His Royal Highness The Prince Consort has consented to accept the Presidency of the British Association. Science has no need of a Patron in the ordinary sense of that term; but her interests are much advanced when those who occupy exalted stations, and are endowed with great qualities of mind and heart, are induced to take part in her councils, and become cognizant of the character, views, and requirements of the most distinguished of her cultivators.

It would appear from the Report of that truly zealous and indefatigable officer of the Society, your Treasurer, dated October 1857, that we had then a considerable excess of income over our average ordinary expenditure, that probably we should shortly be in the receipt of annual sums in respect of the Stevenson Bequest, and that on the demise of the tenant for life, we should come into possession of the Handley legacy. This therefore renders it probable that we shall have a clear balance at the end of each year; and in the course of time, as annuitants die and the legacies come into possession, that balance will be augmented.

Now since the date of this Report, we agreed to appropriate a sum of £250 towards the formation of a MS. Catalogue of Scientific Memoirs, a grant which will probably be yearly renewed until that work is completed; and there can be no doubt that the cost of the printing and publishing the Transactions is increasing and will still increase; but still we must look forward to a period which may not be tardy in arriving, when we shall have a larger annual surplus to

dispose of. I quite agree with our Treasurer, that it deserves consideration whether a more advantageous employment might not be found for our surplus income, " more beneficial to science and to the Society itself, than that of continuing to increase the accumulated funds." Dr. Wollaston would probably have supported these views; for in his celebrated letter on establishing the Donation Fund, read to the Council on the 11th of December, 1828, he says, " I hereby enjoin the President, Council, and Fellows not to hoard the dividends parsimoniously, but expend them liberally, and as nearly as may be annually."

Now there are some who think that we should look forward to a time when the payment of annual subscriptions should be discontinued. I cannot subscribe to this opinion. It is true, that there are men who by natural talent, by incredible perseverance, self-devotion and industry, have risen, as it were, from the ranks of science, to whom the large payments exacted from members are a serious inconvenience and an obstruction to their obtaining those honours which they have justly earned by the labours of a well-spent youth. Let such men be excused; but I cannot comprehend on what principle we should refuse to receive pecuniary contributions for a most worthy object, from those who are not unwilling and are perfectly able to pay them in exchange for the honours and many other advantages which their Fellowship confers upon them. There may be some, again, who imagine that there will be some difficulty in discovering a mode of expending this anticipated surplus which is likely to meet with general approval. I cannot believe it. There are many works which cannot be written; many researches, experiments, and observations which cannot be made, reduced or published; many voyages and travels that cannot be undertaken because the necessary funds are not to be had. Moreover, there is a mode of applying a part of these accumulations, which has suggested itself to some of the members of your Council, to which I will allude very briefly. Your Secretaries are always chosen from among the most distinguished of your Fellows. They are generally men who are occupied in professional and professorial duties which embrace a very large portion of their time; and it is their precious leisure hours only which they can devote to your service; hours, which when not given to necessary relaxation, they would

probably employ in scientific research or private study. It might be truly said that no payment that we can afford to make could be a sufficient return for such services as they render under such circumstances. It is not intended however, doubtless, that the full value should be given. It is perhaps right that the honour conferred by the appointment should constitute a part of the reward, but the remaining or pecuniary part may be so trifling as necessarily to exclude from your service some who cannot afford to accept office on terms so inadequate.

And now, in conclusion, I bid you farewell, and I do so with a most cordial and heartfelt expression of gratitude for the unvaried kindness and confidence which I have ever received from you. The transaction of your business has necessarily brought me into frequent and most confidential intercourse with your officers, and I can truly say that I have ever received from them the most effective support and assistance; they have given me sound advice without obtruding it, and have brought to the conduct and administration of your affairs an amount of talent, zeal for your interests, and varied acquirements, which may be equalled, but which it will be very difficult indeed to surpass. The intimate relations and unreserved confidence which have ever existed between us have created a friendship which I hope may endure as long as my life is prolonged. The members of your Council are most assiduous in their attendance; and the affairs of the Society are by them discussed and transacted in a manner which has always excited in me great admiration.

In resigning the Chair I can feel no distrust as to the future, when I reflect that I shall probably be succeeded by one whose private worth, scientific attainments, and intimate knowledge of the business of this Society are universally admitted, and in whose hands your interests and reputation are safe. I shall account him happy if he be elected to fill a post, so honourable in my estimation, that the recollection that I have once occupied it will be one of the chief consolations of my latter years.

The Copley Medal has been awarded to Sir Charles Lyell for those original researches and comprehensive generalizations which have mainly contributed to raise the study of geology to that high scientific position which it now holds, and to maintain for the English

school of geologists that eminent rank which has hitherto been accorded to it.

A quarter of a century has now just elapsed since the Royal Medal was awarded to Sir Charles Lyell for his 'Principles of Geology.'

It was his good fortune to be associated in his earliest years with those from whom he imbibed a love of Natural History, and who fostered those habits of accurate observation, the successful application of which to geological problems is one of the prominent features of his scientific writings. This it was that gave so much value and promise to his first labours on the geology of the marls and freshwater formations of Angus, his native district; that enabled him to appreciate to the full, the bearings, not only of palæontology (the importance of which had been already recognized), but also of a large class of facts, of almost equal value in relation to the philosophy of geology, conducting him through a series of researches, the merit of originating which is wholly his own:—I allude to the permanence or mutability of animal and vegetable forms, to their reciprocal action, and to the laws which govern their reproduction and dispersion; and we hence owe to him both the enunciation and the demonstration of the doctrine, that the history of many ancient geographical changes and geological phenomena is only to be arrived at after a careful study of the present distribution of organic beings.

The same line of research led Sir C. Lyell, in conjunction with M. Deshayes, to that method of classifying strata, and hence of comparing formations, by the relative proportions of living organisms they contain,—a method which has proved of such eminent service to geologists. By its aid, and after untiring diligence in collecting, and equal perseverance and sagacity in comparing the results of his own and other collections, with those in the various museums of Europe and America, he has not only largely contributed to classify the tertiary rocks of both continents, but has further given us the first clear idea of the relations of modern strata separated by the great valley of the Atlantic Ocean.

In immediate connexion with these inquiries, and no doubt directly originating in his mind from that principle to which he has steadfastly adhered, of endeavouring to account for past changes

by a reference to existing causes, are his philosophical views of the relations of climate to the former extension of land and water, which have resulted in doctrines of the highest importance. This principle, long disputed, is now so far recognized, that no cautious naturalist declares himself satisfied with the explanation of a geological phenomenon except he can test or illustrate it by agencies now in operation. It is true that Hutton and Playfair originated the hypothesis that causes such as are now in action would account for all we see on the surface of the globe; but we owe to Sir Charles Lyell that careful collection of facts and that sagacious reasoning, by which this hypothesis has been raised to the rank of a well-grounded theory.

We cannot overrate the services which he has rendered by his investigations into the dynamics of geology; nor the importance of his memoirs on the risings and subsidences of land, and their incessant fluctuations, on the means, extent, and effects of the distribution of sediments, and the changes wrought on these by metamorphic action, and on the amount of denudation which all formations have undergone. These, and a vast number of subsidiary phenomena, have led him by an elaborate train of reasoning, to gauge boldly the depth of detrition to which our planet has been subjected, by the miles of sedimentary rocks accumulated on its surface; and to those startling speculations on the antiquity of the earth's crust, which, having survived the ridicule and contempt of many critics, and the rejection by more as visionary, are now becoming accepted as fundamental truths.

The geology of volcanos in its widest sense presents another great field for observation and reflection, to which Sir C. Lyell has assiduously applied himself; and here I can do no more than allude to his discoveries and discussions on "craters of elevation," the amount of denudation which volcanic cones have sustained, their valleys and dykes, and the inclination of their lava beds, as all of the greatest importance; especially, however, referring to the results of his recent studies of Etna, because they afford the clearest demonstration of the genesis and internal structure of volcanic mountains.

There is indeed no department of geology,—from the Silurian strata to the mud now depositing on our coasts and in our lakes—

from the Plutonic rocks to the lavas not yet cooled on the flanks of Vesuvius—that Sir Charles has not diligently explored, from Scandinavia to Sicily and the Canaries, and from Canada to the Gulf of Florida; he has sought to develop the history of coal by observations on the ancient carboniferous strata of both hemispheres, on the superficial peats of England, and on the vegetable swamps of the United States; tracing at the same time the coasts of the former glacial ocean, by the boulders that are the landmarks of its ancient shores, and by the broken shells thrown up on its beach.

In thus recognizing those attainments and powers of generalization which have for the first time brought so many branches of science into close connexion, and have placed Sir Charles Lyell foremost in the rank of the founders of the Philosophy of Geology, the Council of the Royal Society do not overlook the scientific value of the narratives of his American travels, the merits of his style, and the evidences of careful attention to those details of scientific writing which have rendered his works so clear and attractive. To these qualities he is greatly indebted for the influence he has exercised, not only in attracting students to follow his steps, and in inducing proficients in geology to reflect, but in advancing the whole domain of natural science; and hence he may justly claim from botanists and zoologists as cordial a tribute of admiration and gratitude for services rendered to their branches of science, as has ever been tendered to him by his brother geologists.

Sir Charles Lyell,

Accept this Medal, the highest reward we can bestow, in token of our due appreciation of a series of scientific researches, prolonged through several years, and prosecuted with very great ability and perseverance; and in wishing you long life and health to enjoy your well-earned honours, allow me to add that my happiness in bestowing them is enhanced by the reflection that I am conferring pleasure on one whose friendship I have now enjoyed for a period of more than forty years.

The Rumford Medal has been awarded to M. Jamin, Professor at the École Polytechnique of Paris, for his various experimental researches on Light.

M. Jamin has for many years been engaged in a series of important experimental researches in optics. One of his earliest labours relates to metallic reflexion. Malus found that metals form an exception to the general rule of polarizing light at a proper angle of incidence, though they do produce a partial polarization. We owe to Sir David Brewster the discovery of the curious modifications which reflexion at the surface of a metal is capable of producing on polarized light. The theory of undulations enables us to form a clear idea of the nature of the light so reflected, and shows, moreover, that in order to know everything about the reflected light, whatever be the nature of the incident light, it is sufficient to know for each angle of incidence three things; the proportion in which light polarized in each of the two principal planes is reflected, and the difference of phase produced by reflexion. The subject of metallic reflexion had afterwards been considered in its bearings on certain phenomena; but there were still wanting, on the one hand, an extensive series of careful measures of the three quantities above mentioned, taken for various incidences and for different metals; and on the other, a theory which should embrace them all, expressing them as functions of the angle of incidence by formulæ containing a certain number of constants depending on the nature of the metal.

M. Jamin applied himself to supply the first of these desiderata; and in an important paper published in the 'Annales de Chimie' for 1847, he has given tables containing the results of careful measures taken on several metals. The methods employed cannot here be described, but they exhibited in a remarkable manner a combination of experimental care and skill with a thorough acquaintance with the principles of the theory of undulations. It belongs to the physical mathematician to supply the requisite formulæ, which, while differing from mere empirical formulæ in being deduced from a physical theory, shall accurately represent the results of observation.

The subject of metallic reflexion had engaged the attention of mathematicians, and M. Cauchy had arrived at certain formulæ to express everything relating to the metallic reflexion of homogeneous light. These formulæ contain only two arbitrary constants, depending upon the nature of the metal, and capable of being determined by the observation of two angles. M. Jamin himself has compared

the results of his observations with the formulæ of M. Cauchy; and the accordance of the calculated and observed numbers is striking in the highest degree.

Another highly important memoir of M. Jamin, read before the Academy of Sciences, and published in the 'Annales de Chimie' for 1850, relates to the reflexion of light at the surface of transparent media. According to the formulæ of Fresnel, when light polarized perpendicularly to the plane of incidence is incident on such a medium, and the angle of incidence is increased from 0° to 90°, at a certain angle (the polarizing angle) the reflected light vanishes, and in passing through this angle the reflected vibration changes sign, or in other words, the phase is changed by half an undulation. It had already been recognized, that in the case of very highly refracting substances these results differ very sensibly from the phenomena actually observed; and by observations on Newton's rings when formed between a glass lens and a plate of diamond, Mr. Airy long ago showed that in the case of diamond the light does not wholly vanish at the polarizing angle, and that in passing through this angle, on increasing the angle of incidence, a continuous but rapid retardation of phase takes place, amounting to a quantity sensibly equal to half an undulation, while the angle of incidence is increased a few degrees. But except in the case of substances of very high refractive power, it had been supposed that Fresnel's formulæ sensibly represented the phenomena observed. Such was the state of the subject when M. Jamin commenced a series of most elaborate experimental researches, which resulted in showing that the phenomena above mentioned, instead of being confined to a few of the most highly refractive bodies, were of almost universal occurrence, though for most bodies the only sensible changes of phase were crowded within a small range of incidence, and the quantity of light reflected at the minimum was very small. A very striking result of these researches was, that the change of phase had not always the same sign. It appeared that for bodies having a higher refractive index than about 1·46, the change was of the same sign as in the case of metals, while for bodies having a refractive index much lower it was the opposite. Thus the case of no reflexion at a particular angle, accompanied by an abrupt change of phase amounting to half an undulation, or, as in this special instance it may be more simply

considered, a change of sign in the reflected vibration in passing through zero, proves to be merely the separating case between two opposite classes of phenomena. M. Jamin has further compared the results of his observations with the formulæ of M. Cauchy, with which they manifest a striking agreement. These formulæ contain, besides the index of refraction, a second constant, which M. Jamin calls the *coefficient of ellipticity*, which must be determined for each substance in particular.

In a later memoir published in the 'Annales de Chimie' for 1851, M. Jamin has extended the same results to liquids, which offer several examples of what had been already perceived in the case of solids; that though if bodies be arranged in order according to their decreasing refractive indices, the coefficient of ellipticity *on the whole* decreases, and after vanishing increases again negatively, yet there exist exceptions enough to this rule to show that the coefficient of ellipticity cannot be a function merely of the refractive index, but must depend also upon the nature of the body.

The preceding volume of the same work contains two other memoirs by M. Jamin; one on the double refraction of quartz, the other on total internal reflexion; both affording new proofs of his skill and sagacity.

In a paper published in the 'Annales de Chimie' for 1852, M. Jamin has shown the influence on the phenomenon of Newton's rings of the reflexion of light polarized in the plane of incidence in the neighbourhood of the angle of maximum polarization, and of the changes of phase which it then undergoes. The paper contains also other results relating to the rings which are worthy of notice, not belonging specially to what takes place near the angle of maximum polarization.

There are other papers of M. Jamin's to which time will not permit me to refer; but in the year 1856 he brought before the French Academy an instrument, in which he utilized, in a most happy manner, the fringes which had long before been observed as arising from the interference of some of the many portions into which light is divided by reflexion at the surfaces of two plates of glass of equal thickness. This instrument constitutes an interference-refractometer which unites delicacy with singular convenience.

In the same communication the author has given some highly

curious applications of his instrument to the obtaining of qualitative results, having applied it to show the concentration of the solution of an iron salt produced by the attraction of a magnet, the state of a solution about a growing crystal, &c. The use of the instrument is, however, by no means confined to qualitative observations; and in two memoirs published last year in the 'Annales de Chimie,' the author has successfully determined by its means the refracting power of the vapour of water, and the influence of even a moderate pressure in altering the refractive power of water in the liquid state.

Mons. Jamin,

Accept this token of the great value which we attach to your extensive series of experimental researches on Light.

A Royal Medal has been awarded to Mr. Albany Hancock for his numerous and varied contributions to Comparative Anatomy and Physiology, but more especially for his "Researches on the Organization of the *Brachiopoda*," which will appear in full in the forthcoming volume of the 'Philosophical Transactions.'

Many of Mr. Hancock's labours having been carried on conjointly with his friends Dr. Embleton and Mr. Alder, it is proper on this occasion that their merits should be duly acknowledged.

Of these conjoint papers may be more particularly noticed those upon the organization of Eolis and Doris, which are remarkable not only for the clear exposition they give of the previously much misunderstood structure of the genera in question, but are also important for the new and enlarged views they contain respecting many points in the economy of the Mollusca.

Together with these must also be mentioned the well-known 'Monograph on the British Nudibranchiate Mollusca,' in which Mr. Hancock was associated with Mr. Alder,—a work eminent alike for the beauty and fidelity of its illustrations, and the value and completeness of its zoological and anatomical details.

Among the more important of Mr. Hancock's numerous independent contributions to science, should be noticed a valuable paper on the "Excavating Powers of certain Sponges;" his discovery and accurate account of a new and curious genus of burrowing Cirripeds;

and several others, in all of which is manifested a remarkable capacity for minute and accurate observation conjoined with great powers of generalization.

But in none of Mr. Hancock's labours are these faculties so eminently displayed as in his more recent investigation of the organization of the Brachiopoda. In his elaborate monograph on this most difficult subject, and of which it may truly be said a more complete specimen of minute anatomy has not appeared since the time of Lyonet, a detailed account is given of the whole organization of the Brachiopoda founded upon the laborious dissection of numerous species; several interesting points in their economy first indicated by Prof. Huxley are confirmed; many additional facts communicated; and a new and clear light thrown upon the previously obscure subject of the physiological and systematic relations of the class in general.

Mr. Huxley,

In conveying this Medal to Mr. Hancock, assure him of the great interest we take in his valuable labours, and express our hope that he may be long enabled to prosecute inquiries, from which so much benefit to natural science may reasonably be anticipated.

Your Council have awarded the other Royal Medal to Mr. William Lassell, for his various astronomical discoveries and researches, and for his skilful construction of several large Reflecting Telescopes, with which such discoveries were made.

The first of these telescopes, having a speculum of nine inches in diameter, was erected at Starfield near Liverpool in the summer of 1840, from which time Mr. Lassell has been a most diligent and successful labourer in the field of astronomical science. Although none of the principal discoveries which entitle him to take rank with the leading astronomers of the day have been made with this instrument, still as this was the first large Reflector to which an equatorial mounting was applied, we are justified in looking upon its construction as an important step towards that eminence to which its maker has since attained.

This instrument, however, did not long satisfy Mr. Lassell, for in the year 1846 we find him observing with a 20-foot reflector with

an aperture of 2 feet, also equatorially mounted, which he had in the mean time constructed; and he soon announced the very important discovery, on the 10th of October, 1846, of a satellite of Neptune,—most important, as affording the means of ascertaining the mass of Neptune.

Great triumphs, however, were yet to come; in September 1848 he detected an eighth satellite of Saturn, and by a singular coincidence the same discovery was made simultaneously by Professor Bond in America.

To these manifold labours we have to add numerous observations of Satellites, and of Comets, Eclipses and other occasional phenomena.

Of this devotion to his favourite science Mr. Lassell has already received a recognition, which astronomers value most highly, viz. the Gold Medal of the Royal Astronomical Society, which was presented to him in February 1849; but this seems only to have stimulated his zeal for the further advancement of astronomical science.

In October 1851 our indefatigable observer established the existence of two satellites of Uranus, nearer to the planet than the first satellite of Sir William Herschel.

In the autumn of 1852 he conveyed his 20-foot reflector to Malta, principally in order to survey in a clearer atmosphere the systems of the three most distant planets, Saturn, Uranus, and Neptune, with which he has particularly identified himself. To use his own words,—"his discoveries abroad were rather negative than otherwise," for he was satisfied that without a great increase of optical power, no other satellite of Neptune would be discovered; and with regard to Uranus, he says,—"*I am fully persuaded that either he has no other satellites than the four, or if he has, they remain yet to be discovered.*"

In the summer of 1854, owing to the encroachment of buildings round Starfield, he was compelled to move two miles further into the country to Bradstones, and incur the expense of building a second observatory, and here his instruments are now erected.

His praiseworthy ambition still impelled him to become a leader, where other men are content to follow. In the prolonged discussions which arose relative to the mounting of the great reflector of 4 feet aperture for the observation of the Southern Nebulæ, Mr. Lassell

took an important and distinguished part; and with his characteristic and laudable energy and zeal in the cause of science, finding that such an instrument was just now a desideratum, he undertook to construct one at his own cost. This he has now completed. This splendid result of the well-applied industry and skill of one to whom Astronomical Science had been already indebted for two fine telescopes, has an aperture of 4 feet and 37 feet focal length, and this is also mounted equatorially. From this magnificent telescope, in the hands of such a man, what may not be expected? And it adds not a little to his merit, that all the specula for these several instruments have been cast by himself, and polished by a machine of his own contrivance, of which he has given a lucid description in the Memoirs of the Royal Astronomical Society; and the instruments themselves are mounted according to his own plans.

In conclusion, it will I think be generally allowed, and posterity will doubtless confirm the statement, that in the history of reflecting telescopes, the name of Lassell must rank with that of Herschel and that of our late President, Lord Rosse, whether we consider the talent and perseverance displayed in their construction, or the important discoveries which have resulted from their use.

Mr. Lassell,

In presenting this Medal, allow me to express a fervent hope that you may be endowed with health and a long life. Your previous labours are the best security that in that case the magnificent astronomical means now at your disposal will in due time contribute still further to enlarge the bounds of that science which you love so much and have so materially advanced.

On the motion of Sir Henry Holland, seconded by Sir Roderick Murchison, the best thanks of the Society were voted to the President for his excellent address, and his Lordship was requested to permit the same to be printed.

The Statutes relating to the election of Council and Officers having been read, and Admiral FitzRoy and Mr. Huxley having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President.—Sir Benjamin Collins Brodie, Bart., D.C.L.

Treasurer.—Major-General Edward Sabine, R.A., D.C.L.

Secretaries.— { William Sharpey, M.D.
George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—Henry Wentworth Dyke Acland, M.D.; Admiral Sir George Back, D.C.L.; Rev. John Barlow, M.A.; Thomas Bell, Esq.; The Duke of Devonshire, M.A.; Edward Frankland, Ph.D.; John Peter Gassiot, Esq.; Philip Hardwick, Esq., R.A.; Arthur Henfrey, Esq.; Lieut.-Col. Henry James, R.E.; Sir Roderick Impey Murchison, M.A., D.C.L.; John Percy, M.D.; Archibald Smith, Esq., M.A.; Charles Wheatstone, Esq.; Rev. William Whewell, D.D.; The Lord Wrottesley, M.A.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s.* Anunally.	Paying £4 Annually.	Total.
December 1, 1857	9	48	374	8	276	715
Since elected	......	+3	+9	......	+8	+20
Re-admitted	......	......	......	......	......	....
Since compounded	......	......	+1	......	−1	....
Withdrawn	......	......	......	......	−1	−1
Since deceased ..	......	−1	−19	−1	−7	−28
November 30, 1858	9	50	365	7	275	706

Statement of the Receipts and Payments of the Royal Society between November 30, 1857, *and December* 1, 1858.

	£	s.	d.
Balance in the hands of the Treasurer	27	5	8
Subscriptions and Compositions	1784	4	0
Rents	197	2	9
Dividends on Stock	1111	8	9
Sale of Transactions, Proceedings, &c.	435	8	6
Cost of Printing Papers on Standards of Weights and Measures (repaid by Treasury)	294	10	7
Miscellaneous Receipts	14	15	0
Balance due to Bankers	155	8	0
Balance due to Treasurer	27	18	8
	£4048	1	11

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s.* per annum.
Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £60 0*s.* 0*d.* per annum.
Fee farm rent in Sussex, £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per cent. Annuities.
£23,569 17*s.* Consolidated Bank Annuities.
£513 9*s.* 8*d.* New $2\frac{1}{2}$ per cent. Stock.

EDWARD SABINE,
Treasurer.

	£	s.	d.
Balance due to Bankers	47	9	0
Rev. A. Walker, Fairchild Lecture, 1857–58	5	15	11
Mr. Gassiot, Bakerian Lecture	3	17	4
Mr. Huxley, Croonian Lecture	2	18	9
Poor, St. Mary-le-Strand, (Croonian Fund)	59	13	8
Salaries, Wages, and Pension	764	6	0
Fire Insurance	42	1	6
Printing Transactions	472	19	1
Ditto Proceedings and Miscellaneous	244	0	11
Engraving	445	2	6
Paper for Transactions and Proceedings	303	2	0
Binding Transactions	84	12	7
Books Purchased and Binding	371	4	3
Burlington House Removal Expenses	31	3	9
Stationery	10	4	6
Shipping Expenses	11	3	9
Fire and Lighting	63	15	0
House Expenses	93	7	4
Acton Estate Enclosure	487	9	3
Taxes	14	16	8
Donation Fund	259	11	4
Wintringham Fund	34	5	6
Catalogue of Periodicals	114	3	0
Postage, Miscellaneous and Petty Charges	80	18	4
	£4048	1	11

Obituary Notices of deceased Fellows.

Rear-Admiral Sir Francis Beaufort, K.C.B.—Among the losses which have been occasioned by death in the lapsed year, we have to deplore that of Rear-Admiral Sir Francis Beaufort, K.C.B., the late Hydrographer to the Admiralty, so well and so justly known as a scientific, indefatigable, and amiable Fellow of this and other Societies. This event occurred on the 17th of last December, at Brighton, whither he had repaired for the benefit of change in failing health, but with his mental faculties clear and vigorous. This, indeed, was exemplified in his discussing historical points on the very evening of his death; and in his consulting the 'Sacra Privata' of Bishop Wilson, a favourite work, almost until he calmly expired at $1^h\ 50^m$ after midnight: he was then in his 84th year.

This valuable officer was a son of the Rev. Daniel Augustus Beaufort, Rector of Navan, and Vicar of Collon in Ireland, who attained a well-merited esteem by his elaborate map of that country, excelled only by the subsequent Ordnance Survey. As young Francis evinced a predilection for maritime life, he had the good fortune to open his nautical career with a very able seaman, Captain Lestock Wilson, of the East India Company's Service, under whom he acquired a proficiency in navigation. At length, in 1789, when proceeding to China, that officer received orders to examine the Macclesfield Strait for a shoal on which an Indiaman had recently been lost. Operations were commenced by a survey of Pulo Leat, an isle in the Strait of Gaspar, the chart of which was entirely drawn by Beaufort. After an unsuccessful search for the hidden danger, from eleven different stations, and just as the attempt was about to be given up, the 'Vansittart' struck upon what proved to be the very reef for which they had been seeking. The destruction of the ship advanced so quickly that all hands had to take to the boats; and they underwent great hardships before reaching the Bay of Sango Boolo, where relief was obtained.

On returning to England, great excitement pervaded the public mind, and armaments were under equipment in all our ports. Forgetting the hardships of his recent disaster, Mr. Beaufort embarked

on board the 'Aquilon' frigate, and shortly afterwards sailed to the Mediterranean, escorting H.R.H. Prince Augustus, since Duke of Sussex and President of this Society. In 1794, the 'Aquilon' was one of Lord Howe's 'repeaters' in the great battle fought on the 1st of June; an event which deeply impressed itself on the young officer's mind, insomuch that he ever retained a vivid recollection of its details. His next ship was the 'Phaëton,' of 38 guns, in which he saw much varied and arduous service, by which he earned his Lieutenant's commission. Continuing in the same frigate till October 1800, he won his further promotion to the rank of Commander by gallantly cutting out a 14-gun Spanish polacca from under the guns of Frangerola, a fortress near Malaga.

This promotion was somewhat dearly purchased, for Mr. Beaufort was severely wounded on the head, and had several slugs through his left arm and body, which compelled him to lie by for a time. But he had scarcely recovered, when we find him aiding his brother-in-law, Richard Lovell Edgeworth, in establishing a line of telegraph stations across Ireland; an object in which he laboured successfully and gratuitously during two years. A mind disciplined to accurate observation acquires additional power of perception and discrimination; and to this power may be assigned the admirable communication which he made to Dr. Wollaston, of certain physiological and successive effects which he experienced under suspended animation, from being all but drowned.

In the summer of 1805 Commander Beaufort was again called into active service, being appointed to the 'Woolwich,' 44; in which ship he carried out naval stores to Bombay, and returned with a convoy of sixteen Indiamen and some country-ships. In 1807 he had an opportunity, though but a hasty one, of exercising his valuable talent for marine surveying by an examination of the vicinity of Monte Video, in the Rio de la Plata. His next appointment was to the 'Blossom' sloop-of-war, in which he was principally employed in taking charge of convoys of merchantmen until he obtained post-rank in May 1810, with the command of the 'Frederickstein' frigate. In 1811 he was directed to make an examination of the southern shores of Asia Minor, a service truly congenial to his mind, since it developed his full capacity as a marine surveyor, a classical scholar, and an inquiring geologist. He had

successfully examined the coast of Karamania, and was about to continue his operations down the shores of Syria, when they were suddenly cut short by an attack of fanatical natives, by one of whom Captain Beaufort was severely, almost fatally, wounded. Thus prevented from continuing the interesting survey, he repaired to Malta, and there, with exemplary fortitude, endured intense suffering from his wound, which for several months endangered his very existence. He was then ordered to England, in company with the 'Rodney,' 74, and his ship was paid off in October 1812, after which year he went no more afloat.

But the leisure of his half-pay time was not expended in idleness, as evinced by the subsequent publication of his admirable survey with its illustrative memoir; and by his exertions in the Councils of the Royal, the Astronomical, and the Geographical Societies. He was, moreover, a member of the Board of Greenwich Visitors, and one of the Committee of the Society for the Diffusion of Useful Knowledge, wherein he originated and conducted the widely-circulated series of cheap maps. Yet twenty years were permitted to pass before he was selected to that post for which he was so obviously and so eminently qualified; for it was not till 1832 that he was installed Hydrographer to the Admiralty. This office had been turned to but small account until Captain Beaufort took it in hand, and manifested its value to the public, not only in the construction of superior charts, but also in affording aid to the Commissions on Tidal Harbours, Ports of Refuge, and Pilotage; and indeed to all branches of naval scientific knowledge.

In carrying out these duties, he was ever ready to advise and assist; and he was a warm supporter of friendless merit, even in cases where he was opposed by certain official obstructions. Through all he conscientiously did his duty, although at times he felt it painful to enjoin services without a prospect of reward, where it was deserved and expected.

Sir Francis Beaufort was appointed one of the Civil Knights Commanders of the Bath; and in 1845 he accepted the rank of retired Rear-Admiral, with permission to retain his office of Hydrographer, the daily duties of which he assiduously attended until he was turned of fourscore. He married the daughter of his first commander, Captain Lestock Wilson, shortly after his last return

from the Mediterranean; and by that amiable lady had a family, of whom three sons and three daughters are living. Some years after her lamented death, he married, secondly, the daughter, by a third marriage, of his brother-in-law, R. L. Edgeworth, Esq., who survived him, but has since died.

Robert Brown, D.C.L.—In offering to the Society a brief sketch of the career of the greatest Botanist of the age, our attention is chiefly arrested by his intense devotion to his favourite study, and by the calm, reflecting, and philosophical spirit which he brought to bear upon its pursuit, the combination of which qualities were alone sufficient to raise him, by his own unassisted efforts, to the highest position in the world of Science. Robert Brown was the second and only surviving son of the Rev. James Brown, A.M., Episcopalian Minister of Montrose, by Helen, daughter of the Rev. Robert Taylor, and was born in that town on the 21st of December, 1773. Several generations of his maternal ancestors were, like his father, ministers of the Scottish Episcopalian Church, and from them he appears to have inherited a strong attachment to logical and metaphysical studies, the effects of which are so strikingly manifested in the philosophical character of his botanical investigations. At an early age he was sent to the Grammar-school of his native town, where among his contemporaries was a boy of kindred talents, the late Mr. James Mill, with whom he maintained through life an uninterrupted intimacy. In 1787 he was entered at Marischal College, Aberdeen, where he immediately obtained a Ramsay bursary in Philosophy; and about two years afterwards, on his father quitting Montrose to reside in Edinburgh, he was removed to the University of that city, in which he continued his studies for several years, but without taking a degree, although destined for the medical profession. At this early period the strong inclination of his mind to the study of Botany gained for him the favourable notice of the amiable Professor of Natural History, Dr. Walker, and he was induced, in the year 1791 (being then in the eighteenth year of his age), to lay before the Natural History Society, of which he was a member, his earliest Paper, containing an enumeration of such plants as had been discovered in North Britain subsequent to the publication of Lightfoot's 'Flora

Scotica,' with critical notes and observations. Although this Paper, like most of those read before the Society, was not intended for publication, it led to the communication of his specimens and observations to Dr. Withering, who was then engaged in the preparation of the second edition of his 'Arrangement of British Plants,' and laid the foundation of a warm and intimate friendship between them. In 1795, soon after the embodiment of the Fifeshire Regiment of Fencible Infantry, he obtained in it the double commission of Ensign and Assistant-surgeon, and proceeded with it to the North of Ireland, in various parts of which he was stationed until the summer of 1798, when he was detached to England on recruiting service. Fortunately for himself and for science, this service enabled him to pass several months, during this and the succeeding year, in London, where he availed himself to the utmost of the library and collections of Sir Joseph Banks, from whom his already established botanical reputation obtained for him a cordial reception. In 1799 he returned to his regimental duties in Ireland, from which he was finally recalled, in December of the following year, by a letter from Sir Joseph Banks, proposing for his acceptance the post of Naturalist in the Expedition for surveying the coasts of New Holland, then fitting out under the command of Captain Flinders. Within two days of the receipt of this letter, which placed within his reach the so-much coveted opportunity of devoting himself entirely to his favourite pursuit, he quitted the regiment and the military service; and in the summer of 1801 he embarked at Portsmouth, full of ardour and confident of success. His absence from England lasted more than four years, during which the southern, eastern, and northern coasts of New Holland, and the southern part of Van Diemen's Land, were thoroughly explored. In the month of October 1805 he arrived in Liverpool with a collection of dried plants amounting to nearly 4000 species, a large proportion of which were not only new to science, but exhibited new and extraordinary combinations of character and habit. Immediately on his arrival in England, he was appointed Librarian of the Linnean Society, of which he had been elected an Associate in 1798. During his voyage he had been indefatigable in describing with the minutest accuracy the whole of the materials which he had collected, and in the accumulation of a vast store of facts and observations in relation to their

structure and affinities, as well as to all the most important points in the anatomy and physiology of plants in general. The new views which were thus opened to him on a multitude of botanical subjects, he was enabled, by his position at the Linnean Society, and by the free and unrestricted access which was liberally accorded to him to the treasures of the Banksian Library and Herbarium, to enlarge and to perfect, and to lay them before the world in a series of masterly publications, which at once stamped upon him the character of the greatest and most philosophical botanist that England had ever produced. In 1810 appeared the first volume of his 'Prodromus Floræ Novæ Hollandiæ et Insulæ Van Diemen,' which was received by all the more profound botanists of this country and of the continent as the work of a mind thoroughly imbued with the principles of the Natural System, and giving to that system, which had hitherto found little favour out of France, a wider and a firmer basis. This important work, together with his Memoirs on *Proteaceæ* and *Asclepiadeæ*, which immediately followed, and his 'General Remarks, Geographical and Systematical, on the Botany of Terra Australis,' appended to the 'Narrative of Captain Flinders's Voyage,' published in 1814, by displaying in the most instructive form the superior advantages of the Natural System, whether in the monographic description of separate families, or in the comparison of the families with each other and with the entire mass of vegetation, gave new life to that system, and speedily led to its universal adoption. A series of Memoirs followed, chiefly in the Transactions of the Linnean Society, or in the appendices to various books of travel and survey, which gave fuller and more complete development to his views on almost every department of botanical science, and induced the illustrious Humboldt not only to confer upon him the title of "Botanicorum facile Princeps," but also to salute him with the more comprehensive and expressive designation conveyed in the dedication of the 'Synopsis Plantarum Orbis Novi,' "Roberto Brownio, Britanniarum Gloriæ atque Ornamento, totam Botanices Scientiam ingenio mirifico complectenti." At the close of the year 1810, on the death of his old and intimate friend, the laborious, accurate and learned Dryander, he succeeded to the office of Librarian to Sir Joseph Banks, who (on his death in 1820) bequeathed to him for life the use and enjoyment of his library and

collections. These were subsequently, in 1827, with Mr. Brown's assent, and in conformity with the provisions of Sir Joseph's will, transferred to the British Museum; and from this latter date to his death, a period of upwards of thirty years, he continued to fill the office of Keeper of the Botanical Collections in the National Establishment. Soon after the death of Sir Joseph Banks he had resigned the Librarianship of the Linnean Society, of which he then became a Fellow, and having been for many years one of its Vice-Presidents, was at last prevailed upon, in 1849, to allow himself to be elected President. This office he retained till 1853. He became a Fellow of the Royal Society in 1811, and was several times elected into the Council. In 1839 he received its highest honour in the Copley Medal, presented to him "for his discoveries during a series of years on the subject of vegetable impregnation." In the meantime honours and titles had flowed in upon him from all quarters; and nearly every scientific Society both at home and abroad felt itself honoured by enrolling his name in the list of its Members. In 1832, the University of Oxford conferred upon him, in conjunction with Dalton, Faraday, and Brewster, the honorary degree of D.C.L. In the succeeding year he was elected one of the eight Foreign Associates of the Academy of Sciences of the Institute of France, his name being selected from a list including those of nine other *savans* of world-wide reputation, nearly every one of whom has since been elected to the same distinguished honour. During the administration of Sir Robert Peel, he received, in recognition of his great eminence in botanical science, a pension on the Civil List of £200 per annum. The King of Prussia subsequently decorated him with the cross of the highest Prussian Civil Order, "Pour le Mérite."

Among the more important of his Memoirs above referred to, may be mentioned his Papers on *Compositæ*, on *Rafflesia*, and on the Fecundation of *Orchideæ* and *Asclepiadeæ*, in the Linnean Transactions; the botanical appendices to the Voyages or Travels of Tuckey, Parry, Franklin, Abel, King, and Denham; his Papers on Active Molecules, and on the plurality of Embryos in *Coniferæ*; and his contributions to Wallich's 'Plantæ Asiaticæ,' and to Horsfield's 'Plantæ Javanicæ.' Of his later publications, the most remarkable are his "Botanical Appendix to Captain Sturt's Expedition into Central Australia," published in 1849; and his Memoir

"On Triplosporite, an undescribed Fossil Fruit," published in the Linnean Transactions in 1851. The pervading and distinguishing character of all these writings is to be found in the combination of the minutest accuracy of detail with the most comprehensive generalization. No theory is propounded which does not rest for its foundation on the most circumspect investigation of all attainable facts. In perusing them, we are first struck with the evident completeness of the investigation, and next with the wonderful sagacity with which the ascertained facts are brought to bear upon the question at issue. And these distinguishing qualities are equally obvious throughout the wide range of objects treated of, whether in the anatomy, the physiology, the classification, the description, the distribution or the affinities of plants, and in the examination both of recent and fossil structures. Among the most important anatomical and physiological subjects of which they treat, particular mention is due to the discovery of the nucleus of the vegetable cell, and of the circumscribed circulation on the walls of particular cells; the development of the stamina, together with the mode of fecundation in *Asclepiadeæ* and *Orchideæ*; the development of the pollen and of the ovulum in Phænogamous plants, with the peculiarities of the latter in *Coniferæ* and *Cycadeæ*, and the bearing of these facts upon the general subject of impregnation; the origin and development of the spores of Mosses; and the discovery of the peculiar motions which take place in the "active molecules" of matter when seen suspended in a fluid under the microscope. Of structural investigations, the most important are those which establish the relation of a flower to the axis from which it is derived, and of the parts of a flower to each other, as regards both position and number; the analogy between stamina and pistilla; the neuration of the corolla of *Compositæ*, their æstivation and inflorescence; and the structure of the stems of *Cycadeæ*, both recent and fossil. To the study of fossil botany Mr. Brown was always strongly attached, and with a view to its prosecution he formed an extensive and valuable collection of fossil woods, which he has bequeathed under certain conditions to the British Museum. His collections in other departments were also considerable, and his library very extensive.

In private life Mr. Brown's character was thoroughly estimable. Shrinking, with instinctive modesty, from all public employments, whether professional or otherwise, which appeared to involve any-

thing like display, he was sometimes thought, by those who knew him little, to be cold, distant, and reserved; while those who were admitted to the privilege of his intimacy bear unanimous testimony to his unvarying kindness of heart, the genial warmth of his feelings, and the pure benevolence of his disposition. To a mind stored with anecdote he united a strong sense of humour, and a happy facility in its expression, which rendered him a most delightful companion. And when to these qualities we add his perfect simple-mindedness, his unswerving devotion to truth, and that singular uprightness of judgment, which rendered him on all difficult occasions a most invaluable counsellor, we shall easily perceive how it was that he became so warmly endeared to the hearts of his friends. From the death of Sir Joseph Banks, who bequeathed to him his house in Soho Square, he continued to occupy that portion of it which opened upon Dean Street; and it was in the library of that illustrious man, the scene of his labours for sixty years, surrounded by his books and by his collections, that he breathed his last, on the 10th of June in the present year, and in the eighty-fifth year of his age.

Sir James MacGrigor was born at Cromdale, in Strathspey, Inverness-shire, on the 9th of April, 1771. He received his literary education at Marischal College, Aberdeen, where he took his degree as M.A. He studied medicine at the University of Edinburgh, and afterwards, with a view to improve his knowledge of anatomy, attended the lectures and demonstrations of Mr. Wilson in London. He then obtained the Degree of M.D. from the Marischal College. In 1793 he entered the Army by the purchase of the Surgeoncy of the 88th Regiment, with which Corps he served in Holland and Flanders throughout the Duke of York's campaign. In 1796 he proceeded to the West Indies, where, with two companies of the regiment, he was engaged in the expedition against Grenada, and in August of the same year returned to England to rejoin head-quarters. In 1799 he accompanied the 88th to Ceylon, and subsequently to Bombay. The regiment formed part of the Anglo-Indian Army sent to Egypt under the command of Sir D. Baird, and arrived at Cosseir in June 1801. Dr. MacGrigor was Superintending Surgeon of the Force, and earned well-merited commendation by his zeal and intelligence, and his judicious arrangements for the sick and wounded. In 1803 he re-

turned home with the regiment, and was shortly afterwards appointed to the Oxford Blues, with which he did duty at Windsor for some time. In 1805 he was promoted to the rank of Deputy Inspector of Hospitals, and was employed in the south-western district. At Portsmouth he superintended the landing and treatment of the wounded sent home from Sir John Moore's Army. In August 1809 he was promoted to be Inspector-General of Hospitals, and in September was sent to Walcheren as Principal Medical Officer of the expedition, to replace Sir J. Webb, and was highly commended by Sir Eyre Coote for the manner in which he discharged his duty amidst great difficulties. In the end of the year, when that unfortunate expedition had terminated, he returned to Portsmouth, where he remained nearly two years. In 1811 he was sent out to Spain as Principal Medical Officer of the Army under Lord Wellington, and arrived in time to be present at the siege of Ciudad Rodrigo. He remained with the Army till the termination of the Peninsular War, and was present in every siege and engagement from Ciudad Rodrigo to Toulouse. The Duke of Wellington, who was at no time very lavish of his compliments to medical officers, thus notices Dr. MacGrigor's services in an Order dated 26th July, 1814 :—"I have every reason to be satisfied with the manner in which Mr. MacGrigor conducted the department under his direction, and I consider him one of the most able, industrious, and successful public servants I have ever met with."

On the termination of the war he was knighted, and received the Royal permission to wear the decoration of Knight Companion of the Portuguese Order of the Tower and Sword. In 1815 he was appointed Director-General of the Medical Department of the Army, which post he filled till 1851, when he retired from active employment. In 1831 he was created a Baronet, and in 1851 was appointed to be a Knight Commander of the Order of the Bath. He died on the 2nd of April, 1858, within a few days of entering his 88th year.

Sir James MacGrigor was the auther of a "Memoir of the state of health of the 88th and other Regiments at Ceylon and Bombay, from 1st June 1800 to 31st May 1801;" of a "Medical sketch of the Expedition to Egypt from India;" and of a "Sketch of the Medical History of the British Army in the Peninsula of Spain and Portugal during the late Campaign."

Shortly after his appointment to be Director-General, he organized a system of Returns from the different stations occupied by British troops, from which, after a lapse of twenty years, the Statistical Reports on the health of the Army were compiled. He also commenced, at the Invalid hospital at Chatham, a Museum of Natural History and Pathological Anatomy, which, by the contributions of the medical officers from all quarters of the world, has become one of great extent and value. But while Sir James was thus endeavouring to promote the interests of science through the instrumentality of the Department of which he was the head, he was not unmindful of the interests of the officers composing it. In 1816 he established a Society for providing pensions for the widows of medical officers in addition to those granted by Government, and one for affording assistance to the orphans of medical officers; both of which institutions have succeeded to an extent which could not fail to be gratifying to their founder.

Sir J. MacGrigor was for two successive years elected Lord Rector of Marischal College, Aberdeen. The University of Edinburgh conferred on him the honorary degree of LL.D. On the establishment of the University of London, he was nominated a member of the Senate. He was a Fellow of the Royal Society of Edinburgh, and member of various Medical Bodies. He was elected into this Society on the 14th of March, 1816.

Sir James MacGrigor was courteous and affable in his demeanour, and at all times accessible to the officers of his department, by whom he was much esteemed and respected. He retired into private life after having faithfully and efficiently served his country for the long period of fifty-eight years, during thirty-six of which he had been at the head of the Army Medical Service.

Hugh Lee Pattinson, Esq., was born at Alston in Cumberland, where his family, belonging to the class of smaller landholders of the neighbourhood, had long resided. He received his early education at the school of his native town; what further acquirements he made he owed to self-instruction. Having when a youth been present at a lecture on chemistry, his ready mind, deeply impressed with what he heard and saw, was inflamed with a love of the science, and he thenceforward gave himself earnestly to its pursuit, with the help of such books and rude apparatus as his scanty means afforded.

While still very young, Mr. Pattinson left Alston for Newcastle-on-Tyne, to occupy a situation in a soap-work, and his position there, though a subordinate one, afforded him facilities for pursuing his favourite study. A few years after this he was appointed Assay-Master to the Commissioners of Greenwich Hospital, the chief duty of his office being to inspect, as to quality and quantity, the ores which are levied as royalties from the extensive lead mines in his native district belonging to that establishment. It was while thus employed, and when his mind was directed to the improvement of metallurgic operations, that he was led to discover his admirable and now well-known process for extracting the silver from argentiferous lead. Returning to Newcastle after a few years, to undertake the management of Mr. Beaumont's lead-smelting and refining-works in that neighbourhood, he was enabled to put in practice his method of de-silvering lead, for which he took out a patent. The profits thence accruing afforded him the means to establish, in partnership with two of his friends, a chemical manufactory at Felling, which, through subsequent additions, has become one of the most extensive in the district; and at a later period he discovered and brought into practical use a method of separating magnesia from the limestone rock containing that earth, and a process for producing oxychloride of lead, a valuable pigment, directly from the ore.

But while thus engaged in improving industrial chemistry, Mr. Pattinson was not unconcerned in matters of more purely scientific interest; and it is more especially deserving of mention, that, contemporaneously with Mr. Armstrong, he was one of the first to give an account of the remarkable fact of the evolution of electricity by effluent steam. He was also attached to the study of astronomy; and although he took little part in its pursuit as a practical observer, he possessed an elegant observatory, furnished with a transit-instrument, and also an admirable equatoreal, which, as is known to many of the Society, he liberally lent to Professor Piazzi Smyth, to be used by that gentleman in his recent expedition to Teneriffe.

Mr. Pattinson was elected a Fellow of this Society on the 3rd of June, 1852; he belonged also to the Royal Astronomical, Geological, and Chemical Societies: his death took place at Scots' House, his residence near Newcastle, on the 11th of November, 1858.

The Very Rev. GEORGE PEACOCK, D.D., Dean of Ely, was born on the 9th of April, 1791, at Thornton Hall, Denton, in the parish of Gainford near Darlington, in the county of Durham, and about fourteen miles from Richmond in Yorkshire, being the residence of his father, the Rev. Thomas Peacock, incumbent and during fifty years perpetual curate of that parish, where he also kept a school. His family consisted of five sons and three daughters, three of the sons by a first marriage, and the other two, with the daughters, by a second; George being the youngest son of the five. In early youth he showed no precocity of genius, but was a bold and active lad fond of out-door sports, and, if remarkable for anything, rather for his daring feats in climbing, which sometimes led him into very dangerous situations, than for any special attachment to study. From the nature of his father's occupation, it is not probable that he lacked the usual elementary instructions; but his early reading was desultory, books of voyages and travels being most in favour with him; nor was it until, with a view to his future college career, he was sent at nearly seventeen years of age (in January 1808) to the school of the Rev. Mr. Tate (formerly a Fellow of Sydney Sussex College, Cambridge) at Richmond, that his great natural powers began to develope themselves. Here, however, he applied himself with diligence to the studies of the school, and with such success, that at the July examination he was placed alone, by a decided superiority, at the head of his class, in which it may be noticed were two boys who afterwards became Fellows, and four others who became Scholars of Trinity College. He did not live in Mr. Tate's house, but in lodgings near it, and had his evenings uninterrupted for study, which he used to such purpose as to have read far in advance of the classical course of the school, and to have obtained an accurate knowledge of the niceties of Greek criticism, as well as a habit of sound rendering both of the Greek and Latin classics. During one or more of the vacations, particularly the summer one of 1809, he also read mathematics with Mr., afterwards Dr. Brass, at that time a distinguished Undergraduate of Trinity, from the town and school of Richmond, and who subsequently took a Wrangler's degree. It would seem, however, that up to the period of his entry at Trinity College in October 1809, his mathematical reading had not extended much beyond the first year's subjects then studied at Cambridge. We

have the testimony of one of his schoolfellows, afterwards himself a distinguished ornament of the same University and College, that during his whole time at Richmond, "though a severe student, he was a joyous, sociable, and genial spirit, always ready for good companionship, for any pleasurable excursion, for manly exercise, and for all innocent mirth and playfulness." How well calculated, as a teacher, Dr. Tate must have been to bring forward the powers and to win the affectionate regard of his pupil, may be gathered from the terms in which their connexion is spoken of in the dedication of his first considerable mathematical work,—terms which indicate more than an ordinary community of feeling and facility of intercourse between the pedagogue and the pupil.

During the first year of his residence as an undergraduate at college, he does not appear to have applied himself with any extraordinary diligence to the studies of the place; but this temporary relaxation of energy was amply compensated during the remainder of his pupillage by a very extensive and conscientiously accurate course of mathematical reading, which issued in his taking the degree of Second Wrangler in January 1813. Shortly after the examination for the degrees, he also gained one of the Smith's prizes.

In 1812, being the earliest period at which, as a sizar of his college, he was allowed to compete, he obtained a Scholarship, and on his first offering himself as a candidate for a fellowship (in 1814), was elected to one of the only two then vacant, his extensive classical knowledge no doubt standing him in stead on that occasion. In the subsequent year he was appointed Assistant Tutor and College Lecturer; in 1823, Full Tutor, conjointly with Mr. Evans; and finally, in 1835, Sole Tutor of the "side" which bore his name in that great and venerable establishment, an office which he held till called away from the performance of its duties by his appointment to the Deanery of Ely in 1839, when he also took the degree of Doctor of Divinity, having been admitted into Holy Orders in or about the year 1817. In one of the summer vacations in this interval (1816) he visited Italy.

Of his conduct in the important and responsible office of tutor, there has never been but one opinion in the University. While his extensive knowledge and perspicuity as a lecturer maintained the high reputation of his college, and commanded the attention and

admiration of his pupils, he succeeded to an extraordinary degree in winning their personal attachment by the uniform kindliness of his temper and disposition, the practical good sense of his advice and admonitions, and the absence of all moroseness, austerity, or needless interference with their conduct. "His inspection of his pupils," says one of them, "was not minute, far less vexatious; but it was always effectual, and at all critical points of their career, keen and searching. His insight into character was remarkable."

It was impossible for any one, at the epoch of his undergraduacy, and for several years preceding that epoch, drawn on to read extensively in mathematics for the sake of the science itself, and thus becoming aware of the progress made on the continent in that department of knowledge, while at the same time subjected to the course of reading then pursued for the Senate-house examinations, not to become at the same time unpleasingly sensible to what we must now consider the discreditable state of Cambridge mathematics then prevalent. Peacock, in common with many other students of his own standing, was profoundly impressed with this, and resolved, so far as in him lay, to contribute towards remedying the evil. Accordingly we find him, so soon as relieved from the pressure of examinations, exerting himself vigorously in the cause of mathematical improvement. As a preliminary step towards introducing the continental methods and the spirit of the higher analysis, he joined with two fellow-students of his own year (Messrs. Babbage and Herschel) in the task, more useful than brilliant, of translating the smaller work of Lacroix on the differential and integral calculus. This translation, published at Cambridge in 1816, was followed by a copious collection of examples in 1820; and, the sale of both being rapid, contributed no doubt materially to further the object in view. His position as Moderator for 1817 supplied him with a powerful lever for urging forward this movement, and he was not backward in availing himself of it. In his questions for the Senate-house examination for that year, the differential notation of the continental analysts was for the first time *officially* employed in Cambridge; an innovation which passed not altogether without censure. How little this affected him will appear from the following extract of a letter to a friend, which we have before us, dated March 17, 1817.

"I assure you, my dear ——, that I shall never cease to exert

myself to the utmost in the cause of reform, and that I will never decline any office which may increase my power to effect it. I am nearly certain of being nominated to the office of Moderator in the year 1818–19*, and as I am an examiner in virtue of my office, for the next year I shall pursue a course even more decided than hitherto, since I shall feel that men have been prepared for the change, and will then be enabled to have acquired a better system by the publication of improved elementary books. I have considerable influence as a lecturer, and I will not neglect it. It is by silent perseverance only that we can hope to reduce the many-headed monster of prejudice, and make the University answer her character as the loving mother of good learning and science."

Nor was it only towards placing on a better footing the purely mathematical studies of the University that his aspirations were directed. In the best spirit of a faithful and devoted son of Alma Mater, he repudiated the idea of her approaching decrepitude, and contended for her progress in all the great lines of scientific distinction. He was one of the most zealous promoters of the establishment of an Astronomical Observatory at Cambridge, and succeeded, in spite of considerable opposition, in procuring the appointment of two successive Syndicates for the consideration of the subject, and finally in carrying it triumphantly through the Senate. The result, it need hardly be remarked, has brilliantly justified the effort. He was also one of the first members of the Cambridge University Philosophical Society founded in 1819,—a body, which has established a well-earned scientific reputation, and of which he held the office of Vice-President in 1831 and 1840, and of President in 1841–42. He was also one of the earliest members of the Astronomical Society, which he joined immediately on its foundation in 1820. In 1818 he became a Fellow of the Royal, and subsequently of the Geological Society.

In 1825–26 he contributed to the Encyclopædia Metropolitana an article on Arithmetic, which has been designated by one eminently qualified to form an opinion on every point of mathematical history, as "the most learned work on the history of that subject which exists," and which, entering as it does into the details of the arithmetical nomenclature, notation, and methods of every age and lan-

* This was the case. He was also Senior Moderator in 1821.

guage, must have been the result of a world of reading and toilsome antiquarian research. In 1830 he supplied by his treatise on Algebra one of the greatest deficiencies in our whole circle of mathematical reading,—that, namely, of a sound elementary work on that subject based on truly philosophical principles, and explaining the true gist and nature of symbolical reasoning, in its relation to ordinary arithmetic and the science of concrete numerical magnitude, and pointing out (on the principle of the 'Permanence of equivalent forms') the origin and the solution of many of those difficulties which were usually slurred over by the student, in a way little conducive to the formation of clear logical habits of thought. In this remarkable work, the ideas propounded by Buée, Argand, Mourey and Warren, respecting the geometrical interpretation of imaginary symbols, were for the first time presented to the student in an elementary treatise as part and parcel of the general subject, and as intimately interwoven in the very texture of the algebraic methods; thus preparing them to understand and appretiate those more abstruse and powerful systems of imaginary representation subsequently developed in the double and triple algebra of Professor De Morgan and the quaternions of Sir William Hamilton. A report which he presented to the British Association in 1834, "On the recent progress of certain branches of Analysis," afforded him the occasion of still further maturing his views of the subject; and finally, in 1842 and 1845, he published in two successive volumes a more elaborate and complete treatise, in which the purely arithmetical or technical view of algebra is presented quite separately from the purely symbolic or formal one, and which leaves little to desire in respect of metaphysical completeness, and nothing in that of lucid exposition. The position which he then held in the University, as Lowndean Professor of Mathematics (to which office he was elected in 1837), identifies this work with the University in which it was produced as a contribution to scientific literature of which it may well be proud.

In this, his capacity of Lowndes Professor, he at first gave a series of lectures on practical and theoretical astronomy; and when, by mutual arrangement with the Plumian Professor, these lectures, belonging more properly to the department of the latter, were given by that officer, he delivered a course on geometry, and for three suc-

cessive years attempted to form a class for a course on the principles of analysis and their application. Those who are conversant with the mode in which the mathematical studies of the junior members of the University are prosecuted, will not be astonished that the attendance was small. Not discouraged, he attempted to form a class for astronomy, but though at first successful, the attendance was not maintained in subsequent years.

In 1838 Professor Peacock was appointed a member of the Parliamentary Commission for considering the steps to be taken for the restoration of the Standards of Weight and Measure destroyed by the burning of the Houses of Parliament. To the duties of this Commission he gave his diligent attention, and it was indebted to him for many valuable and useful suggestions. Of the Second Commission, appointed in 1843 to carry out the report of the first by the construction of new standards, he was also a member.

In 1839 he was appointed to the Deanery of Ely, vacated by the death of Dr. Wood, and with this appointment ceased, of course, his connexion, as Tutor, with Trinity College, and his residence at Cambridge other than such as the duties of his Professorship required. In this position it is too little to say that he conscientiously devoted himself to the performance of its duties. He went into them with all the zeal of an earnest and pious spirit, and with all the energy and prudence of an able and practical administrator. The venerable and beautiful fabric of the Cathedral had fallen into grievous decay, and had even become endangered by neglect. Its restoration became one of his principal objects, for the accomplishment of which he exerted himself with such success, that it remains distinguished as one of the most beautiful specimens of our ecclesiastical architecture. He laboured hard to introduce, and he succeeded in effectually introducing into the city of Ely, in spite of much opposition, the sanitary measures required by the Public Health Act; the result being a material improvement of the recorded salubrity of the place. Its educational establishments, especially the schools more immediately connected with the Chapter, received from him the most assiduous attention and active support, and its public charities his vigilant supervision. These duties, however, neither withdrew him from the pursuit of science, nor from his favourite and cherished object of University Reform. In his Life of the late Dr. Young, and

in his collection and republication of his numerous and important papers and memoirs, originally printed either as separate works, or in the Transactions of this Society and various journals and periodical works, he has conferred a lasting benefit on Science while doing justice to one of its most distinguished ornaments. There can be no doubt that this work must have cost him a vast amount of labour. Few scientific writers, thinking so profoundly and arriving at such important conclusions, have adopted a form of exposition so obscure and difficult to follow as Dr. Young. The discussion of these memoirs in the Biographical volume of Dr. Peacock's work, however, shows that he had completely overcome this difficulty, and obtained a perfect appretiation both of their merit and method. In the Archæological department of this work he had for a coadjutor Mr. Leitch, who edited the volume devoted to Dr. Young's Hieroglyphical discoveries. This work occupied him at intervals spread over a period of twenty years, and was only published in 1855, three years before his own decease.

Dr. Peacock was an active member of both the Cambridge University Commissions (of 1850 and 1855). Earnestly devoted to the improvement of the University system, he had early made its statutes and history an object of especial study, and had stated, in the form of observations published in 1840 on its constitution and studies, and in 1841 on its statutes, the result of his impressions on a variety of points in which he conceived amelioration practicable. He came therefore to this arduous and by no means popular duty fully prepared, by intimate practical acquaintance with the working of the then existing system, and by long meditation, resulting in an entire conviction of the desirableness of a very considerable amount of change in the directions indicated in the Report of the first Commission. These views he throughout supported, however, with perfect candour and moderation, and with an earnest desire, as far as possible, to conciliate opposition, and to wound no private or individual feeling.

In 1841 he accepted the office of Prolocutor of the Lower House of the Convocation of Canterbury, which he filled till 1847, and again from 1852 to 1857; an office for which the well-known temperateness of his views on all those subjects where, in imperfectly-balanced minds, strong feeling is apt to degenerate into passionate

advocacy, the weight of his character, and the uniform dignity (combined as it always was with exceeding courtesy and gentleness) of his personal bearing, peculiarly fitted him.

His health, which in the earlier days of his residence at Cambridge, after taking his Bachelor's degree, had not been strong, latterly gave way under the influence of repeated attacks of influenza and bronchitis, which necessitated his passing the winters in warmer residences. That of 1848 he passed in Madeira with every promise of permanent benefit. The disorder, however, recurred in succeeding winters, and was aggravated in 1857 by an attack of dysentery. On the 28th of October in the present year he attended a meeting of the University Commission, from which returning, he took to his bed, exhausted by the effort, to rise no more—a striking comment on the expressions used by him in his letter above cited. His decease took place on the 8th of November, 1858.

Dr. Peacock married, in 1847, Frances Elizabeth, second daughter of W. Selwin, Esq., Q.C. He has left no family. He was for several years a Vice-President of this Society; in 1830–31, and various subsequent sessions down to 1856–57, he acted as a Member of the Council. Few men have left behind them a memory more cherished, or been attended through life by more universal manifestations of affectionate regard and reverential esteem.

Major-General Sir William Reid, K.C.B., was born on the 25th of April, 1791: his father was a Minister of the Established Church of Scotland, at Kinglassie, in Fife, and with slight previous advantages of education, he was sent, soon after he entered his fifteenth year, to the Royal Military Academy at Woolwich. Young Reid made rapid progress, completed his course of study before he had attained his eighteenth year, and was sent, as was at that time the custom, to the Ordnance Survey, then directed by Colonel Mudge, Royal Artillery: in February 1809, he was commissioned in the Royal Engineers. In those stirring times the interval was short between the hall of study and the field. Lieutenant Reid joined the army of Wellington in 1810, was present at the first unsuccessful siege of Badajoz in April 1811, and at the final capture of that fortress twelve months later. Early and continuously conspicuous for his zeal, intelligence and energy, even among the very many young officers of Engineers who greatly distinguished themselves in that

war, he took part, while yet a subaltern, in the sieges of Ciudad Rodrigo, Burgos, and St. Sebastian, in each of which he was wounded, and in the battles of Salamanca, Vittoria, Nivelle, Nive, and Toulouse. He did not obtain his Captaincy until 1814. He was present at the bombardment of Algiers under Lord Exmouth in 1816; and he took an active part, twenty years later, in the operations of Sir de Lacy Evans in Spain, where he commanded the Engineers of the British auxiliary force.

Ever ready, however, as he was, to follow the leadings of his own profession, his active mind was not less alive to its scientific interests. He was the contributor of nine papers to the 'Professional Papers' of the Royal Engineers, usually on technical subjects; but sometimes on subjects, such as the movement of the shingle along our coasts, which are more nearly related to his favourite studies. It was in 1832 that his mind first received the bias which he afterwards followed with so much distinction and success. It fell to his lot, as the officer of Engineers at Barbadoes, to have to re-establish the Government buildings blown down in the hurricane of the 10th of August, 1831: no less than 1477 persons out of a population of about 130,000 lost their lives on that occasion, and property to the value of more than £1,600,000 was destroyed. The devastation and misery he witnessed, led him, in his own words, "to search everywhere for accounts of previous storms, in the hope of learning something of their causes and mode of action." In this he was materially assisted by the previous labours of Mr. Redfield of New York, who, as early as 1831, had published in the 'American Journal of Science' the first of a numerous series of papers in which he demonstrated, not only that the storms of the American coast were whirlwinds, in opposition to high authorities, who maintained that the direction of the wind is rectilinear, but also traced some of them from the West Indies to the sea-board of the United States, and proved that they were progressive whirlwinds, moving forward on curved tracks with a considerable velocity. Fully acknowledging his obligations to this great meteorologist, Lieut.-Colonel Reid set himself to confirm and extend his deductions, by a laborious collation of the log-books of British men-of-war and merchantmen. Impressed also with the idea that to the south of the equator, "in accordance with the regularity nature follows in all her laws, storms would be found to move in a directly contrary direction," he endeavoured to collect

such facts as would aid further inquiry on that subject. None but those who have attempted a like task can fully appreciate its difficulties,—observations which the investigator dare not reject, although convinced that they are wrong, provoking silence where a word would clear up a doubt,—still more provoking record of useless details, to the omission of those that are important; nevertheless he persevered, and, gaining confidence in the key he had obtained to the real nature of these intricate phenomena, he ventured in 1838 to lay down, for the guidance of the seaman, those broad general rules of navigation which are known as the law of storms. He showed that it is possible to deduce from the facts, rules applicable to every emergency; to tell unerringly when ships must run before the hurricane, when they must lie to, and on which tack, so as to avoid being taken aback by the veering of the wind; lastly, how to anticipate its coming changes, and shape the course which best turns them to account.

The announcement of this law, so important to the mariner, and to every naval and commercial nation, was received with the greatest interest by the scientific world; and Lieut.-Colonel Reid's work, entitled 'An Attempt to develope the Laws of Storms,' has gone through several editions, and has been translated even into Chinese.

Lieut.-Colonel Reid was appointed Governor of Bermuda in 1839, an opportunity which he did not fail to improve for pursuing his inquiries: he was transferred to the Government of the West Indies in 1847. Happening at the latter station to entertain the late Dr. Fownes of University College, he induced that eminent chemist to draw up a treatise on rudimentary chemistry for the use of his newly-founded School of Practical Chemistry at Barbadoes: this treatise, which the author presented to him, he first printed for local use, then presented to Mr. Weale, in reference to a design for a series of cheap popular treatises on scientific subjects which he had long previously discussed with that gentleman. It was the parent of the extensive and very valuable series of rudimentary works since brought out by Mr. Weale; but, with characteristic modesty, he requested the suppression of a notice to that effect, which may be seen in the first edition of Dr. Fownes's treatise. Resigning the government of the West Indies, on grounds highly honourable to his sense of independence, Lieut.-Colonel Reid resumed his military duties, and was serving as Commanding Engineer at Woolwich when he was selected

for the difficult post of Chairman of the Executive Committee of the Great Exhibition of 1851. It has been said that his singular simplicity of manner and total absence of pretension caused the distinguished men, with whom he was associated on that occasion, to wonder at first what had led to his selection for the office. They soon discovered, under that simplicity, the patient but genuine enthusiasm, the varied experience, the calm and even temper, and the devotion to the duties of the moment, whatever they might be, which eminently fitted him for it. It is not too much to say that his judicious arrangements contributed materially to the success of that great undertaking, and they were fitly rewarded by the ribbon of K.C.B., and his appointment to the important military command of Malta. To that island Sir William Reid carried all the unostentatious activity which had distinguished his former governments. In a time of extraordinary difficulty, when Malta becoming an *entrepôt* of the first importance to the British Army in the East, all its resources were strained to the utmost, he managed to meet every demand, and while he restrained the political excitements of the day, to carry forward homely designs for the permanent benefit of the people. Thus he founded a botanical school for the working classes; he imported improved agricultural implements; he introduced a new species of the cotton plant, and other seeds adapted to the climate; he established barometers in public places to warn the Maltese fishermen of impending gales; he took in hand the Library of the old Knights of Malta, and by the introduction of modern books, fitted it to be a true public library for a large community. Whatever attainable practical object commended itself to his judgement, that he undertook, with the same quiet determination which in 1851 enabled him to falsify adverse predictions and attain the object to which he was pledged, in the punctual opening of the Great Exhibition.

The Government of Malta was the last public service of Sir William Reid. He returned home in 1858, having two years previously attained the rank of Major-General, and died after a very short illness on the 31st of October. He was elected a Fellow of the Royal Society in 1839, and was appointed Vice-President in 1849.

Sir William Reid was married to a daughter of the late Mr. Bolland of Clapham. His wife died a few months before him, and he has left five daughters.

Logocyclic Curve by a continuous motion; and a very ingenious instrument has been contrived by Mr. Henry Johnson of Crutched Friars, to describe the spiral of Archimedes, which is as simple as it is effective.

June 17, 1858.

The LORD WROTTESLEY, President, in the Chair.

The Earl Granville, Professor Hennessy, and the Rev. Samuel Haughton were admitted into the Society.

In accordance with notice given at the last Meeting, the Earl of Rosse proposed the Right Hon. Sir John Pakington, Bart. for election and immediate ballot.

The Ballot having been taken, Sir John Pakington was declared duly elected.

The following communications were read :—

I. "On the Problem of Three Bodies." By CHARLES JAMES HARGREAVE, LL.D., F.R.S. Received May 3, 1858.

(Abstract.)

The author states that the principal object of this memoir is to set forth two new methods of treating the dynamical equations by processes of variation of elements, differing from the ordinary processes of this nature principally in this particular, that the variations are represented in explicit terms of the elements themselves and of the time, and not through the medium of partial differential coefficients. It has been his object to render the processes as elementary as possible; and to preserve them in a rigorous form, by postponing all attempts at approximation until the formulæ are actually applied to practical problems. The applications given in the paper comprise the circular and spherical pendulums, and the planetary and lunar theories, and a special theorem as to the movement of the plane of a planet's motion under the influence of several other planets.

The original normal problem which is taken as the basis, is that

of motion about a fixed centre of force, where the force is directly as the distance; or, in other words, the system of equations not exceeding three in number, of the form

$$x''+n^2x=0;$$

whose solutions are represented under the form

$$x=\lambda_a\, a\cos(nt+\rho)+\mu_a\, b\sin(nt+\rho),$$
$$y=\lambda_b\, a\cos(nt+\rho)+\mu_b\, b\sin(nt+\rho),$$
$$z=\lambda_c\, a\cos(nt+\rho)+\mu_c\, b\sin(nt+\rho);$$

where

$$\lambda_a=\cos\phi\cos\psi-\sin\phi\sin\psi\cos\iota,$$
$$\lambda_b=\cos\phi\sin\psi+\sin\phi\cos\psi\cos\iota,$$
$$\lambda_c=\sin\phi\sin\iota;$$
$$\mu_a=-\sin\phi\cos\psi-\cos\phi\sin\psi\cos\iota,$$
$$\mu_b=-\sin\phi\sin\psi+\cos\phi\cos\psi\cos\iota,$$
$$\mu_c=\cos\phi\sin\iota;$$

to which are afterwards added,

$$\nu_a=\sin\psi\sin\iota,$$
$$\nu_b=-\cos\psi\sin\iota,$$
$$\nu_c=\cos\iota.$$

These are the equations of an ellipse whose centre is at the force, and situated in a plane inclined at the angle ι to the plane of $x\,y$, and the longitude of whose node is ψ; and ϕ is the angular distance of the major axis of the ellipse from the node; a and b are the semi-axes of the ellipse; and ρ is the angular distance, from the major axis, of the zero-point of the motion, measured on the circle described on the major axis. A uniform motion around the circle represents the place of the body by the corresponding point on the ellipse, where it is cut by a perpendicular dropped on the major axis.

If the force be not situated at the origin, but at the point (X, Y, Z), we have merely to substitute $x-X$ for x, &c. in the above equations of motion and solutions.

It is then shown that a system of the form

$$x''+n^2x=P_x,\ \&c.,$$

where n^2 and P_x, P_y, and P_z are any variables, may be solved by the

same set of final integrals, and the same values of x', y', and z', by supposing the elements a, b, ϕ, ψ, ι, and ρ to become variable. These elements are those of an ellipse tangential to the actual curve of motion; and the following formulæ are obtained for their variation:—

Let

$$\lambda_a \mathrm{P}_x + \lambda_b \mathrm{P}_y + \lambda_c \mathrm{P}_z = (\mathrm{P}_x),$$
$$\mu_a \mathrm{P}_x + \mu_b \mathrm{P}_y + \mu_c \mathrm{P}_z = (\mathrm{P}_y),$$
$$\nu_a \mathrm{P}_x + \nu_b \mathrm{P}_y + \nu_c \mathrm{P}_z = (\mathrm{P}_z);$$

and let (putting T for $nt+\rho$),

$$a \cos\phi \cos \mathrm{T} - b \sin\phi \sin \mathrm{T} = \xi,$$
$$a \sin\phi \cos \mathrm{T} + b \cos\phi \sin \mathrm{T} = \eta;$$

then

$$nab\,\delta\iota = \xi(\mathrm{P}_z),$$
$$nab \sin\iota\,\delta\psi = \eta(\mathrm{P}_z),$$
$$\delta(nab) = a \cos \mathrm{T}(\mathrm{P}_y) - b \sin \mathrm{T}(\mathrm{P}_x),$$
$$\tfrac{1}{2}\delta(n^2(a^2+b^2)) = -n(a \sin \mathrm{T}(\mathrm{P}_x) - b \cos \mathrm{T}(\mathrm{P}_y)) + r^2 nn',$$
$$(a^2-b^2)\,(\delta\phi + \cos\iota\,\delta\psi) = \frac{1}{n}(b \cos \mathrm{T}(\mathrm{P}_x) - a \sin \mathrm{T}(\mathrm{P}_y)) + 2ab \sin \mathrm{T} \cos \mathrm{T}\frac{n'}{n},$$
$$(a^2-b^2)\delta(nt+\rho) = -\frac{1}{n}(a \cos \mathrm{T}(\mathrm{P}_x) - b \sin \mathrm{T}(\mathrm{P}_y)) - (a^2+b^2) \sin \mathrm{T} \cos \mathrm{T}\frac{n'}{n}.$$

It may be observed that ξ and η are coordinates of the body referred to the plane of the tangential ellipse, and to an axis of ξ coinciding with the node.

This method is denominated the method of Tangential Variation; and it is applied directly to the problem of the circular pendulum, that of the spherical pendulum, and that of the motion of a particle where the force is a function of the distance, and in particular that of elliptical motion, where the law of force is that of the inverse square.

In a subsequent part of the paper it is shown that a system of the form

$$x'' + n^2(x - \mathrm{X}) = 0, \text{ \&c.},$$

where n^2, X, Y, and Z are any variables, may be solved by the same set of final integrals, and the same values of x', y', and z', as those which have been already given as the solutions of the same system when n, X, Y, and Z are constant, by supposing the elements to

become variable. In such a case, the elements are those of an ellipse osculating with the actual curve of motion, always of course having its centre at the moveable point (X Y Z). The following formulæ are obtained for the variation of these elements :—

Let

$$\lambda_a X' + \lambda_b Y' + \lambda_c Z' = (X'),$$
$$\mu_a X' + \mu_b Y' + \mu_c Z' = (Y'),$$
$$\nu_a X' + \nu_b Y' + \nu_c Z' = (Z');$$

then

$$nab\,\delta\iota = (Z')\frac{d\xi}{dt},$$

$$nab \sin\iota\,\delta\psi = (Z')\frac{d\eta}{dt},$$

$$\delta(nab) = -n((X')\,b\cos T + (Y')\,a\sin T),$$

$$\tfrac{1}{2}\delta(n^2(a^2+b^2)) = -n^2((X')\,a\cos T + (Y')\,b\sin T) + r^2nn',$$

$$(a^2-b^2)(\delta\phi + \cos\iota\,\delta\psi) = -((X')\,b\sin T + (Y')\,a\cos T) + 2a\,b\sin T\cos T\frac{n'}{n},$$

$$(a^2-b^2)\,\delta(nt+\rho) = (X')\,a\sin T + (Y')\,b\cos T - (a^2+b^2)\sin T\cos T\frac{n'}{n};$$

in which $\frac{d\xi}{dt}$ and $\frac{d\eta}{dt}$ are the differential coefficients of the expressions for ξ and η, taken explicitly with regard to t.

This method is denominated the method of Osculating Variation.

Applying the method of tangential variation to the system

$$x'' + \frac{\mu}{r^3}x = 0, \text{ \&c.},$$

we perceive that this system admits of complete solution in finite terms, leading in fact to the usual theory of elliptical motion. Taking this system, therefore, as a normal system, the author proceeds to deduce the formulæ for the variation of the elements of this system, in order to arrive at the solution of the system

$$x'' + \frac{\mu}{r^3}x = P_x, \text{ \&c.}$$

The elements which have been selected, for reasons fully explained in the paper, are ι and ψ, whose meanings are already known; A and Nτ denoting respectively the mean distance, and the longitude of the epoch measured in the plane of the tangential ellipse as it exists at the time t, and measured from the node at that time; and e and ϖ denoting respectively the eccentricity of the tangential

ellipse, and the longitude of its perihelion measured as above; and it is observed that these are strictly normal elements, according to Professor Donkin's definition of normal elements.

The variations of these elements are then rigorously found, and are expressed as follows:—Denote

$$\cos\psi\, P_x + \sin\psi\, P_y \text{ by the symbol } P_\xi,$$

and

$$\cos\iota\,(\cos\psi P_x - \sin\psi P_y) + P_z \sin\iota \text{ by the symbol } P_\eta;$$

and let

$$-P_\xi \sin\theta + P_\eta \cos\theta = P_{\xi,\theta}; \quad -P_\xi \sin\varpi + P_\eta \cos\varpi = P_{\xi,\omega};$$

$$P_\xi \cos\theta + P_\eta \sin\theta = P_{\eta,\theta}; \qquad P_\xi \cos\varpi + P_\eta \sin\varpi = P_{\eta,\omega};$$

then

$$\delta\iota = \frac{r\cos\theta}{NA^2(1-e^2)^{\frac{1}{2}}}(P_z),$$

$$\sin\iota\,\delta\psi = \frac{r\sin\theta}{NA^2(1-e^2)^{\frac{1}{2}}}(P_z),$$

$$\delta e = \frac{r}{NA^2(1-e^2)^{\frac{1}{2}}}\left\{(\cos(\theta-\varpi)+e)P_{\xi,\theta} + (1+e\cos(\theta-\varpi))P_{\xi,\omega}\right\},$$

$$e(\delta\varpi + \cos\iota\,\delta\psi) = \frac{r}{NA^2(1-e^2)^{\frac{1}{2}}}\left\{\sin(\theta-\varpi)\,P_{\xi,\theta} - (1+e\cos(\theta-\varpi))\,P_{\eta,\omega}\right\},$$

$$\delta A = \frac{2}{N(1-e^2)^{\frac{1}{2}}}(P_{\xi,\theta} + e\,P_{\xi,\omega}),$$

$$\delta(N(t+\tau)) = \frac{r}{NA^2 e}\left\{-\sin(\theta-\varpi)\,P_{\xi,\theta} + (1+e\cos(\theta-\varpi))P_{\eta,\omega} - 2eP_{\eta,\theta}\right\};$$

which are capable of being expanded in terms of the elements, and t by means of the ordinary expressions for r, θ, and $\theta-\varpi$ in terms of the same quantities. The values of the elements at the time t being supposed to be found, by the integration of these formulæ, in terms of t, and their initial values, are to be substituted in the ordinary expressions for the coordinates, so as to obtain their values at the time t.

The author exhibits the application of the preceding formulæ to certain simple examples, and then proceeds to apply them to the planetary theory. For two planets (distinguished by the suffixes 2 and 3) supposed to move in the same plane, the following are the rigorous expressions for the variations. Let α_2 and α_3 be the ratio of the mass of each planet to that of the central body. Let P denote the cube of $r_3 \div r_{23}$, and let $(P-1)\sin(\theta_3-\theta_2)$ be called Q, and

$(\mathrm{P}-1)\cos(\theta_3-\theta_2)-\frac{r_2}{r_3}\mathrm{P}$ be called R; then

$$\delta e_2=\alpha_3\frac{\mathrm{N}_2\mathrm{A}_2 r_2}{r_3^2(1-e_2^2)^{\frac{1}{2}}}\Big\{(\cos(\theta_2-\varpi_2)(2+e_2\cos(\theta_2-\varpi_2))+e_2)\mathrm{Q}$$
$$+\sin(\theta_2-\varpi_2)(1+e_2\cos(\theta_2-\varpi_2))\mathrm{R}\Big\},$$

$$e_2\,\delta\varpi_2=\alpha_3\frac{\mathrm{N}_2\mathrm{A}_2 r_2}{r_3^2(1-e_2^2)^{\frac{1}{2}}}\Big\{\sin(\theta_2-\varpi_2)(2+e_2\cos(\theta_2-\varpi_2))\mathrm{Q}$$
$$-\cos(\theta_2-\varpi_2)(1+e_2\cos(\theta_2-\varpi_2))\mathrm{R}\Big\},$$

$$\delta\mathrm{A}_2=2\alpha_3\frac{\mathrm{N}_2\mathrm{A}_2^3}{r_3^2(1-{}_2^2)^{\frac{1}{2}}}\Big\{(1+e_2\cos(\theta_2-\varpi_2))\mathrm{Q}+e_2\sin(\theta_2-\varpi_2)\mathrm{R}\Big\},$$

$$\delta(\mathrm{N}(t+\tau))=-(1-e^2)^{\frac{1}{2}}\delta\varpi-2\alpha_3\frac{\mathrm{N}_2\mathrm{A}_2 r_2}{r_3^2}\mathrm{R}.$$

From these formulæ, the secular variations of the elements are obtained without difficulty; and a new method of integrating the equations for the variations of the eccentricity and longitude of perihelion is given.

The author then enters upon a minute examination of the mathematical character of secular variations, and their bearing upon the methods of approximation to which the problem of three bodies has given rise. It is pointed out that the disturbance finally effected through the medium of a secular variation is not of the order of the disturbing force, or rather of the ratio of the disturbing force to the central force; but that it may remain precisely the same, though this ratio should be diminished or increased without limit. The difference affects not the aggregate amount of deviation or disturbance caused, but the time in which this aggregate amount is produced. If we consider the undisturbed problem of two planets about a sun as representing motion in two planes inclined to each other at the angle I, and in ellipses having eccentricities e_2 and e_3, it is shown that, no matter how small or how large may be the disturbing force produced on each orbit by the other planet, the aggregate amount of disturbance of the planet m_2 is of the order of the quantities I and e_2, and that of the planet m_3, of the order of I and e_3. From considerations of this nature, which are dwelt upon at length in the memoir, the author concludes that the ordinary direct methods of solution by approximation, being based upon the erroneous assumption that the variations of the coordinates are of the order of the disturbing

force, are not, in a mathematical sense, legitimate processes; and that, in the planetary theory, they produce results practically true only on account of the minuteness of the disturbing forces, and the consequent great length of the secular periods; and that, in the lunar theory, their failure is made evident, in consequence of the comparatively large magnitude of the disturbing force, and the consequent rapidity with which the elements of the moon's orbit pass through their secular periods.

The formulæ for the variations of the elements are then applied to the lunar theory; and some of the integrations are effected by means of a lemma containing the solution of the differential equation

$$\frac{d\phi}{dt}=\mathrm{F}\cos(pt-q\phi)$$

(where F, p and q are numerical coefficients), in the form

$$\cos(pt-q\phi)=\frac{q\mathrm{F}+p\cos \mathrm{M}t}{p+q\mathrm{F}\cos \mathrm{M}t},\ \mathrm{M}\text{ being }\left(1-\frac{q^2\mathrm{F}^{2}}{p^2}\right)^{\frac{1}{2}}p.$$

By this method, the total motion of the moon's perigee, as well as the coefficients of the evection, are fully obtained in the first instance, without the necessity of any second approximation; and the usual difficulty as to the movement of the perigee does not present itself. The motion of the node, and the evection in latitude, are correctly obtained in a similar manner.

This part of the memoir is concluded by an extension of the general formulæ for the tangential variation of elements to the case in which we suppose the constant μ to become variable, the result being to add to each variation a term involving $\delta\mu$.

The third part of the Paper contains the development of the method of osculating variation, before briefly described; from which are deduced the formulæ for the osculating variations of elliptic elements. This method is capable of being applied to the planetary and lunar theories, as well as that of tangential variation; but the advantages of this method did not appear to be such as to justify the actual expansion of the formulæ for these theories. The author, however, shows that with reference to any system of three bodies, the equations of motion for each body naturally assume the form

$$x''+n^2(x-\mathrm{X})=0,\ \&c.$$

(being the system solved by this method); and that the X, Y, and

Z are absolutely the same for each of the three bodies. This is shown by demonstrating, that at any given moment the three lines which represent the direction of the force acting on each of the three bodies all pass through the same point, which is denominated the centre of force. The coordinates of this common centre of force are,

$$X=\frac{(23)x, +(31)x_2 +(12)x_3}{(23) +(31) +12};$$

with similar expressions for Y and Z; (12) being $r_{12} \div m\, m_2\, \chi r_{12}$, χ denoting the law of force, &c. Each body has its own value of n^2; their ratios being denoted by the proportion

$$n_1^2 : n_2^2 : n_3^2 : : \frac{r_{23}}{\chi r_{23}} : \frac{r_{31}}{\chi r_{31}} : \frac{r_{12}}{\chi r_{12}}.$$

The invariable plane of this system of three bodies is then found; and it is shown that the nodes of the three orbits upon this plane are always in a certain relative position, constituting a kind of triangle of equilibrium about the centre of force; resulting, in the limiting case where one of the three bodies is infinitely larger than the other two (or in what is denominated the undisturbed Problem of Three Bodies), in an exact opposition of the two nodes of the orbits of the latter two bodies upon the invariable plane of the system.

The formulæ for the osculating variation of elements are then applied to a system of three bodies, of which one possesses a predominating magnitude, so far as is necessary to determine the movement of the planes of the orbits; and it is readily shown that, if we consider only the first order of the disturbing force, the inclination of the plane of each orbit to the invariable plane is absolutely constant; and that the two nodes are always in opposition to each other, and move with a uniform angular velocity round the invariable plane.

This theorem is then extended to a system of n bodies moving about a central predominant body; and it is shown that the aggregate effect of the disturbing forces of such a system upon the plane of any one of the bodies can always be represented by stating that its node upon a certain fixed plane revolves with a uniform angular velocity, the plane of the orbit always remaining at the same inclination to the fixed plane. The rate of this angular move-

ment, and the coordinates of the fixed plane upon which the movement takes place, are found by means of formulæ of remarkable simplicity. These three quantities may be ascertained once for all for each planet (viz. the inclination of the fixed plane on which the node moves to any coordinate plane, the longitude of the node of the fixed plane in relation to any coordinate line, and the angular rate of movement of the node of the orbit upon this fixed plane), and, when once ascertained, may be regarded as fixed elements of the planet, from which the position of the plane of its orbit can always be determined without the use of tables.

II. "Description of some Remains of a Gigantic Land-Lizard (*Megalania prisca*, Ow.) from Australia." By Prof. Richard Owen, F.R.S. Received May 13, 1858.

(Abstract.)

The subject of this communication forms part of a collection of fossil remains from Australia, recently acquired by the British Museum, and demonstrates the former existence in that continent of a land-lizard considerably surpassing in bulk the largest species now known. The characters are chiefly derived from vertebræ, partially fossilized, equalling in size those of the largest existing Crocodiles; they are of the 'procœlian' type, but present lacertian modifications, and closely agree with those in the great existing 'Lace-lizard' of Australia (*Hydrosaurus giganteus*, Gray), of which individuals upwards of six feet long have been taken. A generic or subgeneric distinction is indicated by the comparatively contracted area of the neural canal, and by the inferior development of the neural spine, of the fossil vertebræ, which have belonged to an individual not less than twenty feet in length, calculated from the vertebræ and proportions of the body of the existing *Hydrosauri*. For this, probably extinct lizard, the name of *Megalania prisca* is proposed.

The results of an extended series of comparisons of its vertebræ with those of recent and extinct Sauria are given; and the paper is illustrated by drawings of the vertebræ of *Megalania* and those of *Hydrosaurus*.

III. "Notes of Researches on the Poly-Ammonias."—No. III. Contributions towards the History of the Diamides; Cyanate and Sulphocyanide of Phenyl. By A. W. Hofmann, Ph.D., F.R.S. &c. Received May 7, 1858.

About ten years* ago, when engaged in the study of aniline, I discovered two beautiful crystalline compounds, carbanilide and sulphocarbanilide, which can be produced by a variety of processes. The former is best prepared by the action of phosgene-gas on aniline, while the latter is most readily and most abundantly procured by the action of bisulphide of carbon on aniline. The composition and the constitution of these bodies is indicated by the formulæ—

$$\text{Carbanilide} \ldots\ldots\ldots C_{26}H_{12}N_2O_2 = \left.\begin{array}{l}(C_2\,O_2)''\\(C_{12}\,H_5)_2\\H_2\end{array}\right\}N_2,$$

$$\text{Sulphocarbanilide} \;..\; C_{26}H_{12}N_2S_2 = \left.\begin{array}{l}(C_2\,S_2)''\\(C_{12}\,H_5)_2\\H_2\end{array}\right\}N_2.$$

They may be viewed as derived from two molecules of ammonia (diammonia) in which two equivalents of hydrogen are replaced by two molecules of phenyl, and two other equivalents by the biatomic molecules C_2O_2 and C_2S_2.

The two substances in question, as far as their formulæ are involved, obviously correspond to urea and sulphocyanide of ammonium:—

$$\text{Urea} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots C_2H_4N_2O_2 = \left.\begin{array}{l}(C_2O_2)''\\H_2\\H_2\end{array}\right\}N_2,$$

$$\text{Sulphocyanide of ammonium} \;..\; C_2H_4N_2S_2 = \left.\begin{array}{l}(C_2S_2)''\\H_2\\H_2\end{array}\right\}N_2.$$

In their formation likewise a certain analogy with urea and sulphocyanide of ammonium may be recognized; for recent experiments have proved that urea is actually produced by the action of phosgene-gas on ammonia, while the formation of sulphocyanide of ammonium by means of ammonia and bisulphide of carbon is a long-established fact. The analogy, however, seems to disappear altogether, if the

* Journal of the Chemical Society, vol. ii. 36.

John Forbes Royle, the only son of Capt. W. Royle, H.E.I.C., was born at Cawnpore in 1800; educated at the High School, Edinburgh, he was originally intended for the army, but whilst waiting for an appointment to Addiscombe he studied medicine under Dr. A. T. Thomson, and from him derived, in great part, that love for natural history which led him eventually to relinquish the profession of arms and to adopt that of physic. In 1822 he went out to India as an Assistant-Surgeon on the Bengal establishment, in charge of troops, and the following year was appointed to the medical duties of the station at Saharunpore, together with the superintendence of the Company's Botanical Gardens at that place; a post which he held for nearly ten years. Here he enjoyed ample opportunities for studying the natural history of the Northern provinces, and employed all the time which he could spare from the active duties of his profession in collecting specimens of plants, vegetable productions, and minerals; in amassing information of every kind bearing on the arts, commercial produce, and medicines of India; and in minutely observing the phenomena of tropical vegetation, as influenced by soil, climate, cultivation, and surrounding circumstances.

Dr. Royle returned to England in 1831 on furlough, bringing with him collections of great extent and value; and for several years was engaged in the study, examination, and arrangement of the materials he had collected, and in generalizing and digesting the facts he had observed. The result of these labours was published in 1839, in his "Illustrations of the Botany and Natural History of the Himalaya Mountains and Cashmere," a work remarkable for the large amount of useful and practical information which it contains; including, at the same time, an elaborate systematic account of the botany of these parts of India, enlarged and comprehensive views of the climate of the country and the influences of meteorological phenomena on its vegetation, and abundant and minute details of the various vegetable productions forming articles of export, or used in the arts and manufactures of the natives. Especially valuable too, in a practical point of view, are the technical generalizations with which the work abounds; in which the careful study of years is brought to bear on the economical production of cotton, tea, corn, and other similar substances; the exact laws of scientific research being employed to indicate new and improved methods of cultivation or production.

In 1837, on the retirement of Dr. Paris as Professor of Materia Medica in King's College, London, Dr. Royle was elected to the vacant chair; on this occasion he published "An Essay on the Antiquity of Hindoo Medicine," a work of much research, and one alike valuable to the medical man, the antiquary, and the philologist. In 1845 he brought out a "Manual of Materia Medica," as a text-book for the students attending his lectures at the College, which rapidly passed through several editions both at home and abroad.

A special department of correspondence, relating to vegetable productions, was founded at the East India House in 1838, and confided to Dr. Royle, and to the duties of this office his best energies were devoted for nearly twenty years; it was an occupation peculiarly congenial to his taste and to the bent of his mind, as he was, to some extent, able practically to carry out those views, which in the earlier part of his life he had gradually deduced from scientific observations and theoretical considerations. In connexion with this office he published three valuable technical works on India; namely, in 1840, "An Essay on the Productive Resources of India;" in 1851, "An Essay on the Cultivation of Cotton;" and in 1855, "An Essay on the Cordage-Plants and Vegetable Fibres of India;" besides numerous smaller works and pamphlets on similar subjects.

Dr. Royle was a warm and active supporter of industrial exhibitions. Fully alive to their vast value and importance in a commercial point of view, he was mainly instrumental in obtaining from India those extensive and valuable illustrations of Eastern arts and produce, which excited so much interest at the great International Exhibitions of 1851 and 1855; and at the time of his death he had just completed the formation and arrangement of a technical museum at the East India House, designed especially to exhibit the arts and productions of India, and to illustrate its boundless capabilities and resources.

Though chiefly known as a botanist, Dr. Royle was well-skilled in other branches of natnral history, and was an active member of most of the societies devoted to them; at different periods he filled the duties of Secretary to the Geological and Horticultural Societies, and repeatedly served on the Councils of the Royal and Linnean Societies. As a botanist, his careful and laborious habits, and accuracy of observation, give authority to his writings and weight to his opinions;—as a technical writer his works possess a peculiar value, from the

circumstance that he combined at the same time, high scientific attainments, accurate geographical and commercial information, sound practical knowledge of the arts and manufactures, and an intimate acquaintance with the habits, customs, and prejudices of the people of India, and a full appreciation of the capabilities of the country.

Richard Horsman Solly, born in 1778, was educated and took his degree at Magdalen College, Oxford; he entered for the Bar, but being heir to an ample fortune, he relinquished the legal profession as soon as he had completed the preliminary course of study; treating it rather as an amusement than as an occupation, and abandoning it as soon as he had been called to the Bar. At an early age he became attached to scientific pursuits, joined most of the scientific societies of London, and took an active interest in their management and welfare. He was one of the original promoters of the Royal Institution in 1800, and during a period of more than forty years was a constant attendant at its meetings and an active member of its committees. He also took part in the formation of the Geological and Horticultural Societies, serving repeatedly in the Council of the latter as well as of the Linnean Society. He was a warm supporter of the Society for the Encouragement of Arts, and for a period of many years was unwearied in attending its meetings, and ever liberal towards its objects; a large number of the mechanical engravings which embellished the Transactions of that Society, were furnished at his expense.

Mr. H. Solly was elected a Fellow of the Royal Society in 1807, and without pretending to high scientific standing in any one department of knowledge, was possessed of a very considerable amount of general information in most branches of natural science, more especially vegetable physiology and systematic botany; and though seldom occupying himself with original observations or investigations, he did good service in the cause of science, being always ready to aid young inquirers with advice, encouragement, and pecuniary assistance.

In connexion with the Society of Arts, he devoted much careful consideration towards improving the mechanical and chemical processes of the engraver, more especially with reference to the printing of bank-notes and other similar documents of such elaborate designs and perfect execution as should render forgery impossible. In the

same society, too, he contributed mainly towards the improvement of the microscope, directing his attention from year to year to the various schemes brought forward for perfecting that instrument, and seeking by the offer of special prizes to call forth its improvement in those points in which superior excellence was most to be desired.

Thomas Tooke, Esq. was the eldest son of the Rev. William Tooke, F.R.S., author of various literary works, and chaplain of the English Factory at St. Petersburg. Thomas Tooke was born in that city on the 29th of February, 1774; and after undergoing a general education, entered early in life into active mercantile pursuits as partner in one of the largest houses engaged in the Russian trade. During this period, Mr. Tooke laid the foundation of that accurate and surprising knowledge of detail in connection with commerce and political economy which enabled him to raise, upon so wide and solid a basis, the economical doctrines and discoveries inseparably associated with his name.

These assumed a palpable form in a work which he published in 1823, entitled "Thoughts and Details on High and Low Prices," the prelude to his great work "The History of Prices," the first two volumes of which appeared in 1838, the third and fourth in 1840 and 1847, and the two closing volumes, in which he was assisted by Mr. Newmarch, in 1857.

This remarkable work, evincing a rare combination of practical wisdom, sound judgment, and great knowledge of commercial statistics, caused the author to be regarded as a most distinguished writer on the science of political economy. The Royal Society testified their sense of his merits by electing him a Fellow on the publication of his first work, and the French Academy more recently elected him a Corresponding Member.

Nor were Mr. Tooke's labours confined to authorship. He was an active participator in the inquiries and legislation connected with the social reforms of the last five-and-twenty years. He took a leading part in the Factory Worker's Commission, and was the Chairman of the Commission for investigating the difficult subject of the employment of children. For a long period he presided over the Royal Exchange Assurance Corporation and the St. Katharine's Dock Company; and was one of the founders of the Statistical

Society, in which he took great interest. Mr. Tooke was also one of the early promoters of the London and Birmingham Railway; and the celebrated Merchants' Petition in favour of Free Trade emanated from him. At that period (1821), he projected and founded the Political Economy Club, which still exists.

Thus, the active sphere occupied by Mr. Tooke was of scarcely less importance than his pursuits as a philosopher, and his name will be always associated with the great social improvements of this century, as for nearly fifty years he applied his high mind and great acquirements to purposes of practical legislation, which have been conducive of much public good. Mr. Tooke died at his residence, 31 Spring Gardens, on the 26th of February, 1858, in his 84th year; and it was only within the last few months prior to his decease, that he manifested very sensibly the decay of powers generally incident to his extreme age.

Benjamin Travers, Esq.—This eminent surgeon was born in London, on the 3rd of April, 1783, so that he was ten years old when John Hunter died. In his sixteenth year he betook himself to the great Hospital School of Anatomy and Surgery in the Borough, then superintended by the elder Cline and Astley Cooper.

In the year 1800 he was apprenticed to Sir Astley at Guy's Hospital. This connection at once secured for him all the advantages and best privileges of a student's life. His career was early marked by great ardour and diligence, and a sojourn at Edinburgh during the Session 1806–7 (after passing his examination at the London College), was always spoken of by him, as a period of unbounded delight, and keen interest, in all that pertained to the prosecution of his medical studies. Here he formed many lasting friendships; and the names of Thomson, Barclay, and Gordon, amongst others, might be mentioned as being descriptive of the eminent men whose society he more especially sought and appreciated at this important epoch of his educational career.

On his return to London he quickly gathered about him a large class of pupils to attend the anatomical demonstrations which he delivered at Guy's Hospital, and in this practice he persevered for several successive seasons.

Mr. Travers obtained no professional employment until the year

1809, when he was appointed to the lucrative post of Surgeon to the East India Company's Volunteer Corps. This piece of good fortune at once determined the aspect of his future life. He was a man of great natural endowment, to which was added the charm of manners, to say nothing of an exquisite tact and great tenderness in dealing with the misfortunes of others. It is well known that for these reasons Mr. Travers was always held in the highest esteem, both by the profession and the public.

In 1810, on the death of Mr. Saunders, Mr. Travers joined Dr. Farre at the Eye Infirmary in Charterhouse Square. This appointment speedily brought with it a great accession of private business. To this connection the profession is indebted for one of Mr. Travers's earliest and most popular works, "The Synopsis of Diseases of the Eye." This book speedily ran through three editions, besides being republished in America, and translated into Italian by Dr. Apolloni, a physician of Pisa. It possesses the great merit of being founded on original observation, and was long held in much regard as a Text-Book, though since superseded by larger and more ambitious publications.

No Hospital Surgeon ever attained a wider or more justly-deserved reputation for a profound knowledge of eye-disease than the subject of this notice. His papers on Cataract in the Medico-Chirugical Transactions, and the treatise above mentioned, were received at the time as masterpieces of accurate symptomatology; they abound in new facts, and form an elegant and comprehensive digest of all that was then known touching various important points of practice.

At the time of Mr. Travers's appointment to the office of Surgeon to St. Thomas's Hospital in 1815, the post was one of extreme independence, and the field of observation has at all times been very extensive.

So long as his health lasted, Mr. Travers availed himself of this splendid opportunity to its fullest extent, and we soon find him associated in the surgical course with his great colleague and former master Sir Astley Cooper, then in the zenith of his fame as an operator and a lecturer on Surgery. Mr. Travers, unfortunately, soon found that his bodily vigour was not such as would enable him to maintain his post as a lecturer; he was one night carried from the Theatre in a fainting fit, from which for some time it remained doubtful whether he would

ever rally. He thus felt himself most reluctantly compelled to forego one of the great and early objects of his ambition, but in compensation for this it may be remarked, that had he retained his post as Sir Astley's colleague in the Surgical Chair, he probably never would have penned the "Treatise on Constitutional Irritation," a production, which has long since secured for its author a European reputation.

Early in this century, Dr. Jones had explained the operation of a single ligature upon the coats of an artery, and dispelled the obscurity which had gathered round this question. There were, however, some material points of doubt and discussion still remaining to be dealt with. These the subject of our memoir finally and completely elucidated by experiment, so that the causes of secondary hemorrhage are now well ascertained, and are far more effectually guarded against than was the case before the appearance of Mr. Travers's papers in the fourth and ninth volumes of the "Medico-Chirurgical Transactions." It was after his appointment to St. Thomas's that he perfected this inquiry, by proving, on the person of a patient under his own care, that a ligature may be withdrawn fifty hours after its application, without risk, and successfully, so far as concerns the obliteration of the trunk of the brachial artery. A similar result was obtained after tying the carotid in the horse and ass, although the ligature was removed so early as twelve or even nine hours after its original application. On one occasion Mr. T. removed a ligature from the femoral artery of a man twenty-seven hours after tying that vessel for a popliteal aneurism, but here pulsation returned and the experiment failed. This suggestion, or rather the discovery of these effects of the temporary use of ligatures, was entirely original, as well as the announcement of another new fact, to which we shall now make some allusion.

In 1811 Mr. T. communicated to the Royal Society an account of some experiments which exhibit the means adopted by nature for the cure of wounded intestines. This paper was accepted for publication, but it was withdrawn to form the groundwork of a larger treatise, published in 1812, which was most favourably received by the profession. It is there proved that if the intestine of a dog be strangulated by a single ligature, the ulcerative inflammation provides for the escape of the thread into the cavity of the bowel; nature at the same time restoring the wall of the gut by a deposit of lymph, which undergoes a rapid organization. Of the great work on Constitutional

Irritation, we have little more to say in the brief space assigned to us, than that it is truly "the work of a master." It consists of two parts: the first contains an account of the direct effects of local irritation upon the great centres of life; the heart, the brain, and nervous system. The second is a more elaborate production: it embraces a review of all those obscure relations between parts and centres respectively, which the author terms "reflected," wherein the latter are not abruptly roused to a direct response and sympathy with the local excitement, but where the action passes on, *via* the system, to some other tissue or organ of the body, or is remitted back from the centres directly to the offending part, as shown by the specific form or type of the local changes. This is the most profound portion of the whole work; the author was always diffident of the success of this second part of the inquiry; he felt he should not be understood, and yet, to use his own majestic phrase, "he sought to rise to the dignity of a discourse upon the philosophy of Surgery."

Mr. Travers contributed largely to the best periodical literature of his time. These productions are for the most part to be found in the earlier volumes of the "Medico-Chirurgical Transactions," nor must we omit to mention that his first paper narrates the success of an operation performed for the cure of a remarkable aneurismal tumor. On this occasion Mr. T. tried the common carotid artery. The woman perfectly recovered. At that time this operation had only once before been performed successfully by Sir Astley Cooper. The two papers on Malignant Disease, and a small theoretical discourse on Syphilis, must not pass without praiseworthy mention, to say nothing of his last work on Inflammation, a crowning effort, worthy of this great disciple of Hunter.

Mr. Travers in early life was a very good operator. He was still young when he first held the Surgeoncy to the Eye-Infirmary. He used to say that a man who can extract the cataract with tolerable success can do anything with the knife. Add to this, that from a very early period he was in the daily habit of cutting down upon arteries, and performing other hazardous experiments upon living animals, which must also have contributed to give him readiness and dexterity as an operating Surgeon.

He lived to achieve all the honours of the race set before him. He was twice President of the College of Surgeons, and had long

passed the Chair at the Medical and Chirurgical and the Hunterian Societies, in addition to the reward finally bestowed by Her Majesty, of creating him one of her Sergeant-Surgeons a few months before his death. He died somewhat suddenly, after having suffered from prolonged illness, at his residence, No 54, Green Street, Grosvenor-square, on the 6th of March, 1858.

In manner and personal appearance he was eminently refined and gentlemanlike. Of such men we may say, with the younger Pliny, "Accepisse te beneficium credes, quum propius inspexeris hominem, omnibus honoribus, omnibus titulis parem."

Henry Warburton, Esq. was the son of a London merchant, and was himself for a time engaged in business. He had, however, passed through a distinguished career at Cambridge, and being possessed of sufficient fortune, he exchanged the pursuit of commerce for science, literature, and politics. He entered Parliament in 1826, and finally retired from it in 1847. The course he usually took as a member of the legislature is well known, and among various measures of public utility in which he had a principal share, we need here only specify, as connected with science, his Chairmanship of the Committee on Medical Education, and his authorship of the Anatomy Act. He was one of the founders of the London University, now the University College, and for many years was a Member of its Council. He was also an original Member of the Senate of the University of London, and continued in that body till the time of his death. He was elected a Fellow of the Royal Society in 1809. He was also an active member of the Geological Society, and in 1843 and 1844, filled the office of President. He died at his house in Cadogan-place, on the 16th of September, 1858, at the age of seventy-three.

Mr. Warburton was always in the habit of keeping up the mathematical knowledge which he had acquired at Cambridge. His mathematical library was extensive; and the retirement in which he lived, even while he was a member of Parliament, gave him time to use it. It was not until the termination of his public life that he thought of printing any speculation of his own: and it is a remarkable instance of the manner in which even men inured to publicity feel diffidence in entering on a new career, that the veteran politician, accustomed to face overpowering majorities of the House of Commons,

with unpalatable propositions, committed his thoughts to a friend to be digested and presented to the Cambridge Philosophical Society, from nothing but timidity at the idea of appearing in person. But by the time the paper was drawn up and publicly read, the real author took heart of grace, and drew up his own thoughts with additions. This paper "On the Partition of Numbers, and on Permutations and Combinations," was printed by the Cambridge Society in 1847. Another, "On Self-Repeating Series," was published in 1854. Both papers show a great command over the German factorial notation, and add several curious theorems to their subjects.

JOHANNES MÜLLER was born in the city of Coblentz, on the 14th of July, 1801. His father, Matthias Müller, was a shoemaker, in a small way of business, but, notwithstanding his narrow means, determined not to deny his son the advantages of a good education. Accordingly, after such tuition as was suited to his earlier years, the boy, in 1810, entered the secondary school or gymnasium of his native town, where for eight years he was instructed in classics, mathematics, and other branches of liberal learning. His rather monotonous life at this institution, which is said to have been carried on in an old-fashioned scholastic way, was relieved, and his mind expanded, by independent reading, especially of Goethe, and by frequent rambles in the country, in which he gratified his love of external nature, and collected plants and animals, for the study of which he showed an early predilection.

At the recommendation of the Director of Schools of the province, Johannes Schultze, who had doubtless noted the intellectual promise of the youth, Matthias Müller had destined his son for a learned profession; and although he did not live to see the fulfilment of his intentions, they were dutifully carried out by his widow. Accordingly in 1819 young Müller was sent to the University of Bonn, having in the mean time, after leaving the gymnasium, gone through a year's military service, as was the custom with those of his age and condition.

Before entering on his university course, the young man had an important question to settle. Born of Roman Catholic parents and nurtured in the same faith, he had when yet a child manifested a desire to be brought up for the priesthood, and this inclination had been fondly cherished by his pious and affectionate mother. The

time had now come for choosing his path; and we are told that it was after three days of anxious communing with himself that he gave up thoughts of the Church, and decided for Medicine.

He remained three years at Bonn, and took his degree of Doctor in December, 1822; having presented an inaugural dissertation on the laws of animal locomotion*, a subject on which he had already published some observations in 'Oken's Isis.' His career at the university was characterized by intense application to study, but with the constant exercise of independent thought, and by a keen relish for original investigation. Prompted by this, though but in the first year of his studies, he engaged in a series of experiments and observations on the respiration of the fœtus, a subject which had been proposed for a prize question by the university; and his essay†, distinguished alike by learned research and by original and varied experiment, was declared the successful one. Müller's scientific tendencies at this period may be also inferred from the fact that he acted as secretary of a Natural History Society established among the students at Bonn, by Nees von Esenbeck.

But while thus intent on the proper work of a student, Müller was not indifferent to the general yearning after constitutional freedom, which, after expulsion of the French, pervaded the liberal mind of Germany; and we are told that he heartily joined the Burschenschaft, and even took part as a leader in that rather enthusiastic association, in which, notwithstanding the ban of the Carlsbad decrees, the academic youth still cherished their hopes of German unity, and laid plans for social improvement.

After taking his degree, Müller went to Berlin to pass his examinations for licence to practise (Staatsprüfungen), and continued for a year and a half to prosecute his philosophical and medical studies in that university. He had not gone through his career at Bonn without contracting some leaven of the "Naturphilosophie" with which the leading German schools of biology were then fermenting. Of this however he was radically cleared at Berlin, through the influence of Rudolphi, of whom he became a favourite pupil. Rudolphi was an enemy to subjective speculation in biological science; he looked on the so-called philosophy as mistaken and futile in its application to the phenomena of the animal economy, and based his physiology

* Diss. Inaug. de Phoronomia Animalium. Bonnæ, 1822.

† De Respiratione Fœtus. Commentatio physiologica. Lipsiæ, 1823.

chiefly, and perhaps rather exclusively, on the study of the animal structure. Of the encouragement and aid received from that excellent man, Müller afterwards spoke in the most grateful terms, and he declares that it was through the influence and example of Rudolphi that his scientific pursuits were afterwards turned so much to comparative anatomy.

Müller returned to Bonn in 1824, and in October of the same year began his career as an academical lecturer in that university. In 1826 he was made Professor Extraordinary. In the meantime, however, the duties he imposed on himself as a teacher had been unusually onerous, and to these was added unremitting employment in original investigation, with all its concomitant labour and thought. Such overstrained exertion brought on a state of bodily exhaustion and mental depression, which in 1827 obliged him wholly to lay aside work for a season, and to seek for health and recreation in a journey up the Rhine, and through the south of Germany, in which he was accompanied by his newly-married wife. Returning with recruited health, and resuming his duties in Bonn, he was in 1830 promoted to the grade of Professor in Ordinary; and in the spring of 1833 he was called to occupy the chair of Anatomy and Physiology in Berlin, which had become vacant by the death of his friend and preceptor, Rudolphi.

Of the works published by Müller during his stay at Bonn, the first in point of time was one "On the Comparative Physiology of Vision," which appeared in 1826 *. This was immediately followed by a smaller essay "On the Phantasmal Phenomena of Vision †," a class of phenomena which had greatly interested and attracted Müller when a boy, and in the contemplation of which, as he himself informs us, he used to give free play to his fancy. The appearances, thus become early familiar to him, he subjected in maturer years to philosophical scrutiny, and the work in which they are described and discussed forms properly the continuation of the larger treatise on vision which preceded it. Of this treatise, the leading characteristics are, according to the opinion of one well qualified to judge ‡, the masterly application of anatomy, physiological experiment, phy-

* "Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Thiere," &c. Leipzig. 1826.

† "Ueber die phantastischen Gesichtserscheinungen," &c. Coblenz. 1826.

‡ Professor Theod. L. W. Bischoff, of Münich, in his "Festrede über Johannes Müller." München. 1858.

sics, psychology and other branches of knowledge to the elucidation of the physiology of vision, and the thorough, searching, and many-sided way in which the whole matter is handled.

In an essay "On the Development of the Reproductive Organs," which appeared a few years later*, Müller traced the steps of that process in the embryo of man and animals, detected the minute primordial filament (now known by his name) which gives rise to the oviduct or Fallopian tube, and applied with much success the knowledge thus acquired to the elucidation of certain perplexing malformations which sometimes occur. Pursuing his researches into the intimate structure and development of organs, he was able about the same time to produce his treatise on the secreting glands†. In this well-known work the intimate structure of the organs in question is investigated in the varied conditions which it presents, from the lowest animals to man, and from the embryonic to the perfect state; and one great result of this labour was to establish, on a wider and more satisfactory basis, the true doctrine of the relation of the blood-vessels and gland-ducts, as first correctly conceived by Malpighi. It was also shown that the same kind of secretion might be yielded by glands formed, as far as discoverable, on an entirely different type of construction. It was at this time, also, that Müller, almost simultaneously with Panizza of Pavia, made the discovery of the lymphatic hearts in reptiles; a discovery, which especially deserves notice on the present occasion, inasmuch as it was communicated to this Society and published in the "Philosophical Transactions‡."

When he settled in Berlin, Müller's first care, next to his professorial duties, was the continuation and completion of his "Handbook of Physiology," commenced before he left Bonn. Appearing in successive parts, the book was at length finished in 1840.

To this important work, so well and favourably known to English readers through the admirable translation of Dr. Baly, it is unnecessary here to make long reference. With defects of construction which detract from its usefulness as a systematic guide to the student of physiology,—although, as a general treatise, unequal in scope to

* Bildungsgeschichte der Genitalien. Dusseldorf. 1830.

† De glandularum secernentium structura penitiori earumque prima formatione, &c. Lipsiæ. 1830.

‡ Read Feb. 14, 1833. Phil. Trans. 1833, p. 89.

the "Elementa" of Haller, and making no pretence to emulate the prodigious learning and elaborate finish of that stupendous work, which occupied its author for the greater part of a long life,—Müller's "Handbook" was accepted, we may almost say, with universal accord, as the most valuable general work on physiology which had appeared in the long interval since Haller's time. And, indeed, the two great physiological writers have much in common. In both, we perceive the same earnest purpose of placing the doctrines of physiology on a basis of fact, the same constant endeavour to extend and consolidate this foundation, or test its validity, by materials and methods placed at their command by their accomplishment in the cognate and collateral sciences. Anatomy, human and comparative, experiments on animals, chemistry, and physical science, in its various departments, are all brought to bear in the investigation of physiological truth.

Müller's work is, moreover, enriched throughout with the fruits of the author's own observation and experimental inquiry, which are sometimes, it is true, given with a detail better suited for a separate memoir than for a chapter in a handbook, but which signally enhance its value as an original source of information. Almost every part of the book affords evidence of this, but it is enough to refer specially to the examination of the blood, the disquisitions on the nervous system, and the valuable experimental investigations on the voice and hearing. Here, as in his other writings, it is characteristic of Müller that he takes nothing on trust; every statement, whether of matter of fact or doctrine, is thoroughly sifted. Difficulties, however perplexing, are never evaded or slurred over; defects, however they may deface the picture to be presented, are never disguised. Every question is resolutely attacked; the result, whether success or failure, is honestly told; and there is no yielding to the temptation, so powerful with writers of systems, of rounding off a rugged subject with smooth plausibilities.

While carrying on his experimental inquiries in physiology, Müller did not neglect the study of pathological anatomy, and he was one of the first to apply the microscope to the study of morbid growths*;

* Ueber den feinen Bau und die Formen der Krankhaften Geschwülste, Berlin, 1838.

still his chief pursuit to the end of his life was comparative anatomy, with occasional excursions into the neighbouring fields of zoology and palæontology. Fishes and marine invertebrata were his favourite subjects. The chief fruits of his inquiries were—his memoirs on the myxinoid fishes; his systematic description (in association with Henle) of the Plagiostomata; the reintroduction into zoology of the placentiferous shark of Aristotle; his essay on the Ganoids and on the natural arrangement of fishes; his papers on Rhizopoda; and his remarkable succession of memoirs on the embryology and structure of the Echinoderms. It will not have been forgotten by the Fellows of this Society, that for the last-mentioned discoveries in particular, in addition to his previous labours in physiology and comparative anatomy, Professor Müller received the Copley Medal in 1854.

Of the memoirs on the myxinoid fishes, we may observe, in the language of the President's address on the occasion mentioned, that their title conveys but a faint idea of their scope and importance; for while the anatomy of a particular family of fishes may be said to form the text, there is an ample commentary, rich in new and original matter, in which the structure is compared in other tribes, and the facts sagaciously applied to the elucidation of great questions in animal morphology. Referring to the researches on the Echinodermata, the President thus continued:—"Professor Müller early applied himself to the study of the structure and economy of the Echinoderms. After describing, in a special memoir, the anatomy of the Pentacrinus, so interesting as a living representative of the extinct Crinoidea, and publishing, in conjunction with M. Troschel, a systematic arrangement and description of the Asterida, he was at length happily led to investigate the embryo life of this remarkable class of animals. The field of inquiry on which he entered had scarcely been trenched upon before, and he has since made it almost wholly his own by persevering researches carried on at the proper seasons for the last nine years, on the shores of the North Sea, the Mediterranean, and the Adriatic. In this way he has investigated the larval conditions of four out of the five orders of true Echinoderms, and has successfully sought out and determined the common plan followed in their development, amidst remarkable and unlooked-for deviations in the larval organization and habits of genera even of the same order; and his inquiries respecting these animals have

made us acquainted with larval forms, with relations between the larva and future being; and with modes of existence; such as nature has not yet been found to present in any other part of the animal kingdom. Finally, with the light thus derived from the study of their development, Professor Müller has subjected the organization of the entire class of Echinoderms, both recent and fossil, to a thorough revision, and has added much that is new, as well as cleared up much that was obscure, in regard to their economy, structure, and homologies. It is to these researches, which occupy seven memoirs in the 'Transactions of the Royal Academy of Sciences of Berlin,' that more special reference is made in the award of the Medal. Besides their other claims to distinction, they may be justly regarded as revealing a new order of facts in the history of animal development."

Soon after his settlement in Berlin, Müller established the "Archiv für Anatomie und Physiologie," which goes by his name; and he continued the publication till the time of his death. Besides containing numerous original contributions of his own, and the valuable Annual Reports on the progress of these sciences, drawn up by himself or his able assistants, this Journal, following after "Meckel's Archives," has formed the principal medium of publicity for the labours of the leading physiologists of Germany during the period of its existence; and the establishment and continued superintendence of it by Müller, in the midst of other laborious employments, must be ever regarded as an important service rendered to science.

What remains of Müller's personal history may be soon told. Only two events, so far as we know, after this time broke in upon the even tenour of his life. In 1848 he was Rector of the University, in a time of civil commotion, when political agitation distracted the academic body, and both students and professors left the lecture room to equip themselves as soldiers. Müller, who in youth had been an ardent "Bursch," was now a sober conservative, and in his mind the aspect of affairs threatened disaster to the State and the University. His situation was one of difficulty and not without peril; he strove manfully to maintain authority, though with little success; but even those who took a different view of passing events paid a willing tribute to his honesty of purpose and the personal courage he displayed in most trying circumstances. The other remarkable occurrence in Müller's latter years was the following.

He was accustomed to spend his autumn vacation somewhere on the sea-coast, for the sake of studying marine animals. While he was returning from one of these visits which he had made in 1855 to the coast of Norway with two of his pupils, the steamer, between Bergen and Christiansand, in which he was travelling, was run into by another, and speedily sank. Nearly fifty people lost their lives, and among them one of Müller's companions, a young man of great promise. In a letter to a friend in England, in which Müller gives an account of this deplorable calamity, he says that on finding himself in the water, he at first kept himself up by swimming; but that having his clothes on, he soon became exhausted, and would have inevitably perished, had he not caught hold of a ship's ladder that was floating by. He held on for a long time, and had given up all hope of succour, when he was picked up by a boat from the other vessel. His remaining companion, Dr. Schneider, saved himself in a similar way. This event had a deep effect upon him, and, although he still resorted to the sea-side, he dreaded afterwards to trust himself on ship-board.

Still working hard as before, but with altered spirits, he in the spring of 1858 began to fail in health; he complained of headaches and passed sleepless nights, owing, doubtless, to a recurrence of cerebral disease from which he had twice before suffered in the course of his life. Experiencing no amendment, and at length feeling that his end was approaching, he settled his affairs both public and private; called his son by telegraph from Bonn on the 27th of April, and fixed on the morrow for a medical consultation on his case; but the next morning found him a corpse.

A man of Müller's eminence had of course been enrolled a member of the chief learned bodies of Europe and America. He was elected a Foreign Member of the Royal Society in 1840.

Müller was rather grave and reserved in manner; he was upright in all his dealings, ever ready to perceive merit in others, candid and just in acknowledging the scientific labours of his predecessors and contemporaries. The tidings of the unlooked-for extinction of his laborious and valuable life caused profound sorrow in every part of the world where science is cultivated.

December 9, 1858.

Sir BENJAMIN COLLINS BRODIE, Bart., President, in the Chair.

The President announced that, under the provisions of the Charter, he had appointed the following Members of the Council to be Vice-Presidents:—

The Lord Wrottesley.
Major-General Sabine.
Thomas Bell, Esq.
John Peter Gassiot, Esq.
Sir Roderick Murchison.
The Rev. Dr. Whewell.

The President, on taking the Chair, addressed the Meeting as follows:—

Gentlemen,

Although I have already had the opportunity of offering to you my thanks for the great honour which you have conferred on me in placing me in this Chair, it is but fit that I should repeat them now, when we are assembled in a more formal manner, and when probably some Fellows are present who were not present at the Anniversary dinner. It is impossible that I should be otherwise than highly gratified by such an expression of the good opinion of a Society, which may justly be regarded as including a larger proportion of individuals distinguished for their knowledge and intelligence than any other in this country. At the same time I must own that my feelings on the occasion are somewhat modified when I see around me so many of our Fellows who have devoted their lives to scientific pursuits, and who in their respective departments have contributed so much more than I have done to the advancement of scientific knowledge. It is now long since the requirements of an arduous profession, and the public not less than the private duties belonging to it, compelled me to direct my attention to other objects, and in a great degree to relinquish those researches, to which during many

previous years I had been able to devote a large portion of my time, and which were to me the chief objects of interest during the early period of my life. Still, although I have ceased, except to a limited extent, to be a labourer in that field of science in which I laboured formerly, I have never failed to sympathize with those who in this respect were more happily situated, and to regard with satisfaction, or I ought rather to say with admiration, the grand results at which they have arrived in extending the boundaries of human knowledge.

If it were possible for any one of that small but illustrious band of philosophers,—who just two centuries ago were associated in Gresham College for the purpose of mutually communicating and receiving knowledge, and who there laid the foundation of the Society which is now assembled—to revisit the scene of his former labours, we may well conceive the delight which it would afford him to learn that the success of that noble enterprise had been so much greater than his most sanguine aspirations could have led him to anticipate. Not only would he find an ample development of sciences which were then in the embryo state of their existence, but he would find other sciences, not inferior to these in interest and importance, added to the list. He would find that, instead of a limited number of individuals who were then occupied with scientific inquiries, whose labours were held in little estimation by the general public, and even held to be objects of ridicule by the presumptuous and ignorant, there is now a large number devoted to the same pursuits, and successfully applying to them the highest powers of the human intellect. He would perceive that, instead of being confined as it were to a corner, the love of knowledge is gradually becoming extended throughout the length and breadth of the land; and that, of those whose position does not afford them the opportunity of penetrating to the inmost recesses of the temple of science, there are many who, having advanced as far as the vestibule, are enabled even there to obtain their reward, in the improvement of their own minds, and in being rendered more useful members of the community.

Now, to say that all that has been accomplished as to the cultivation of science in this country during the last two centuries is to be attributed to the Royal Society, would be an absurdity. As, in now far-distant times, the course of events led the ancient nations, first of Greece and afterwards of Rome, to the cultivation of literature, of

moral philosophy, of geometry, and of the fine arts; so in these latter times, the course of events, taking another direction, has led the nations of Europe to the investigation of the physical sciences. The Royal Society has been one of the results of this movement; but being once established it became itself a cause, and has been a most powerful and efficient instrument for the carrying on, and giving a right direction to, the movement in which it had itself originated. It has been the means of bringing those who have the same objects in view into communication with each other; and we all know how the interchange of knowledge and opinions, and the spirit of emulation, tend, at the same time that they increase the energy and activity of the imagination, to correct and mature the judgment. Nor should we overlook the fact, that the institution of the Royal Society has always afforded an honourable distinction for those whose labours have contributed to build up the fabric of human knowledge,—a distinction which has this peculiarity, that it can never be obtained through favour or interest, while the selection of candidates for the Fellowship is as carefully and impartially conducted as is the case at present.

Among the portraits which we see around us is one of the Sovereign who granted us the charter by which we are incorporated, and who conferred the title of Royal Society on us. Whatever defects posterity may have discovered in the character of King Charles the Second, we are bound to express our obligations to him, not only for the charter which we hold, but for the real interest which he seems to have taken in our Society when it was yet in its infancy, and for the attention which he paid to it during the early period of his reign, at a time when the patronage of the Crown was of so much the greater importance, as there were but few among the public who sympathized with the new association in its pursuits, or were capable of estimating the objects for which it was established. Nor did His Majesty merely grant us a charter, but it was one especially suited to the genius and character of the English people. When nearly forty years afterwards the Académie des Sciences was founded by King Louis the Fourteenth, it was placed wholly under the dominion of the Crown. The number of its members was limited: those belonging to one of its sections received pensions from the State; and when a vacancy occurred in any of the sections, it was necessary that the election of the new member should be confirmed by the Crown. Now we must not

find fault with the constitution of a Society which has earned for itself so lofty a reputation; including in the list of its members the names of the most profound philosophers, and the greatest geniuses of the age, and of whose works all who are engaged in the pursuit of knowledge are justly proud; but we cannot doubt that with us such a constitution, so different from that of every other corporation in this country, would have been very much less successful than that which we actually possess. The charter of the Royal Society leaves the management of its affairs entirely in the hands of the Fellows, without the interference of any higher power. No one, in virtue of his belonging to it, receives any pension or derives any other advantage from the Government, and our funds are supplied altogether by ourselves. The sum of £1000, for some time past, has been annually voted by Parliament for the promotion of science. The Royal Society have undertaken the task of suggesting to the Treasury the manner in which this may be most usefully and economically distributed, the duty of accomplishing this object being devolved on a committee specially appointed for that purpose. But from this Parliamentary grant the Royal Society derives no special advantage, it being applied indifferently, for the purpose of supplying apparatus or other means of carrying on scientific inquiries, whether these inquiries belong to their own Fellows or to other persons. Being thus independent of the powers by which the State is governed, and having no other object than that of observing the physical phenomena of the universe, and tracing the laws by which they are regulated, the Royal Society has always pursued its course free from political excitement, and beyond the influence of anything in the shape of party politics. The effect of this has been not to sever the connexion which ought to exist between an institution of Royal foundation and the State, but to cause that connexion to manifest itself only by mutual exchange of good offices. The Royal Society has been always ready to lend its assistance to the Government whenever they required it, either in the way of giving their opinion on scientific questions, or in that of carrying out any public work; and I may add, that thus they have been enabled, not in a few, but in numerous instances, to render good service to the community; while, on the other hand, they are indebted to the Government, first, for the apartments in Somerset House, formerly allotted to them by King George

the Third, and now for the more ample accommodation granted to them by Her present Majesty.

When the Royal Society was first established, there was no other Society devoting itself to the pursuit of any branch of knowledge; and hence it was that many communications were made on subjects not strictly belonging to those sciences, to which it was intended that their attention should be more especially directed. If we refer to their earlier publications, we find in one of them a scheme for a universal alphabet; in another, a dissertation on the Chinese language. Father Gaubil, a missionary belonging to the order of Jesuits, sends them a map of Pekin, with an exact account of the imperial palace. An English merchant gives a history of his journey to Aleppo and Tadmor; others describe the discovery of tessellated pavements and other Roman antiquities. In short, there is scarcely any one department of knowledge, whether it be philology, history, antiquities, medicine, geography, political economy, and even metaphysics, which is not to a greater or less extent represented in the Philosophical Transactions. But all this time knowledge of all kinds was rapidly increasing, *vires acquirens eundo*. The time arrived when a division of labour was required, and the Royal Society discovered the necessity of confining themselves to their more legitimate pursuits. In the year 1717 the institution of the Society of Antiquaries attracted one large class of communications from them. After an interval of seventy years, the Linnean Society was founded for the cultivation of natural history; and I need not enumerate the various other societies which have been since called into existence, and which are now pursuing their course, not as rivals of the Royal Society, but as cooperators with it in the great work of exploring the phenomena of the universe. Whatever may have been the apprehensions which some may have entertained formerly, the event has proved that these new institutions have in no degree interfered with the reputation and usefulness of that from which they derived their origin. Indeed, without such fellow-labourers as these it is difficult to understand how, in the present state of knowledge, the Royal Society could have met the expectations of the scientific portion of the community. There would have been no means of recording a vast number of valuable details, from which important conclusions may be drawn in after-times. At the same time, we need

only refer to the volumes of the Philosophical Transactions, published since the beginning of this century, to be satisfied that the disposition to communicate the higher class of investigations to the Royal Society is not less than formerly. It is, indeed, the interest of every one who is ambitious that his name as a discoverer should be transmitted to posterity, that his works should have a place in the Philosophical Transactions, where, as has been observed by a writer in the Edinburgh Review, "He has the benefit of the great name acquired by that distinguished body, by the labours of Newton and Halley and Cavendish, and by two centuries of constant services performed to the commonwealth of letters*."

With the exception of the achievements of those small communities of ancient Greece, to whose works we still refer as affording the highest standard of excellence in literature and the fine arts, and from whom has been transmitted to us that marvellous science of geometry which enabled Newton to unravel the system of the universe,—with this exception, there is nothing in the history of what belongs to the advancement of knowledge so remarkable as the progress which the European nations have made in the cultivation of the physical sciences during the last two hundred years. It is not only those who are engaged, as you are, in researches of this kind, that must contemplate with satisfaction the results of this movement. The moral philosopher, recognizing in the desire of knowledge one of the noblest of our aspirations, will regard the extension of that desire, and the more general diffusion of knowledge, as an important means of elevating our species in the scale of intellectual beings. The unprejudiced theologian will allow that there is no better foundation for the religious sentiment than the study of natural phenomena, opening as it does to our view everywhere examples of design, and of the adaptation of means to ends, combined with mighty power and benevolent intention. The philosophical statesman, who, contemplating the progress of society, endeavours to explain the changes which it has undergone, and thence to anticipate the future, cannot fail to perceive that the cultivation of the physical sciences has been in these later times one of the most important instruments of civilization; while the mere utilitarian, however little he may be capable of estimating knowledge for its own sake, must admit that it has contri-

* Edinburgh Review, 1811.

buted more than anything else to the comforts and conveniences, not of one order only, but of every order of the community, from those who dwell in palaces to the tenants of cottages and garrets. I need not occupy your time by adducing particular instances of the benefits to which I allude; and indeed they are so obvious, that I should not have thought it worth while to allude to them at all, if it were not that they show how complete a refutation the lapse of time has afforded of the views of those short-sighted cynics, among whom I am sorry to include even so distinguished a person as the author of Gulliver's 'Travels,' who formerly opposed or ridiculed the Royal Society, as if it were engaged in trifling pursuits of no advantage to mankind.

Science has already arrived at great results, far beyond what could have been reasonably anticipated. But the inquisitive mind, looking into the future, will find reason to believe that if we could lift up the veil by which it is concealed, we should find that there are other and still greater results reserved for those who will come after us, and which even some of those who are now among us may live long enough to witness. Astronomy has been said to be the most perfect of all the sciences; and some time since it might have been supposed that, as regards it, we had not the power of carrying our researches much further. But the observations of Lord Rosse, penetrating by means of his improved telescope into the more remote regions of space, have enabled him to determine the nature of the so-called nebulous matter, and to enter on a new order of inquiries respecting the construction of the universe. The needles which now vibrate in the magnetic observatories which have been established in different regions of the earth, under the direction of the Treasurer of our Society, have already not only made us acquainted with many important facts connected with terrestrial magnetism, but have disclosed to us some remarkable relations between the earth and the sun, of which we had no conception previously. But these magnetic observations are still in progress, and if not prematurely brought to a conclusion, it would be unreasonable to doubt that they must lead to still more important discoveries as to the operation of a force, which, probably like that of gravity, pervades the universe, and places even the most distant parts of it in connexion with each other. A Fellow of our Society—devoting to it his time, his fortune, and his intellectual powers—is in a fair way to attain the great object

of his life, in the construction of an arithmetical engine of far superior capabilities to any previously invented by himself or others; such as may not only be a lasting monument of profound mathematical knowledge, of inventive genius, and of that perseverance amid difficulties which is one of the highest attributes of genius; but may be of great use to mankind hereafter, by solving with unerring certainty problems which practically are in any other way beyond the reach of the human intellect.

It is merely because it happened that they first occurred to me, that I have adduced these instances of investigations which are now going on, but of which the principal results remain to be worked out hereafter. I need not tell you that analogous instances might be furnished from almost every department of knowledge. If we add to these the number of investigations of a more limited kind, each complete within itself, which every year produces, also bearing in mind how great an influence the cultivation of the physical sciences during the last century has exercised on our social system, and how much it has contributed to give to modern civilization its peculiar character, we may well ask, what may not be the effect of a continuance of the same spirit of inquiry during the century which is to come? In considering such a question, it must be remembered that, whenever any addition has been made to the general stock of knowledge, the practical application of it to the ordinary purposes of life, for the most part, is made not immediately, nor until a long time afterwards. In the year 1739, the Rev. Dr. Clayton, at that time Dean of Kildare, communicated to the Royal Society his experiments on the distillation of coal, and his discovery of what he called "the spirit of coal." This spirit he confined in bladders, and occasionally diverted his friends by puncturing one of them near a candle, thus exhibiting a bright flame which issued from the puncture, until the whole of the spirit in the bladder was exhausted. Now the application of this discovery seems, as we see it now, to have been sufficiently obvious, yet nearly sixty years elapsed before Mr. Murdoch was led to avail himself of it for the purpose of lighting a factory in Manchester. In the year 1800, Volta, following up the researches of Galvani, discovered the effect which the multiplication of metallic plates has of increasing the electric force. About five years afterwards Davy began that grand series of experiments, in which he succeeded in

decomposing the alkalies and earths by means of an apparatus similar to that invented by Volta, but of larger extent and greater power. But very many years yet elapsed, and many improvements and modifications of the battery had been effected, before the same method was made use of for the purpose of electro-plating. Early in the present century, Davy published an account of the effects produced on the nervous system by the respiration of the nitrous oxide. It was afterwards ascertained by physiologists that the respiration of the vapour of ether operates in a similar manner, symptoms, like those of intoxication, being followed by a temporary loss of sensibility. But it was still many years afterwards that it first occurred to a dentist in America that the respiration of ether might be employed for the purpose of producing insensibility to pain during surgical operations. The time may often be long deferred, but our experience warrants the assertion, that there are very few of the discoveries which have been made in the physical sciences which have not, sooner or later, directly or indirectly, had the effect of promoting the well-being, the convenience, and comforts of mankind. As it has been hitherto, so we may expect it to be hereafter. In the meanwhile, the Royal Society, gathering to itself those who are most eminent as cultivators of any branch of natural philosophy, has no small share of responsibility, and has important duties to perform. It may encourage the deserving; it may lend a helping hand to those who want it; it may, as it always has done, render assistance to the Government where such assistance is required. Nor am I arrogating too much for the Royal Society when I say that it has still another function, which it even now exercises, not less substantially and really because the Fellows of the Society are themselves unconscious of it. Of the value of knowledge I apprehend that few at the present day will venture to express a doubt. But in all ages much of that which has been given to the world as knowledge has been no knowledge at all; and from this evil even the present age, in spite of the efforts made for the improvement of education, is not exempt. An institution such as ours is in this respect a great safeguard to the public. Here individuals engaged in pursuits which require accurate observation and cautious induction, are brought more or less into communication with each other. Mistakes as to matters of fact, and too hasty conclusions, are alike corrected. Though not

in any regular and formal manner, whatever is put forth under the pretence of it being knowledge, is submitted to a competent tribunal, whose decisions silently and imperceptibly pervade general society, and go far towards exposing the shams and impostures of the day.

But I feel that I am occupying too large a portion of the time which belongs to this evening's meeting, and that I owe you my apologies for doing so. Allow me, however, to make one more observation, which will, I feel sure, have the cordial assent of every one who hears me; namely, that it is desirable that the Royal Society should persevere in the independent course which it has hitherto pursued, relying on its own character and on the exertions of its Fellows, seeking no adventitious aid, and satisfied with the conviction that no one can labour in the acquirement of knowledge without, sooner or later, rendering service to mankind.

On the motion of Dr. Charles Holland, the thanks of the Society were voted to the President for his Address, and he was requested to allow the same to be printed in the 'Proceedings.'

The following communications were read:—

I. "Researches into the Nature of the Involuntary Muscular Tissue of the Urinary Bladder." By GEORGE VINER ELLIS, Esq., Professor of Anatomy in University College, London. Communicated by Dr. SHARPEY, Sec. R.S. Received November 6, 1858.

(Abstract.)

In the present communication the author endeavours to show, that the involuntary muscular tissue of the bladder and the voluntary muscle in other parts of the human body have a like composition, and that Prof. Kölliker's view, that involuntary or smooth muscle is made up of fusiform cells, is incorrect. On the contrary, the muscular substance of the bladder is composed of lengthened fibres with fixed and tendinous terminal attachments. The fasciculi of muscular fibres in the bladder are interwoven into a network, and are marked at varying intervals by tendinous intersections, like those of the Rectus abdominis on a small scale.

The author terms what are usually called the 'nuclei' of the muscular tissue—'corpuscles,' and distinguishes two varieties of them,

the oval and the fusiform. The latter are the more numerous, and are the rod-like nuclei of Kölliker. Two or even three of these may be observed in the length of a single fibre. If a single muscular fibre of the bladder is isolated, it will be found to terminate as in voluntary muscle; connective tissue investing not only the fibre, but each of the separate portions into which it ultimately divides.

The author considers that the 'sarcous elements' of voluntary muscle are represented by the lines of dots visible in the muscular fibres of the bladder.

II. "On the Ova and Pseudova of Insects." By JOHN LUBBOCK, Esq., F.R.S., F.L.S., F.G.S. Received Nov. 10, 1858.

(Abstract.)

In the 'Philosophical Transactions' for 1857, I endeavoured to show that the agamic eggs of *Daphnia* are formed upon the same type, and consist of the same parts, as any other egg. My object in undertaking the investigation of which the present paper is the result, was to determine whether the same held good of the agamic eggs or pseudova of *Coccus, Cynips,* and other insects. This inquiry was more interesting, because Prof. Huxley had found several differences between the ovarian products of the oviparous and viviparous Aphides; and because, according to Prof. Leydig, the development of the pseudova in *Coccus* was extremely peculiar.

My examination of *Coccus* was concluded, and the results committed to paper, in the early part of June last, but I then found that so little was known, especially in this country, about the development of insect-eggs generally, that I withheld my notes from publication, in order to add to them some account of the process of true egg-formation in the Insecta, which would enable me to point out more satisfactorily the differences between, or the identity of, these two processes.

In all female insects there are two ovaries, each consisting of at least two egg-tubes opening into a common chamber, the uterus. The egg originates and attains to nearly its full size in the egg-tube, and it is therefore with this portion of the generative organs that we are now mainly concerned.

The egg-tubes differ very much in number and length. In all the larger orders, except perhaps the Lepidoptera and Heteroptera, some species have very few, while others possess a great many. Thus in Coleoptera, *Lytta vesicatoria* has a great many, *Lixus* has only two; in Orthoptera, *Acheta domestica* has a great many, while in a small *Locusta* I only found six; in Neuroptera, *Libellula* has a great many, *Psocus* only five; of the Diptera, the majority have many, *Melophagus* only two; in Homoptera, *Coccus* has a great many, while *Aphis padi* has only three; in Hymenoptera, *Apis mellifica* has about 170, and *Chelonus* has only two; and even in so small a group as the Dermaptera, *Labidura gigantea* has, according to Léon Dufour, only five, while *Forficula auricularia* has a great number.

The number of egg-germs in each egg-tube differs also very much. The Lepidoptera, in which the number of egg-tubes is very small, have a great number of egg-germs in each, while the Homoptera, in which the egg-tubes are so numerous, have very few egg-germs in each tube. On the other hand, in Heteroptera, the number varies very little; while in Coleoptera, Hymenoptera, and Diptera, it differs greatly, though not so much as that of the egg-tubes. The number of egg-germs, is, however, by no means so easy to determine as that of the egg-tubes. It is probable that in each species the number is definite, except perhaps where it is very numerous, as, for instance, in certain Lepidoptera which have more than a hundred. In most egg-tubes, however, the egg-germs become so "small by degrees," that it is almost impossible to say exactly how many there are.

Each egg-tube consists generally, if not always, of two membranes. The outer or muscular one is very evident in Hymenoptera, Geodephaga, Diptera, and indeed in most insects, but in some cases I could not distinguish it. The inner membrane is delicate and structureless. On its inner side lies a layer of epithelial cells, which in most parts form a continuous layer; but in those insects which have a group of vitelligenous cells between each of the egg-germs, they are at these parts more sparingly distributed.

These epithelial cells probably take an active part in the secretion of the yolk in all insects, and are the principal, if not the only organs, which form the yolk in Orthoptera, *Pulex*, and the Libellulina.

Between each of the egg-germs in Lepidoptera, Diptera, Hymenoptera, Geodephaga, Hydradephaga, and Neuroptera (except the

Libellulina), is situated a group of large cells. These were first noticed by Heroldt, who described them as rings; Stein, however, is no doubt correct in asserting that they are the secretors of the yolk, and I therefore agree with Prof. Huxley in calling them vitelligenous cells. Hermann Meyer, who was followed in this respect by Dr. Allen Thomson, considered them as aborted eggs, an opinion, however, which is quite untenable. A cursory examination of an egg-tube of any Lepidopterous or Hymenopterous insect, will show that although the vitelligenous cells increase individually in size, as does the yolk mass, yet that the latter constantly grows at the expense of the former*, which become gradually fewer in number, and finally disappear altogether.

Stein has observed, that in *Acilius sulcatus*, in which the yolk is brightly coloured, the vitelligenous cells are of the same hue. Prof. Huxley has observed in Aphis, and I have found in certain Hemiptera, as in *Nepa* for instance, a canal leading down from the terminal chamber into the egg-tube, and which can be for no other purpose than to convey the yolk matter to the growing eggs.

Finally, if, as Stein also remarks, we press the vitelligenous cells or nuclei out of one of the egg-chambers, we shall generally find some of them in which the cell-wall is almost entirely absorbed, so that on the application of slight pressure, the contents spread in all directions.

In its earliest stage, however, the egg-cell cannot be distinguished from the vitelligenous cells, and at the upper part of the egg-tube of any Heteropterous or Dipterous insect, will be found cells, which are neither vitelligenous cells nor egg-cells, but which are apparently capable, under certain circumstances, of becoming either the one or the other.

Dr. Carpenter has suggested to me that these vitelligenous cells are perhaps analogous to the "yolk-segments" of *Purpura*, and this idea throws, I think, some light on the very remarkable phenomena presented by that genus.

The separation of the egg-germs from the vitelligenous organs, is a condition found in many worms, in some crustacea, as for instance in *Cyclops*, and probably in the Cirrhipedes.

Our knowledge of the modes of egg-formation in the Insecta, is

* See Lacordaire, Introd. à l'Entomologie, v. ii. p. 386.

perhaps hardly sufficient to enable us to generalize upon it as yet with much confidence. As far, however, as we at present are aware, alternate groups of large vitelligenous cells are found in all Lepidoptera, Diptera, Hymenoptera, Neuroptera (except Libellulina), Geodephaga, and Hydradephaga. The large vitelligenous cells are contained in a terminal chamber in other Coleoptera, Homoptera, and Heteroptera; whilst apparently they are absent in Orthoptera, Libellulina, and *Pulex*.

This curious subdivision of the Insecta is not exactly that which would be given by any other characters. It is, however, remarkable, that the mode of formation of the thorax would divide the Insecta into two groups very nearly equivalent to those just mentioned, except as far as regards the Geodephaga and Hydradephaga.

In fact, the Coleoptera, Orthoptera, Dermaptera, and Hemiptera have a very large prothorax, while this segment is small in the Lepidoptera, Diptera, Hymenoptera, and most Neuroptera. In the Libellulina, however, it is distinct from the rest of the thorax and considerably developed.

In the Orthoptera, Libellulina, and the genus *Pulex*, we find the simplest type of egg-formation which occurs among the Insecta, the large vitelligenous cells being entirely absent, and their functions probably monopolized, instead of being only shared, by the layer of epithelial cells.

The macula germinativa, which is in fact the nucleus of the germinal vesicle, has in the Orthoptera, as usual, the form of a small round vesicle.

In *Œstrus* the germinal vesicle contains several small vesicles, one of which grows much larger than the remainder, and becomes the macula germinativa.

In *Pulex* the germinal vesicle is dark, and the macula germinativa, which is very distinct in the young egg-germs, soon disappears.

In the Coleoptera (except the Geodephaga and Hydradephaga), the Homoptera, and the Hemiptera, each egg-tube ends in a large terminal chamber, full of round cells, each of which can apparently become either an egg-cell or a vitelligenous cell.

In *Nepa* and some other forms, I found, as Prof. Huxley first discovered in *Aphis*, a duct or passage leading down from the terminal chamber to the egg-germs. In one specimen there were four

distinct ducts, so that probably each egg-germ has a separate yolk duct.

In *Nepa* there is a large lateral projection at the anterior end of each egg, and it is always on the same side as the germinal vesicle; but this latter varies from side to side without any apparent regularity.

In the common earwig, the egg-tubes are short and numerous; each consists of a large, lower chamber, which is more or less pear-shaped, and two or three other chambers, but slightly separated from one another, bent down on the lower chamber, and so short and small as to resemble very closely the stalk to the pear.

Each egg-germ in this insect consists of two parts; an egg-cell containing the germinal vesicle and the yolk, and a vitelligenous cell. The vitelligenous cell has no distinct nucleus, and in the small stalk-like part of the egg-tube is double the size of the egg-cell, which at this period contains the germinal vesicle, but not as yet any yolk matter. In this part of the egg-tube it sometimes appeared as if there were two vitelligenous cells to one egg-cell. In the large lower egg-germ this is never the case. In this part of the egg-tube the vitelligenous cell is still larger than the egg-cell, which however grows larger, both absolutely and relatively, until it almost fills the egg-chamber.

The yolk mass in the lower egg-germ consists of dark granules and oil globules surrounding the germinal vesicle, which generally contains two or three minute cell-like vesicles. The contents of the vitelligenous cell are light brown, granular, and in part arranged in somewhat cylindrical masses, which lie generally rather transversely to the egg-tube, and do not appear to have any firm boundaries. At least, I was never able to isolate them, except after applying reagents, as, for instance, acetic acid, and then only sometimes, and with difficulty. They then appeared to be somewhat elliptic in shape.

From M. Léon Dufour's description, the ovary in *Labidura gigantea* is entirely unlike that of *Forficula*.

The egg-formation in *Forficula* is not the least remarkable peculiarity of this extraordinary genus, and does not at all resemble that of either the Coleoptera or the Orthoptera.

The Neuroptera (except the Libellulina) offer the next step to-

wards the type which prevails in the Lepidoptera, Hymenoptera, and Diptera.

In these four orders, and in the Geodephaga and Hydradephaga, each egg-chamber contains, at its upper end, a group of large vitelligenous cells, which are generally few in number in the Neuroptera, rather more numerous in the Diptera, and still more so in the Geodephaga, Hydradephaga, and Hymenoptera.

In all these six groups, except the Diptera, each egg-chamber is divided into two parts by a transverse constriction, which separates the vitelligenous cells from the germinal vesicle and the yolk surrounding it. At first, the lower division of the egg-chamber is quite small, but it grows gradually larger at the expense of the upper part.

In the Diptera the egg-chamber is not divided into two smaller chambers, but has a rounded or oval form, and contains a number of vitelligenous cells, the lowest of which becomes darker from the formation of granular yolk-matter, and thus forms the egg-cell. Its nucleus becomes the germinal vesicle. The wall of the egg-cell gradually disappears, and the yolk-mass being enlarged by the yolk secreted by the vitelligenous cells, continues to increase until it occupies the whole egg-chamber.

Between each egg-germ, in the Diptera, the egg-tube becomes extremely narrow, which is not so much the case in other Insecta. It is remarkable, that *Pulex,* which is in many respects nearly allied to the Diptera, should differ from them so greatly in the mode of egg-formation.

The germinal vesicle in the Carabidæ generally contains, besides the macula germinativa, several small vesicles, which indeed are sometimes very numerous. In *Carabus violaceus,* the macula itself appeared to consist of many small oval masses, and in one specimen the macula appeared to have broken up, and the constituent bodies were floating about loose in the germinal vesicle. From these observations, and from what has been mentioned as occurring in *Pulex,* I am disposed to think that the first embryonic cells, at least in Insecta, appear in this manner.

According to M. Léon Dufour, *Chelonus oculator,* one of the small Ichneumonidæ, possesses no ovary nor eggs, but merely four long tubes leading into as many matrices, containing a great number of

living embryos, or perhaps nymphs. I examined several specimens, and found the female generative organs quite in accordance with his descriptions and figure. They are, however, undoubtedly true ovaries, though the thickness of the outer membrane gives them a deceptive appearance. If the egg-tubes are torn asunder, egg-germs will be found in them, as in the corresponding organs of any other Hymenopterous insect, and presenting as usual a transverse constriction with the vitelligenous cells in the upper division.

In the four matrices the eggs are cylindrical and somewhat curved, and under the action of water, one end of each swells up considerably. In this they present an approach to that form which attains its greatest development in *Cynips*.

The ovaries of *Coccus hesperidum* have been rightly described by Prof. Leydig, as consisting of a long tube on each side opening by a very short oviduct into the egg-canal. The whole surface of the tube is covered with egg-germs in all stages of development. The collateral glands are very small in *C. hesperidum*, but are well developed in *C. persicæ*.

In its earliest stage the egg-follicle is a simple projection of the ovarian wall, which becomes gradually pear-shaped, and may then be seen to consist of a structureless outer membrane, a layer of epithelial cells, and three vitelligenous cells, with very delicate walls.

The walls of these cells soon disappear, and even the nuclei are often scarcely distinguishable, but acetic acid will generally make them more visible. Leuckart has given a correct account of these bodies, and indeed of the whole process; but Leydig apparently mistook them for germinal vesicles.

The epithelial cells line the membrane constituting the egg-follicle. As usual, they are columnar in the lower chamber and flattened and scattered in the upper. They contain a circular nucleus. The action of water causes them, and indeed the whole upper chamber, to swell considerably. The columnar epithelial cells of the lower chamber contain generally small greenish globules, apparently identical with the small oil globules which form so large a part of the yolk, which is, I therefore suppose, in part secreted by them.

The germinal vesicle makes its appearance after the vitelligenous cells, and generally after the egg-follicle has lost its original pyriform-

shape. It is about ·0008 in diameter. The macula germinativa is single, and somewhat granular in appearance.

About the same time as the germinal vesicle, the oil globules make their appearance, and soon become the most conspicuous part of the egg. They are at first very small, but in a mature egg the larger ones are as much as ·0016 in diameter. The oil globules may often be seen with their sides much flattened by mutual pressure, and must therefore possess a somewhat compact pellicle.

Very soon after the appearance of the first oil globules, the egg-follicle loses its pear-shaped form, the basal part swells, and is separated from the apical part containing the nuclei of the vitelligenous cells, by a constriction. It now perfectly resembles the egg-chamber of any ordinary insect, in consisting of an upper chamber containing the vitelligenous cells, and a lower chamber devoted to the germinal vesicle and the yolk.

According to M. Leydig, the constriction gradually disappears, and the egg finally occupies both chambers; but this is incorrect, and M. Leuckart is right in asserting that the vitelligenous cells disappear, and the upper chamber becomes atrophied, so that the mature egg lies in the lower chamber only. This process is exactly that which the analogy of other insects would lead us to expect.

The general cavity of the body of the female *Coccus* always contains an immense number of oval green cells, apparently of a parasitic nature. They are $\frac{1}{8000}$ in length, and vary in length, but on an average about $\frac{3}{8000}$. *Coccus persicæ* contains a number of similar bodies, which however are cylindrical. Almost always immediately after the disappearance of the vitelligenous cells, two or three masses of these cells may be found at the lower part of the upper chamber, and soon after in the egg itself. It is difficult to understand why these cells should appear at so definite a period in the history of the egg-formation. Prof. Huxley has pointed out to me that Dr. V. Wittich has already described a Conferva found in hen's eggs. It does not, however, seem clear that these eggs would have arrived at maturity, and I believe that the parasites of *Coccus* are the first which have ever been known to exist in eggs without impeding their development.

The mature egg contains numerous vitelline sphærules which are from $\frac{2}{8000}$ to $\frac{7}{8000}$ in diameter, and offer every appearance of true cells, but that they contain no nucleus.

The mature egg is a light green, ovate mass about $\frac{25}{2000}$ in length and $\frac{28}{2000}$ in breadth, and possesses apparently only one envelope. It contains a well-formed embryo before it leaves the ovary, and is hatched, I believe, only a few hours after being laid. According to M. Leydig, the first trace of the embryo arises at the free or cephalic end, but my observations have led me to the opposite conclusion.

C. persicæ differs from *C. hesperidum* in being decidedly oviparous; that is to say, the eggs, when deposited, do not contain an embryo, and remain under the protection of the mother some time before being hatched. This difference probably makes tehm require a stronger egg-shell, and accordingly we find the collateral glands more developed. In most respects, however, the egg-development is very similar in these two species, but the egg-follicles are smaller and neater in *C. persicæ* than in the former species, and the vitelligenous cells are five or seven in number instead of three, spherical and very distinct.

In *Cynips lignicola*, the ovary consists of a number of egg-tubes which fall into a common oviduct, and each of which contains thirteen eggs.

This species has in the last few years become very common in the South-west of England, but as yet only females have been discovered.

It is fair to assume, therefore, that the eggs are agamic, or adopting Prof. Huxley's name, pseudova. Nevertheless, there is absolutely nothing, so far as our knowledge at present extends, to distinguish the egg-formation from that which occurs in any other Hymenopterous insect.

The mature egg of *Cynips* is indeed of a very remarkable shape, as it consists of a long tube with a small swelling at one end, and a larger one at the other, in which the yolk is situated.

The larger end occupies the usual place of the egg, but as the tube elongates, the smaller end pushes its way up the egg-tube, which elongates considerably; and, finally, all the large ends are at the lower end, and the small ones at the upper end, of the egg-tube, which gives the ovary a curious appearance. Even after the egg is fully formed, a slight pressure will bring the germinal vesicle into view.

Many of the Lepidoptera have presented us with cases of Parthenogenesis, and in these instances there is no reason to suppose that the formation of the eggs differs from the usual type. The same

holds good in *Solenobia lichenella,* in which agamic eggs are the rule, instead of the exception.

In the Hive-bee also, the early development of the ova and of the pseudova must apparently be identical, since it would appear that in an impregnated female, the ovarian product has already left the ovary before it is decided whether it is to become an ovum or a pseudovum, and whether it is to give birth to a male or to a female.

It is, therefore, I think, proved that we must not look in the ovarian egg for differences necessarily depending on sexual influence, but that we shall find them, if anywhere, in the subsequent stages of egg-development.

Prof. Huxley and Leuckart have recently shown, that whereas the vitelligenous cells are well developed in the oviparous *Aphides,* they are much less apparent in the agamic or viviparous forms; so much so indeed as to make these naturalists doubt whether they take any part in the secretion of the yolk. While waiting for the publication of Prof. Huxley's observations, I have paid but little attention to the *Aphides*; but it struck me as a curious coincidence, that, in *Coccus* also, while the vitelligenous cells are very distinct in the oviparous *C. persicæ,* they are much less apparent in the almost viviparous *C. hesperidum.* It would of course be highly unphilosophical to draw any conclusion from four instances, but it will be curious if the same connection between oviparity, and the presence of well-developed vitelligenous cells on the one hand, and viviparity, with less developed vitelligenous cells on the other, is found to prevail in other species.

It has been generally stated that all species of *Aphides* are, in spring and summer, self-fertile and viviparous, and become in autumn oviparous, while the eggs require impregnation. I cannot, however, help thinking it probable, that in cold and in mountainous regions, where at any period of the year frosts may occur, we shall find species which are always oviparous (as indeed is said to be the case with *Aphis abietis*); while in tropical regions, where frosts are unknown and leaves are less often deciduous, other species may occur which are naturally viviparous all through the year, and whether or not they have undergone impregnation.

December 16, 1858.

SIR BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with notice given at the last Meeting, the Right Rev. the Lord Bishop of Ripon was proposed for election and immediate ballot.

The ballot having been taken, his Lordship was declared duly elected.

The following communications were read:—

I. Extract of a Letter from Professor LAMONT to Major-General SABINE, Treas. and V.P.R.S., dated Munich, Dec. 19, 1858. Communicated by Professor W. H. MILLER, For. Sec. R.S.

"My magnetic observations in France, Spain, and Portugal are now published, and a copy for you is on the way. The observations of last summer are under the press. They comprehend about thirty stations in the North of Germany, Belgium, Holland, and Denmark. Next year I am going to Italy, and in 1860 I intend to revisit Spain, in order to observe the total eclipse of the sun, and also to make magnetic observations.

"I have found that on the Continent the lines of horizontal intensity move from south-west to north-east, making an angle of about 20° with the meridian, that is, in a direction coinciding with the lines of declination. The lines of inclination seem to move in the same direction, and the motion of the lines of declination will probably coincide with the lines of horizontal intensity. Calling $+\Delta H$ and $-\Delta i$ the annual changes of horizontal intensity and inclination of a central station (suppose London), the annual changes for a place situated x degrees in latitude to the north, and y degrees in longitude to the west, will be—

$\Delta H - 0{\cdot}00018x + 0{\cdot}00008y$ (absolute measure, French units),

$-\Delta i \quad + 0'{\cdot}21x \quad - 0'{\cdot}09y.$

"The new survey of the British Islands will offer an opportunity of testing the correctness of these formulæ."

II. Extract of a Letter from Professor KREIL of Vienna, to Major-General SABINE, Treas. and V.P.R.S., dated Nov. 26, 1858. Communicated by Professor W. H. MILLER, For. Sec. R.S.

"I am returned after an absence of nearly six months, during which I have been travelling in the Danubian Principalities, in Turkey in Europe, and along the south-west and north coasts of the Black Sea, in order to make magnetic observations, and to determine more accurately the geographical position, as well as the magnetic declination, of many points of the coast."

III. "Fossil Mammals of Australia (Part I.). Description of a mutilated skull of a large Marsupial Carnivore (*Thylacoleo Carnifex*, Ow.), from a conglomerate stratum, eighty miles S.W. of Melbourne, Australia." By Professor R. OWEN, F.R.S., &c. Received September 18, 1858.

In this paper the author gives a description of a fossil skull and certain of the teeth of a quadruped of the size of a lion, in which he points out the characters indicative of its carnivorous habits and of its affinities to the marsupial order.

The large size of the temporal fossæ, meeting to form a low crest on the parietal bone, and bounded behind by a strong occipital crest; together with large carnassial teeth in both upper and lower jaws, evince the carnivorous habits of the extinct species. Its marsupial nature is, in the author's opinion, demonstrated by the following cranial structures:—A large vacuity in the bony palate; a proportionally large lacrymal bone extending upon the face and perforated by the lacrymal canal, anterior and external to the orbit; three external precondyloid foramina; the perforation of the basisphenoid by the entocarotid canal; the great interval between the foramen ovale and foramen rotundum; the separation of the tympanic from the petrous bone; and the development of the 'bulla auditoria' in the alisphenoid; the position of the outlet for a vein from the lateral sinus behind and above the root of the zygoma; finally, the low and broad occiput, and the very small relative capacity of the brain-case.

In the marsupial order, the present large extinct Carnivore, for which the author proposes the name of '*Thylacoleo** *carnifex*,' is most nearly allied to the *Dasyurus* (*Sarcophilus*) *ursinus*; but is very different in its dentition from that and all existing Carnivora.

The fossils described were discovered by William Adeney, Esq., in a calcareous conglomerate stratum in a bank of a lake situated 80 miles south-west of Melbourne, Australia.

IV. "On the Nature of the Action of Fired Gunpowder." By LYNALL THOMAS, Esq. Communicated by Dr. GRAY. Received November 17, 1858.

(Abstract.)

Since the year 1797, when Count Rumford made his experiments for ascertaining the initial force of fired gunpowder, an account of which appears in the Philosophical Transactions of that year, very little light has been thrown on the subject. Count Rumford's experiments, valuable in many respects, afforded indeed nothing conclusive respecting it.

The object of the present paper is to show the unsatisfactory nature of the present theory of the action of gunpowder, and to point out some of the principal errors upon which this theory is based. For this purpose, the results of various experiments made by the author, and which were repeated in the presence of a Select Committee at Woolwich, are described and explained.

These experiments are held by the author not only to afford complete evidence of the unsoundness of the present theory, but as sufficiently conclusive to serve as a basis for the formation of a new set of formulæ, both correct and simple, in place of those at present in use.

The initial action of the fired charge of powder upon the shot,—the first movement of the shot itself in the gun,—and the force exerted upon the gun by different charges of powder,—and, therefore, the actual strength of metal required for the gun,—are circumstances, which, as the author believes, have not only been misunderstood, but for which laws have been assigned directly opposed to the truth.

* From θύλακος, *a pouch*; λέων, *a lion*.

As an instance of this, the hitherto received theory supposes that when a shot is fired from a gun, it acquires its velocity gradually, from the pressure of the elastic fluid generated by the fired powder acting upon it through a certain space. It is also supposed that the initial pressure of this elastic fluid is the *same* in all cases (the quantities of powder being proportional), whether the gun from which the shot is fired be large or small; so that the larger the calibre of the gun, the slower the first movement of the shot is supposed to be. The result of the following experiment is given to prove that the first of these propositions is incorrect. The author placed a cast-iron shot 3 inches in diameter and 3 lbs. 14 ozs. in weight upon a chamber half an inch in diameter and half an inch deep. This chamber was formed in a block of gun-metal, and contained, when filled, one dram of powder. Upon lighting the powder, the ball was driven to a height of 5 feet 6 inches; when the ball was placed at $\frac{1}{8}$ of an inch over the chamber, the charge failed to move it.

From this it is inferred that the first force of the powder is an *impulsive* force, that is to say, it imparts to the shot at once a finite velocity. In order to place the matter beyond a doubt, and to ascertain the relative force of different quantities of powder, the author caused a chamber to be made similar in form to, but of twice the linear dimensions of, the former; he then placed a cast-iron ball of 6 inches in diameter upon the orifice of this chamber, which was filled with powder; upon firing the latter, the ball was driven up to a height of 11 feet, that is to say, to double the height of the smaller; the state of the metal in which the chamber was formed also showed the increase in the initial force of the powder: this is considered to be sufficient proof that the last two of the above-mentioned propositions are as incorrect as the first.

Assuming the initial force of the powder to be of an impulsive nature, it is not difficult to understand the increase of force shown in the last-named experiment, inasmuch as a certain time being required for the complete conversion of the powder into an elastic fluid, a quantity contained in a chamber of a similar form, but of greater linear dimensions than another, must ignite in a less comparative time, the linear dimensions increasing in the ratio of the first power, and the quantity of powder increasing in the ratio of the third power, so that the flame will traverse a larger quantity in comparative less time.

Thus it appears that the powder which inflames more rapidly has a much greater initial force, being more *concentrated* in its action; a quick burning powder therefore is better for ordnance of small length, such as mortars and iron howitzers. The different results produced by powder of different quality have, according to the author, been entirely overlooked in the hitherto received theory. This theory, which considers the *secondary* force, namely, the elasticity of the fluid only, and takes no account whatever of the enormous impulsive, or initial force, produced by the sudden conversion of the powder into an elastic fluid, is that which regulates the system upon which ordnance are at present constructed; hence the reason why large guns are so liable to burst, so much so, that it has been said that no gun larger than a 32-pounder is safe to fire. From the variety of experiments made by the author, he arrives at the conclusion, that when powder is of the same quality, and confined in chambers of similar form, but of different sizes, the initial force varies, within certain limits, in the ratio of $\frac{w^{\frac{4}{3}}}{w'}$, where w is the weight of the powder and w' of the ball.

Thus were this new theory recognized, the question of the increase of strength with increased thickness of metal, would wear an entirely new aspect. So far from the metal in large guns diminishing in strength in the proportion assumed, it will be a matter for inquiry how it resists the great strain to which it is subjected, rather than why it yields; for we find from the experiments described above, that a 68-pounder gun, which has a calibre of twice the diameter of a 9-pounder gun, must, when fired with the same proportionate charge of powder as the latter, continually be subject to as great a strain as the latter would suffer if always fired with the proof charge, which is three times the quantity of the ordinary service charge.

The Society adjourned to January 6, 1859.

January 6, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. Letter to Dr. SHARPEY, Sec. R.S., from Dr. THOMAS WILLIAMS, F.R.S., dated Swansea, Dec. 12, 1858.

In my paper entitled "Researches on the Structure and Homology of the Reproductive Organs of the Annelids," lately published in the Philosophical Transactions, the following passage occurs in a note at page 113:—"Some of these demonstrations have been recently witnessed by Mr. Busk and Dr. Carpenter."

Some time after the publication of my paper, I received a letter from Mr. Busk, observing that when taken in connexion with the context, the above allusion to his name and that of Dr. Carpenter might lead to the supposition that they acquiesced in the views which I ventured to advocate in my paper.

If permitted, I should be glad to state, that although I had shown to Mr. Busk and Dr. Carpenter certain of my dissections, and although I was favoured at the time with what I believed to be their concurrence and approbation, I am most anxious at present to explain that in referring to their names, I had no intention whatever to implicate them in the opinions which I entertained.

I remain, &c.,

THOMAS WILLIAMS, M.D., F.R.S.

II. "A Sixth Memoir on Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received November 18, 1858.

(Abstract.)

I propose in the present memoir to consider the geometrical theory: I have alluded to this part of the subject in the articles Nos. 3 and 4 of the introductory memoir. The present memoir relates to the geometry of one dimension and the geometry of two dimensions corresponding respectively to the analytical theories of

binary and ternary quantics. But the theory of binary quantics is considered for its own sake; the geometry of one dimension is so immediate an interpretation of the theory of binary quantics, that for its own sake there is no necessity to consider it at all; it is considered with a view to the geometry of two dimensions. A chief object of the present memoir is the establishment upon purely descriptive principles of the notion of distance.

III. "On the Mathematical Theory of Sound." By the Rev. S. Earnshaw. Communicated by Professor W. H. Miller, For. Sec. R.S. Received November 20, 1858.

(Abstract.)

The principal feature of this communication is the discovery of an integral of a certain class of differential equations. This class includes, as a particular case, the differential equation of motion when a disturbance is transmitted through a uniform elastic medium confined in a horizontal tube. If the equation $\frac{dy}{dt}=\mathrm{F}\left(\frac{dy}{dx}\right)$ be differentiated with regard to t, it produces the equation

$$\frac{d^2y}{dt^2}=\left\{\mathrm{F}'\left(\frac{dy}{dx}\right)\right\}^2\cdot\frac{d^2y}{dx^2};$$

which, by means of the general function F', can be made to coincide with any proposed differential equation in which the ratio between $\frac{d^2y}{dt^2}$ and $\frac{d^2y}{dx^2}$ is dependent on $\frac{dy}{dx}$ only. The integral obtained in this manner is that which arises from the elimination of (a) between the two following equations,—

$$y=ax+\mathrm{F}\,(a)\,.\,t+\phi\,(a),$$
$$0=\;x+\mathrm{F}'(a)\,.\,t+\phi'(a).$$

This integral, though not found by the direct integration of the differential equation, and though evidently not the general symbolical integral of it, is proved to be the general integral for wave-motion, from its affording the means of satisfying all the necessary equations of initial disturbance and wave-motion.

The author first discusses wave-motion when temperature is supposed to be unaffected by the passage of a wave; and then when the

change of temperature is allowed for. The most important result in the former case is the relation between pressure and velocity, which is shown to be that which is expressed by the equation

$$p=p_0\epsilon\frac{\nu}{\sqrt{\mu}};$$

from which several new results are obtained.

With respect to the velocity of sound, which has hitherto been found experimentally to exceed the velocity obtained by theory, it is shown that the value obtained by approximative methods is the *minimum limit* of sound-velocity, so that the actual velocity will always be greater; the excess depending upon the intensity and genesis of the sound. It is shown that all the parts of a wave do not travel at the same rate,—a circumstance which leads to the formation of a bore in the front of the wave. Several previously unexplained phenomena, which have been recorded by different experimentalists, such as double reports of fire-arms heard at a great distance, the outrunning of one sound by another observed by Capt. Parry, the comparative powers of different gases of transmitting sounds, and the laws of transmission of sound from one medium to another, are accounted for in this paper, and directly deduced from the integral of the equation of wave-motion.

IV. "Contributions towards the History of the Monamines." By A. W. Hofmann, LL.D., F.R.S. Received November 25, 1858.

2. *Action of Bisulphide of Carbon upon Amylamine.*

In a note on the alleged transformation of thialdine into leucine, addressed to the Royal Society about eighteen months ago*, I alluded to a crystalline substance observed by Wagner when submitting amylamine to the action of bisulphide of carbon. This substance was not analysed, but considering its mode of formation, Wagner suggested that it might possibly be thialdine.

$$\underbrace{C_{10}H_{13}N}_{\text{Amylamine.}}+C_2S_4=\underbrace{C_{12}H_{13}NS_4}_{\text{Thialdine.}}.$$

* Proceedings, vol. viii., Op. 4.

A superficial comparison of the properties of thialdine with those of the substance produced by the action of bisulphide of carbon upon amylamine, enabled me at once to recognize the difference of the two bodies; and satisfied with the result, I did not at the time examine more minutely into the nature of the latter substance.

The new interest conferred upon leucine by recent researches which characterize this substance as capronamic acid, has called my attention back to the sulphuretted derivative of amylamine.

This body may be readily procured by mixing anhydrous amylamine with a solution of dry bisulphide of carbon in anhydrous ether. The mixture becomes hot, and deposits, on cooling, white shiny scales which are scarcely soluble in ether, and may therefore be purified by washing with this liquid.

The new body is likewise insoluble in water, but readily dissolves in alcohol; when dry, it may be exposed for a time to a temperature of 100° C. without undergoing fusion; after some time, however, the substance begins to be liquefied and to undergo complete decomposition. The same change occurs, although more slowly, at the common temperature, when sulphuretted hydrogen is evolved; a mixture of free sulphur with a new crystalline substance, extremely fusible, insoluble in water, but soluble both in alcohol and ether, remaining behind.

Analysis has proved that the compound produced by the action of bisulphide of carbon upon amylamine contains

$$C_{11}\,H_{13}\,NS_2,\ \text{or rather}\ C_{22}\,H_{26}\,N_2\,S_4\,;$$

and that it is formed by the union of 2 equivalents of amylamine with bisulphide of carbon.

$$\underbrace{2C_{10}\,H_{13}\,N}_{\text{Amylamine.}}+C_2\,S_4=\underbrace{C_{22}\,H_{26}\,N_2\,S_4}_{\text{New compound.}}$$

A glance at this formula suffices to characterize this compound as amylsulphocarbamate of amylamine.

$$C_{22}\,H_{26}\,N_2\,S_4=C_{10}\,H_{13}\,N,\ C_{12}\,H_{13}\,NS_4=\left.\begin{matrix}\left[(C_2\,S_2)''\,(C_{10}\,H_{11})\,H\,N\right]\\ \left[(C_{10}\,H_{11})\,H_3\,N\right]\end{matrix}\right\}S_2.$$

This view is easily confirmed by experiment. Addition of hydrochloric acid to the crystalline compound immediately separates an oily liquid, which gradually solidifies, and the acid solution now con-

tains amylamine which may be liberated by potassa. The oily substance is obviously amylsulphocarbamic acid: it dissolves in ammonia and in potassa; mixed with amylamine, it reproduces the original crystalline compound.

Experiments with ethylamine have furnished perfectly analogous results. I have been satisfied to establish qualitatively the analogy of the reactions.

It is of some interest to compare the deportment of amylamine under the influence of bisulphide of carbon with that of phenylamine in the same conditions. If both bodies gave rise to similar changes, we should expect in the case of phenylamine the formation of phenylsulphocarbamate of phenylamine. But experiment has proved that phenylamine immediately produces diphenyl-sulphocarbamide (sulphocarbanilide), sulphuretted hydrogen being evolved—

$$2\underbrace{(C_{12}\,H_7\,N)}_{\text{Phenylamine.}}+C_2\,S_4=\underbrace{C_{26}\,H_{12}\,N_2\,S_2}_{\substack{\text{Diphenylsulpho-}\\ \text{carbamide.}}}+H_2\,S_2.$$

Nevertheless it is extremely probable that further experiments will establish a perfect analogy in the deportment of bisulphide of carbon with amylamine and phenylamine. Diphenyl-sulphocarbamide is probably the product of decomposition of a very unstable phenylsulphocarbamate of phenylamine—

$$\underbrace{C_{26}\,H_{14}\,N_2\,S_4}_{\substack{\text{Phenylsulpho-}\\ \text{carbamate of}\\ \text{phenylamine ?}}}=H_2\,S_2+\underbrace{C_{26}\,H_{12}\,N_2\,S_2}_{\substack{\text{Diphenylsulpho-}\\ \text{carbamide.}}},$$

while a more minute examination of the crystalline substance obtained by the action of heat upon amylsuphocarbamate of amylamine cannot fail to characterize it as diamylsulphocarbamide—

$$\underbrace{C_{22}\,H_{28}\,N_2\,S_4}_{\substack{\text{Amylsulphocarbi-}\\ \text{mate of amylamine.}}}=H_2\,S_2+\underbrace{C_{22}\,H_{24}\,N_2\,S_2}_{\substack{\text{Diamylsulpho-}\\ \text{carbamide.}}}\,?$$

The apparent dissimilarity of the two reactions would thus be reduced to the unequal stability of the sulphocarbamic acids of the amyl- and phenyl-series.

V. "On New Nitrogenous Derivatives of the Phenyl- and Benzoyl-Series." By P. GRIESS, Esq. Communicated by Dr. HOFMANN. Received December 9, 1858.

Piria's important discovery that the action of nitrous acid upon asparagin gives rise to the formation of malic acid, has led to a very general application of this agent in the study of nitrogenous substances. The results obtained have been almost always analogous to those produced by Piria; the reaction may be illustrated by the following examples:—

$$\underbrace{\left.\begin{matrix}(C_8H_4O_6)''\\ H_4\end{matrix}\right\}}_{\text{Asparagin.}} N+2NO_3=H_2O_2+4N+\underbrace{\left.\begin{matrix}(C_8H_4O_6)''\\ H_2\end{matrix}\right\}}_{\text{Malic acid.}} O_4.$$

$$\underbrace{\left.\begin{matrix}C_{12}H_5\\ H_2\end{matrix}\right\}}_{\text{Phenylamine.}} N+NO_3=HO+2N+\underbrace{\left.\begin{matrix}C_{12}H_5\\ H\end{matrix}\right\}}_{\text{Phenol.}} O_2.$$

The plan hitherto adopted consisted in submitting the *aqueous* solution of the nitrogenous body directly to the action of nitrous acid, or in dissolving the body in nitric acid, and passing into the solution a current of binoxide of nitrogen. By employing *alcoholic* and *ethereal* solutions, I have arrived at different results, establishing a *new mode of reaction*; of the facts which I have observed the following may be quoted as illustrations.

Action of Nitrous Acid on Picramic Acid. Diazodinitrophenol.

On passing a current of nitrous acid into an alcoholic solution of picramic acid—

$$C_{12}H_5N_3O_{10}=C_{12}\begin{pmatrix}H_3\\ (NO_4)_2\\ H_2N\end{pmatrix}O_2.$$

the red liquid assumes at once a yellow colour, and furnishes rapidly a copious deposit of yellow crystals. *No gas is evolved during the reaction.* The yellow crystals, purified by recrystallization from alcohol, are found to contain

$$C_{12}H_2N_4O_{10},$$

and are obviously formed according to the equation—

$$C_{12}H_5N_3O_{10}+NO_3=3HO+C_{12}H_2N_4O_{10}.$$

The new body, for which I propose the provisional name *diazodinitrophenol,* is soluble in alcohol and ether, and crystallizes from the former solvent in magnificent golden-yellow plates, which detonate on heating. Acids have no action upon this substance; on ebullition with water it appears to undergo decomposition; alkalies induce at once a copious evolution of gas, and give rise to the formation of dinitrophenol. This metamorphosis appears to indicate that the new body still belongs directly to the phenol-group; the constitution of diazodinitrophenol may perhaps be best understood by representing it by the formula

$$C_{12}\left(\begin{matrix} H_2 \\ (NO_4)_2 \\ N_2 \end{matrix}\right)O_2.$$

The transformation of this compound into

$$C_{12}\left(\begin{matrix} H_4 \\ (NO_4)_2 \end{matrix}\right)O_2$$

involves the decomposition of 2 equivs. of water, the oxygen of which appears to be consumed in the formation of secondary products of decomposition. No trace of oxygen, either free or combined, could be found among the gaseous products; the gas evolved consisting, according to a minute examination, of perfectly pure nitrogen.

Diazonitrochlorphenol.

Treatment in a similar manner of *amidonitrochlorphenol*

$$C_{12}H_4ClNO_6 = C_{12}\left(\begin{matrix} H_2 \\ Cl \\ NO_4 \\ H_2N \end{matrix}\right)O_2,$$

a new mixed derivative of phenol, as might have been expected, has furnished perfectly similar results. The new compound thus obtained crystallizes in beautiful brown-red needles, of physical and chemical properties similar to those of the preceding compound. It contains

$$C_{12}H_2ClN_3O_6 = C_{12}\left(\begin{matrix} H_2 \\ Cl \\ NO_4 \\ N_2 \end{matrix}\right)O_2.$$

Diazonitrophenol.

This substance is formed by submitting the ethereal solution of *diphenamic acid*

$$C_{24}\,H_{12}\,N_4O_{12}=C_{24}\left(\begin{matrix}H_8\\(NO_4)_2\\(H_2\,N)_2\end{matrix}\right)O_4,$$

discovered by Gerhardt and Laurent, to the action of nitrous acid. It is a yellow crystalline, very unstable compound, containing

$$C_{24}\,H_6\,N_6O_{12}=C_{24}\left(\begin{matrix}H_6\\(NO_4)_2\\N_4\end{matrix}\right)O_4,$$

it explodes with extreme violence at the temperature of boiling water. The alkalies decompose it instantaneously with evolution of nitrogen and formation of products which are not yet analysed.

Action of Nitrous Acid upon Benzamic Acid.

The product obtained in a similar manner from *benzamic acid* is an orange-yellow crystalline precipitate, which constitutes a dibasic acid of the formula

$$C_{24}\,H_{11}\,N_3\,O_8.$$

Its formation is illustrated by the following equation

$$\underbrace{C_{24}\,H_{14}\,N_2\,O_8}_{\substack{\text{2 equivs. of ben-}\\\text{zamic acid.}}}+NO_3=3\,HO+\underbrace{C_{24}\,H_{11}\,N_3\,O_8}_{\text{New acid.}}.$$

This acid is insoluble in water, alcohol, and ether. It is dissolved without decomposition by the alkalies in the cold, giving rise to the formation of soluble crystalline salts, which produce precipitates with nitrate of silver and acetate of lead.

All these salts are decomposed on heating, with evolution of nitrogen gas. The action of fuming nitric acid upon the dibasic derivative of benzamic acid produces a new acid, furnishing with barium a splendid yellow crystalline salt. The dibasic acid is likewise decomposed by hydrochloric acid; in combination with this acid remains a body which can be sublimed in white crystals.

An alcoholic solution of benzamic ether when treated with nitrous acid yields the ether of the acid previously described.

The action of nitrous acid on alcoholic solutions of cuminamic and

anisamic acids has likewise furnished new bodies, with the study of which I am at present engaged.

Action of Nitrous Acid on Phenylamine and Nitrophenylamine.

Phenylamine, when submitted to the modified nitrous acid-process, is transformed into a fusible body containing

$$C_{24}H_{11}N_3,$$

which is insoluble in water and easily soluble in alcohol. This compound, which possesses feebly basic characters, is formed according to the equation

$$\underbrace{C_{24}H_{14}N_2}_{\substack{\text{2 equivs. of}\\ \text{Phenylamine.}}}+NO_3=3HO+\underbrace{C_{24}H_{11}N_3}_{\substack{\text{New com-}\\ \text{pound.}}}.$$

Nitrophenylamine (the alpha-variety which is formed by the action of reducing agents upon dinitrobenzol), similarly treated, furnishes a compound crystallizing in beautifully red needles

$$C_{24}H_9N_5O_8$$

the formation of which is represented by the equation

$$\underbrace{C_{24}H_{12}N_4O_8}_{\substack{\text{2 equivs. of Nitro-}\\ \text{phenylamine.}}}+NO_3=3HO+\underbrace{C_{24}H_9N_5O_8}_{\substack{\text{New com-}\\ \text{pound.}}},$$

Treated with concentrated hydrochloric acid, the new compound reproduces nitrophenylamine. The action of chlorine and bromine upon it gives rise to the formation of new crystallized derivatives.

VI. "On the Influence of the Ocean on the Plumb-line in India." By the Rev. J. H. Pratt, Archdeacon of Calcutta. Communicated by Professor Stokes, Sec. R.S. Received December 7, 1858.

(Abstract.)

This paper is a sequel to two former communications made to the Royal Society by the author. In the first of these (communicated in 1855), the deflection of the plumb-line caused by the mountain-mass north of Hindostan is calculated; and in the second (communicated in 1858), the effect of a small excess or defect of density pre-

vailing through extensive parts of the earth's mass, is found, with a view to determine whether any compensating cause can possibly exist below to counteract the large amount of deflection caused by the superficial mass lying above the sea-level. A survey of the causes of disturbance of the plumb-line cannot be complete without taking into consideration the influence of the ocean. To approximate to this is the object of the present paper.

The author first adverts to the peculiar geographical position of Hindostan. The highest mountain-ground in the world lies to the north of it; and an unbroken expanse of ocean extends from its shores down to the neighbourhood of the South Pole. The excess of matter presented by the first causes a deflection of the plumb-line towards the north, decreasing in amount as we travel southwards. The deficiency of matter arising from the second causes a deflection of the plumb-line also towards the north, but decreasing in amount as we travel northwards. The consequence is, that while these two causes conspire to increase the deflection at the different stations, the action of the second tends to reduce in amount the errors which the mountain-attraction causes in the amplitudes.

But the attraction of the mountains northwards, and the deficiency of attraction of the ocean southwards—which last is, in fact, equivalent to a repulsive force northwards—combine to produce another effect upon the measures of the survey besides the deflection of the plumb-line. They have a sensible influence in changing the sea-level, so as to make the level at Karachi, near the mouth of the Indus—to which a great longitudinal chain of triangles is brought down from Kalianpur, in the centre of India—many feet higher than the level at Punnæ near Cape Comorin, the south extremity of the great arc. In other words, the level at Karachi is many feet higher than it would be at that place, if, while the level at Punnæ remained unchanged, the disturbing attractions were removed.

The author then proceeds with the details of the calculation, which is conducted by the method of his former papers. In our ignorance of the form of the bed of the ocean, especially in a part of the world where but few soundings have been taken, it is of course necessary to make some assumption respecting the depth of the ocean and the form of its bed. The author assumes a law as to the variation of depth, which, while it is probably a pretty fair representation of

the actual state of things on the average, permits of calculation without too much labour. The expression of this law involves three arbitrary constants, representing depths at particular places, of which the various deflections are linear functions. He next calculates numerically the coefficients of the arbitrary constants in the expressions for the various deflections, and then proceeds, guided by the probabilities of the case, to make further assumptions as to the ratios of two of these constants to the third; and lastly, as to the numerical value of the remaining constant. The general character of the assumptions is, that at a point 36° south of Cape Comorin, and in the meridian of the measured arc, the depth is assumed to be three miles, and the bottom is supposed to slope down towards this point according to a certain law.

The following are the deflections obtained at the various stations. The fifth station (called Near-Goa) is a point half-way between Punnæ and Karachi:—

At Kaliana....	deflection North	6″·18	deflection East	0″·09
„ Kalianpur..	„	9 ·00	„	0 ·48
„ Damargidæ..	„	10 ·44	„	1 ·80
„ Punnæ....	„	19 ·71	„	2 ·19
„ Near-Goa..	„	13 ·83	„	2 ·79
„ Karachi ..	„	9 ·99	„ West	1 ·26

The author then proceeds to correct the ellipticity, as deduced from the Indian arc, for the defect of ocean as well as the excess of mountain attraction, and obtains—

$$\text{Corrected ellipticity}=0{\cdot}003614=\frac{1}{276{\cdot}7},$$

which is nearer the mean ellipticity than was the value obtained by correcting for mountain-attraction alone.

He then proceeds to calculate the rise of the sea-level at Karachi above that at Cape Comorin, and obtains—

From the defect of ocean attraction......	448·25 feet.
From the excess of mountain attraction ..	66·32 „
Total..................	514·57.

January 13, 1859.

SIR BENJAMIN C. BRODIE, Bart., President, in the Chair.

I. "On the Embryogeny of *Comatula Rosacea* (Linck)." By WYVILLE THOMSON, Esq., Professor of Geology in Queen's College, Belfast. Communicated by Dr. CARPENTER. Received December 7, 1858.

(Abstract.)

The author briefly described the male and female reproductive organs of Comatula. When the ova are mature, and before impregnation, they are protruded and remain hanging from the ovarian orifice, entangled in the areolar tissue of the everted ovary. In this position impregnation appears usually to take place.

After segmentation of the yelk, a solid nucleus is formed in the centre of the mulberry yelk-mass. This nucleus becomes invested in a special membrane, and into this embryonic mass the remainder of the yelk is gradually absorbed. Ciliary motion is observed at various points on the surface of the inclosed embryo, which finally assumes its characteristic form. The young larva, on escaping from the egg, consists of a homogeneous mass of pale-yellow granular matter, with scattered nuclei, cells, and oil-globules. It is barrel-shaped, and girded at intervals with about five broad ciliated bands.

As development proceeds, one of these belts becomes depressed at a certain point; and within the loop thus formed, an inversion of the integument indicates the position of the rudimentary mouth.

A distinct œsophagus and stomach are rapidly differentiated, and a short intestine, ending in a large anal orifice, near the posterior extremity of the animal. The larva at the same time becomes lengthened and vermiform; the girding ciliated bands resolve themselves into a single transverse band, encircling the body near the anterior extremity, and a band passing below the mouth and longitudinally down either side to the tail.

Large lobulated masses of fine granular tissue occupy the cavity of the body on either side of the alimentary canal.

The echinoderm-zooid originates, apparently, beneath the integument of the larva, but perhaps in an inversion of that integument,

in the form of a rosette of cells encysted near the upper extremity of the intestine. The rosette is at first single, but shortly takes the appearance of a double ring, the rings being united by a curved tube. These rings seem to represent the rudiments of the ambulacral vascular system of the echinoderm, and the curved tube the origin of the alimentary canal. A dense coating of granular areolar tissue is formed round the young crinoid, obscuring the further development of the internal organs. The mode of its disengagement from the larva was not observed.

Free from the locomotive larva, the echinoderm in its earliest stage is a motionless, white, egg-like body, covered externally with a thick transparent layer, which is traversed vertically by scattered fusiform oil-cells.

Beneath this layer are seen rapidly-forming patches of the calcified areolar tissue so characteristic of the class. The body becomes club-shaped; the narrow end attaches itself by cement-matter to some foreign substance, and a head and stem are distinguished.

Two corresponding rows of five plates each (the *basalia*, and the first row of the *interradialia*) form a calcareous chalice round the base of the head. Rudimentary arms now first make their appearance, and the development of the attached pentacrinal form proceeds steadily.

From his observations of several broods during the spring of 1858, the author was led to believe that, under circumstances favourable to the production of the pentacrinal stage, the development of the larva may be arrested in any of its earlier stages, and before the complete differentiation of its internal organs. It is hoped that the observations of another season may solve this and other questions which still remain somewhat obscure.

II. "On the Stratifications in Electrical Discharges, as observed in Torricellian and other Vacua."—Second Communication. By J. P. Gassiot, Esq., V.P.R.S.

(Abstract.)

The author of this Paper states that he procured several vacuum-tubes from M. Geissler of Bonn, and alludes to the experiments

made in similarly constructed tubes by M. Plücker (Phil. Mag. August 1858), but finding it impracticable to ascertain with accuracy the nature of the residual gas, he reluctantly laid them aside. All the vacuum-tubes in which his experiments were made, were prepared by himself or in his presence; as each was exhausted and hermetically sealed, it was marked with a consecutive number; upwards of 100 were thus prepared; many were broken or otherwise destroyed, but the remainder he retains with the original numbers for future reference. The author uses several terms, which he explains: air, hydrogen, oxygen, or nitrogen (mercurial) denote that the vacuum-tube contains vapour of mercury *plus* the air or gas remaining in the tube with which it was filled previous to the introduction of the mercury: he applies the terms outer positive or negative, and inner positive or negative, to denote the character of the discharge from the terminals; conductive and reciprocating denote the peculiar conditions of discharges from an induction apparatus when taken in vacuum-tubes; with a conductive discharge the needle of a galvanometer placed in the circuit will be deflected, as are also the stratifications on the approach of a magnet—they having, as the author has shown in his former communication, a tendency to rotate as a whole round either pole, but in contrary directions; in a reciprocating discharge the stratifications are confused, they are divided or separated by the magnet, and the needle of a galvanometer placed in the circuit is not deflected.

The author explains the condition which the stratified discharge assumes if any air or gas remains or is subsequently introduced into a Torricellian vacuum, and describes what he denominates a white and a blue tongue discharge, which under certain conditions always appears at the negative terminal. In Torricellian vacua, if air or nitrogen is introduced, the stratifications, exclusive of their altered form, exhibit a red colour, while when hydrogen or oxygen is added, they retain the bluish-grey appearance: when the ends of the tubes were punctured by means of an electrical spark from a machine, the air or gas could be admitted so gradually as to occupy two or three hours in the experiment, and in this manner the preceding results were obtained.

In the best Torricellian vacua the author has been able to obtain, the stratifications always assumed a long cloud-like appearance; by

using ten cells, he on one occasion observed distinct sets of stratifications, one from each terminal, in opposite directions.

From a variety of experiments made in the laboratory of the Royal Institution in temperatures varying from $-102°$ to upwards of $+600°$ Fahr., he obtained the following results:—

When the flame of a spirit-lamp is applied to the discharge in a vacuum-tube, the stratifications, if they are narrow, will become clearer and divided, attaching themselves to the warmer portion of the tube; if a section of the tube is heated, the stratifications in that section will be more separated, becoming closer in the cooler portion.

If heat is applied to a tube which shows the cloud-like stratifications, they will lose their clear distinctness; the deposit from the negative wire appears to be more free, and distinct sparks or discharges are apparent, but none from the positive.

In a Torricellian vacuum from which the mercury was withdrawn, which gave clear cloud-like stratifications, no change could be observed when the temperature was lowered to $+32°$ Fahr.; at a temperature of $-102°$, all trace of the stratified discharge was destroyed, and in this state the red or heated appearance of the negative wire disappeared, the discharge filling the entire vacuum with a white luminous glow; on the temperature being raised the stratifications reappear. When the mercury in a Torricellian vacuum is boiled, indicating a heat of upwards of $+600°$, the stratifications are also destroyed; but in this case the mercury as it condenses carries the discharge, becoming a conductor.

When the mercury is frozen the stratifications disappear, and the discharge did not then illuminate the entire length of the tube; on presenting a magnet near the tube, the cloud-like stratifications immediately reappear from the positive terminal, very distinct, but not so clearly separated as when the tube is in its normal state of temperature.

The author being desirous to obtain vacua free from all trace of the vapour of mercury, endeavoured to do so by means of fusible metal, but traces of air were perceptible; he also prepared apparatus for a tin vacuum: in a vacuum obtained by means of oxygen and sodium, very good stratifications were observable. At the suggestion and with the assistance of Dr. Frankland, vacua were obtained by absorbing rarefied carbonic acid by means of caustic potassa. This process is described, and a drawing of the apparatus is given.

In carbonic acid vacua the discharge at first appears in the form of a wavy line; it is strongly affected on the approach of a magnet or by the hand, but does not generally present the stratified appearance; if this be present, it is only near the positive terminal: sometimes in the course of a few minutes, but often not until after several days, stratifications are visible, which, as the carbonic acid becomes absorbed, increase; they subsequently assume a conical form, and lastly, the clear cloud-like character of the best Torricellian vacua. Under certain conditions the stratifications disappear, the whole length of the tube being filled with luminosity; when in this state, if the outside of the tube is touched, pungent sparks can be perceived $\frac{1}{8}$th of an inch in length, and the peculiar blue phosphorescent light, that in the ordinary state is perceptible at the negative, is perceptible at both terminals, and a galvanometer shows that the discharge is no longer conductive.

After noticing the difficulty of obtaining in carbonic acid vacuum-tubes precisely the same results, the author describes one experiment in which moisture was purposely introduced; in this tube the stratified discharge was very clear and distinct. He states (and describes the illustrative experiment) that under certain conditions the stratifications entirely disappear, the vacuum insulating the discharge. Carbonic acid vacuum-tubes were prepared, into which *arsenious acid, iodine, bromine, pentachloride of antimony, bichloride* and *bisulphide of carbon* were severally introduced, and the results obtained are described.

In Torricellian vacua the author was necessarily limited in the size of the glass vessels employed, but with carbonic acid this difficulty no longer exists; in one vessel of 7 inches internal diameter, the stratified discharge was observed to fill the entire space; in another, the discharges were made to pass in the middle of the vessel through a small hole in the centre of a glass diaphragm.

After many trials, the author ascertained that if the negative terminal is covered with glass tubing (open at each end) to about $\frac{1}{8}$th of an inch beyond the terminal of the wire, the stratifications are destroyed. In this state the negative discharge appears to issue with considerable force through the orifice; this discharge can be deflected by the magnet, and wherever it impinges, a brilliant blue phosphorescent spot is perceivable, which spot is in a short time sensibly heated. The author remarks that in this experiment there

is the *appearance of a direction of a force emanating from the negative.*

In some of the vacuum-tubes beyond the clear cloud-like stratifications, but nearer the negative terminal, several faint striæ can be obtained: on repeating Mr. Grove's experiment (Phil. Mag. July 1858), of allowing the discharge to pass between two metallic points attached to the coil, the author observed that these faint striæ *invariably* disappeared.

Stratifications remarkably sensitive to induction on the approach of the hand were obtained in a glass cylinder of about $4\frac{1}{2}$ inches diameter, in which the wires were hermetically sealed 21 inches apart.

From the absorption of carbonic acid by caustic potassa, not only were vacua obtained far more perfect than by the Torricellian method, but the process can be made so gradual as to occupy several weeks, or even months, thus enabling the experimenter to examine the phenomena of the stratified discharge under a variety of conditions, several of which the author describes; in this manner the *non-transferring condition for the electrical discharge in a vacuum has been experimentally ascertained.* The author considers that this confirms the opinion he ventured to offer in his previous paper; for if the pulsations or vibrations of an electrical discharge are greatest in the bright bands and least in the obscure, this system of interference or of pulsations would also account for the entire absence of stratifications when the air or gas is not sufficiently rarefied, as well as when the vacuum becomes nearly perfect, while the gradual change of narrow to cloud-like stratifications is thus satisfactorily explained.

In an additional note to his Paper, the author describes some further experiments, particularly one of moving the vacuum-tube to and fro in a rapid manner, or rotating it in a plane, while the discharges are made, either singly or continuously: in the latter case the stratified discharges are separated, giving the appearance of an illuminated fan or wheel; in the former, only a single discharge is perceptible, taking place in whatever direction the tube may at the instant be placed. The author considers this experiment as confirmatory of his former opinion, that the stratifications *are entirely due to a single disruption of the primary circuit.*

The experiments, as described in the Paper, were exhibited by the author to the Society.

January 20, 1859.

SIR BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "Second Note on Ozone." By THOMAS ANDREWS, M.D., F.R.S., and P. G. TAIT, M.A., F.C.P.S. Communicated by Dr. ANDREWS. Received December 16, 1858.

Since the publication of their "Note on the Density of Ozone" (Proceedings of the Royal Society, June 1857), the authors have been occupied with an extended investigation into the nature and properties of that body. The inquiry having proved more protracted than they anticipated, they have thought it proper to send to the Royal Society a brief notice of some of the more important facts which they have already observed, reserving a description of the methods employed, and of the details of the experiments, for a future communication.

The commonly received statement, that the whole of a given volume of dry oxygen gas contained alone in an hermetically sealed tube can be converted into ozone by the passage of electrical sparks, is erroneous. In repeated trials, with tubes of every form and size, the authors found that not more than $\frac{1}{100}$ part of the oxygen could thus be changed into ozone. A greater effect was, it is true, produced by the silent discharge between fine platina points; but this also had its limit. In order to carry on the process, it is necessary to introduce into the apparatus some substance, such as a solution of iodide of potassium, which has the property of taking up, in the form of oxygen, the ozone as it is produced. After many trials, an apparatus was contrived in the form of a double U, having a solution of iodide of potassium in one end, and a column of fragments of fused chloride of calcium interposed between this solution and the part of the tube where the electrical discharge was passed. The chloride of calcium allowed the ozone to pass, but arrested the vapour of water; so that, while the discharge always took place in dry oxygen, the ozone was gradually absorbed. The experiment is not yet finished, but already one-fourth of the gas in a tube of the

capacity of 10 cubic centimetres has disappeared. To produce this effect, the discharge from a machine in excellent order has been passed through the tube for twenty-four hours.

When oxygen is thus converted into ozone, a diminution of volume takes place. The greatest contraction occurs with the silent discharge, and amounts to about $\frac{1}{35}$ of the volume of the gas. The passage of sparks has less effect than the silent discharge, and will even destroy a part of the contraction obtained by means of the latter. If the apparatus be exposed for a short time to the temperature of 250° C., so as to destroy the ozone, it will be found that the gas on cooling has recovered exactly its original volume. This observation proves, unequivocally, that if ozone be oxygen in an allotropic condition, its density is greater than that of oxygen. Experiments still in progress indicate that the density of ozone obtained by the electrical discharge must, on the above assumption, be represented by even a higher number than that deduced by the authors from their experiments on ozone prepared by electrolysis.

When mercury is brought into contact with dry oxygen, in which ozone has been formed by the electrical discharge, it loses to a great extent its mobility, and may be made to cover the interior of the tube with a fine reflecting surface resembling that of an ordinary mirror. It is remarkable that this great change in the state of the mercury is not accompanied by any further diminution of the volume of the gas. The apparatus employed by the authors would have enabled them to estimate with certainty a change of volume amounting to $\frac{1}{12000}$ part of the whole. On the contrary, on allowing the apparatus to stand, the gas begins slowly to expand; and in thirty hours, when the ozone reactions have disappeared, the expansion amounts to a little more than one half of the contraction which had previously taken place.

Dry silver, in the state both of leaf and of filings, has the property of entirely destroying ozone, whether prepared by electrolysis or by the electrical machine. If a stream of electrolytic ozone be passed over silver leaf or filings contained in a tube, the metal becomes altered in appearance where the gas comes first into contact with it; but no appreciable increase of weight takes place, however long the experiment may be continued. The volumetric results are similar to those already described in the case of mercury.

Arsenic also destroys dry ozone, but, as it likewise combines with dry oxygen, its separate action on ozone cannot be observed with precision.

Most of the other metals examined, such as gold, platina, iron, zinc, tin, &c., are without action on dry ozone.

Iodine, brought into contact with oxygen contracted by the electric discharge, instantly destroys the ozone reactions, and a yellowish solid is formed: no change of volume accompanies this action.

Peroxide of manganese and oxide of copper have, it is well known, the property of destroying ozone, apparently without limit. The authors have found that these oxides undergo no sensible increase of weight, even after the destruction of 50 or 60 milligrammes of ozone. The same oxides, when brought into contact with oxygen contracted by the spark, restore it to nearly its original volume.

Hydrogen gas, purified with care, and perfectly dry, was not changed in volume by the action either of the electrical spark, or of the silent discharge.

A similar negative result was obtained with nitrogen and the silent discharge; but with the spark a very slight alteration of volume appeared to occur, the cause of which is still under investigation.

In the experiments now described, the electrical sparks and discharge were always obtained from the common friction-machine. The discharge from the induction coil, even when passed through two Leyden jars, produces very insignificant ozone effects. The heat which always accompanies this discharge, and its comparatively feeble tension, sufficiently explain its want of energy.

All the results recently obtained by the authors fully confirm the former experiments of one of them,* that in no case is water produced by the destruction of ozone, whether prepared by electrolysis or by the electrical discharge. They reserve any further expression of their views as to the true relations which exist between ozone and oxygen, till they shall have an opportunity of laying the results of this inquiry in a more complete form before the Society.

* Philosophical Transactions for 1856, Part I.

II. "Ice Observations." By DAVID WALKER, M.D., Surgeon and Naturalist to the Arctic Discovery Expedition. Communicated by THOMAS ANDREWS, M.D. Received December 16, 1858.

(Abstract.)

The contradictory statements of Dr. Sutherland and Dr. Kane, with regard to the saltness of the ice formed from sea-water,—the former maintaining that sea-water ice contains about one-fourth of the salt of the original water; the latter, that if the cold be sufficiently intense, there will be formed from sea-water a fresh and purer element fit for domestic use,—induced the author to take advantage of his position, as naturalist to the expedition now in the northern seas, to reinvestigate the subject.

The changes which he has observed sea-water to undergo in freezing are the following. When the temperature falls below $+28^{\circ}\cdot5$, it becomes covered with a thin pellicle of ice; after some time this pellicle becomes thicker and presents a vertically striated structure, similar to that of the ordinary cakes of sal-ammoniac. As the ice further increases in thickness, it becomes more compact, but the lowest portion still retains the striated structure. On the surface of the ice, saline crystals, designated by the author "efflorescence," soon begin to form, at first few in number and widely separated, but gradually forming into tufts and ultimately covering the whole surface. At first, the increase in thickness of the ice is rapid, but afterwards the rate of growth is much slower and more uniform. The ice formed yields, on being melted, a solution differing in specific gravity according to the temperature at the time of congelation, its density being less, the lower the temperature at which the process of congelation took place. Although the author's observations extended from $+28^{\circ}\cdot5$ to -42°, he was never able to obtain fresh-water from sea-ice, the purest specimen being of specific gravity 1·005, and affording abundant evidence of the presence of salts, especially of chloride of sodium, in such quantity as to render it unfit for domestic purposes.

The efflorescence already referred to appeared sooner or later, according to the temperature of the air, but generally commenced when the ice was $\frac{3}{8}$ of an inch thick, and continued to form till

the ice attained a thickness of about 9 inches, when, in consequence of the compactness of the frozen mass, it ceased to appear at the surface. The lower the temperature at which the ice was formed, the more abundant was the efflorescence. Direct experiments made by freezing sea-water in a large tub, showed that the unfrozen residuum contained a considerable portion of salts expressed from the ice. The author therefore infers, that after the efflorescence had ceased to form on the surface, the saline particles were precipitated into the unfrozen liquid below. On exposing the residual liquid from which the ice had been separated to a freezing temperature, a second residuum was obtained, containing more salts than the first; and by repeating the process several times, there remained finally a strong solution of brine.

The author endeavoured, by reversing this process, to procure fresh-water. He remelted the ice from sea-water and froze it again, repeating the operation several times. Ice was thus obtained, which, when melted, gave water, having a density of from 1·0025 to 1·0020.

A "heavy nip" having occurred in the floe near the ship afforded an opportunity of examining the quality of the ice at different depths. The thickness of the entire mass was 54 inches; the density of the solution obtained by melting successive portions varied from 1·0078 to 1·0050; those near the surface giving a liquid of higher density than the rest. A specimen taken from the centre of the mass was reserved for analysis.

With regard to the "efflorescence," the author states that its appearance was very different according as the temperature was above or below $-25°$. In the former case, it exhibited a plumose form, with secondary plumes branching off; in the latter, it consisted of fibrous crystals varying from $\frac{1}{4}$ to 2 inches in length. This efflorescence acts an important part in the breaking up of the floe. From the middle of January cracks and lanes occur in the floe, which subsequently become filled with new ice covered as usual with the saline efflorescence and a little snow. When the sun's rays fall upon this incrustation, it melts and forms a thick liquid on the top. This penetrates gradually through the ice and aids greatly in breaking it up. The author supposes that a process of endosmosis and exosmosis is, in fact, established through the body of the ice. A

similar, but less powerful, action is produced by the same cause on the mass of the floe itself.

In the artificial freezing of sea-water, the ice was found to be vertically striated, and often divisible into two or more layers, while the under surface was always marked by fine lines intersecting each other at definite angles. From the bottom of the vessel thin plates of ice formed in the unfrozen liquid. They varied in length from $\frac{1}{2}$ in. to $2\frac{1}{2}$ in., and contained less salt than the ice formed on the top.

To explain the observation of Dr. Kane as to the freshness of ice formed from sea-water under $-30°$, the author supposes that it may have depended on the freezing of a portion of sea-water which was covered at the time of its congelation with a stratum of fresh-water produced by the melting of bergs. On the 12th of April, 1857, whilst lying off Brown's Island, within about 4 miles of a glacier surrounded by bergs, the author observed a layer of fresh-water, 2 or 3 inches in depth, floating, like oil, on the surface of the salt-water. To this cause he attributes the occasional occurrence of hummocks from the upper portions of which ice perfectly free from salt can be obtained, while on digging deeper into these hummocks, the ice is always found to lose its freshness.

III. "Inquiries into the Phenomena of Respiration." By Edward Smith, M.D., Assistant-Physician to the Hospital for Consumption, Brompton. Communicated by Sir B. C. Brodie, Bart., P.R.S. Received December 16, 1858.

(Abstract.)

The author gives in this communication the result of numerous inquiries into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration,—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day, and from season to season; and 3rd, the influence of some kinds of exertion.

After a description of the apparatus employed by previous observers, he describes his own apparatus and method. This consists of a spirometer to measure the air inspired, capable of registering any number of cubic inches; and an analytical apparatus to abstract

the carbonic acid and vapour from the expired air. The former is a small dry gas-meter, of improved manufacture, and the latter consists of—1st, a desiccator of sulphuric acid to absorb the vapour; 2nd, a gutta-percha box, with chambers and cells, containing caustic potash, and offering a superficies of 700 inches, over which the expired air is passed, and by which the carbonic acid is abstracted; and 3rd, a second desiccator to retain the vapour which the expired air had carried off from the potash box. A small mask is worn, so as to prevent any air entering the lungs without first passing through the spirometer, and the increase in the weight of this with the connecting tube and the first desiccator gives the amount of vapour exhaled, whilst the addition to the weight of the potash box and the second desiccator gives the weight of the carbonic acid expired. The balances employed weigh to the $\frac{1}{100}$ of a grain, with 7 lbs. in the pan. By this apparatus the whole of the carbonic acid was abstracted during the act of expiration, and the experiment could be repeated every few minutes, or continued for any number of hours, and be made whilst sleeping and with certain kinds of exertion.

The amount of carbonic acid expired in the twenty-four hours was determined by several sets of experiments. Four of these, consisting of eight experiments, were made upon four gentlemen, on the author, Professor Frankland, F.R.S., Dr. Murie, and Mr. Moul, during the eighteen hours of the working day. In two of them, the whole of the carbonic acid was collected, and in two others the experiment was made during ten minutes at the commencement of each hour, and of each hour after the meals. The quantity of carbonic acid varied from an average of 24·274 oz. in the author to 16·43 oz. in Professor Frankland. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and when scarcely awake 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute; and the whole evolved in the six hours of the night at 1950 grains. Hence the total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 oz. The effect of walking at various speeds is then given, with an estimate of the amount of exertion made by different classes of the community, and of the carbon which would be evolved with that exertion.

The author then states the quantity of air inspired in the working

day, which varied from 583 cub. in. per minute in himself to 365 cub. in. per minute in Professor Frankland; the rate of respiration, which varied in different seasons as well as in different persons; the depth of inspiration, from 30 cub. in. to 39·5 cub. in.; and the rate of pulsation. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. One-half the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 gr. to 54·7 cub. in. to as 1 gr. to 58 cub. in. The variations in the carbonic acid evolved in the working day gave an average maximum of 10·43, and minimum of 6·74 grains per minute. The quantity increased after a meal and decreased from each meal, so that the minima were nearly the same, and the maxima were the greatest after breakfast and tea.

The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food; to render the quantity nearly uniform throughout the day, with a little increase at the hours when food had usually been taken, and to cause the secretions to become alkaline*.

The variations from day to day were shown to be connected with the relation of waste and supply on the previous day and night, so that with good health, good night's rest, and sufficient food, the amount of respiration was considerable on the following morning, whilst the reverse occurred with the contrary conditions. Hence the quantities were usually large on the Monday. Temperature was an ever-acting cause of variation, and caused a diminution in the carbonic acid as the temperature rose.

The effect of season was to cause a diminution of all the respiratory phenomena as the hot season advanced. The maximum state was in spring, and the minimum at the end of summer, with periods of decrease in June and of increase in October. The diminution in the author was 30 per cent. in the quantity of air, 32 per cent. in the rate of respiration, and 17 per cent. in the carbonic acid. The influence of temperature was considered in relation to season, and it

* The quantity of air was reduced 30 per cent., that of vapour in the expired air 50 per cent., the rate of respiration was reduced 7 per cent., and of pulsation 6 per cent.

was shown that whilst sudden changes of temperature cause immediate variation in the quantity of carbonic acid, a medium degree of temperature, as of 60°, is accompanied by all the variations in the quantity of carbonic acid, and that there is no relation between any given temperature and quantity of carbonic acid at different seasons. Whatever was the degree of temperature, the quantity of carbonic acid, and all other phenomena of respiration, fell from the beginning of June to the beginning of September. The author then described the influence of atmospheric pressure, and stated that neither temperature nor atmospheric pressure accounts for the seasonal changes.

The kinds of exertion which had been investigated were walking and the treadwheel. Walking at two miles per hour induced an exhalation of 18·1 gr. of carbonic acid per minute, and at three miles per hour of 25·83 grs.; whilst the effect of the treadwheel at Coldbath Fields Prison was to increase the quantity to 48 grs. per minute. All these quantities vary with the season, and hence the author recommends the adoption of relative quantities, the comparison being with the state of the system at rest, and apart from the influence of food.

The apparatus and various drawings were exhibited.

January 27, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Dr. John Hutton Balfour was admitted into the Society.

In accordance with notice given at the last Meeting, the Right Rev. the Lord Bishop of London was proposed for election and immediate ballot.

The ballot having been taken, his Lordship was declared duly elected.

The following communications were read:—

I. "On the Effect of Pressure on Electric Conductibility in Metallic Wires." In a Letter from M. ELIE WARTMANN of Geneva, to Major-General SABINE, Treas. and V.P.R.S. Communicated by Prof. W. H. MILLER, For. Sec. R.S. Received January 12, 1859.

Geneva, January 3rd, 1859.

My dear Sir,—The newspapers having reported that a society of English shareholders intends to lay a second cable for transatlantic telegraphy, you will allow me to give here a brief account of some experiments by which I have succeeded in proving the effect of pressure on electric conductibility in metallic wires.

The method which I have resorted to is the one devised by MM. Christie and Wheatstone, which is called the electrical bridge. The current of a Bunsen's battery of six large cells was divided between the wire to be tested (a very soft copper wire 0·05 of an inch in diameter, and covered with gutta percha) and another conductor; both being connected with a delicate Ruhmkorff's galvanometer, so that the needle remained on the zero point. All contacts were made invariable by solderings.

No sensible effect being determined by the pressure of nine atmospheres in a piezometer, I made use of a press which enabled me to produce compressions superior to four hundred atmospheres, consequently superior to that which is suffered by an electric conductor immersed in the ocean, at a depth of 12,420 English feet. The wire, besides its coating, was preserved against permanent deformation by two sheets of thick gutta percha, placed between the steel plates which took hold of it.

The experiments have shown—

1°. That a pressure of thirty atmospheres (a number relative to the sensibility of the galvanometer) diminishes the conducting power of a copper wire for electricity.

2°. That the effect increases with the pressure.

3°. That the diminution remains the same for each compression, as long as the latter does not vary.

4°. That the primitive conducting power is exactly restored when the pressure vanishes altogether.

Many interesting results flow from these conclusions, which I pro-

pose to examine in a future letter. For the present, permit me to add, that the fact which I have discovered establishes a new connexion between electricity, heat, and light: for it has been demonstrated by M. de Senarmont—

a. That any artificial increase of density in a non-crystallized solid body diminishes, in the direction in which it is exerted, the conducting power of that body for heat.

b. That in homogeneous media which are in a state of artificial molecular equilibrium, the conformation of the thermic ellipsoid, either oblate or prolate, is always corresponding to that of the optic one.

I shall feel much gratified if you deem this communication worthy to be laid before the Royal Society. * *

I remain, &c.,

ELIE WARTMANN.

II. "Notice of Researches on a New Class of Organic Bases, conducted by CHARLES S. WOOD, Esq." By A. W. HOFMANN, LL.D., F.R.S. Received December 21, 1858.

In his remarkable memoir* on the action of reducing agents on nitro-compounds, in which Zinin first pointed out the formation of organic bases by the substitution of hydrogen for oxygen, some experiments are recorded on the deportment of dinitro-naphtalin (nitronaphtalese) with sulphuretted hydrogen. Zinin states that this process gives rise to the formation of a basic compound crystallizing in delicate copper-red needles, and yielding with acids white scaly salts.

In a subsequent paper† Zinin returns to the action of sulphuretted hydrogen or dinitronaphtalin, and gives a fuller account of the products obtained in this process. The basic substance arising from dinitronaphtalin crystallizes in colourless needles of great brilliancy, which contain

$$C_{10} H_5 N, \text{ or } C_{20} H_{10} N_2.$$

It is a well-defined basic body, which Zinin describes under the name of seminaphlalidam. From this later communication it would

* Bulletin Scientifique de St. Pétersburg, x. 18.

† Journ. für Prakt. Chem. Bd. xxxiii. 29.

appear that the copper-red coloration originally observed was due to the presence of a foreign colouring matter, which can be separated by crystallizing the base alternately from alcohol and water.

Subsequently the copper-red body appears to have been observed by Laurent*, who states that the action of sulphuretted hydrogen upon dinitronaphtalin gives rise to the formation of a carmine-red alkali. He did not, however, analyse this substance, and the discovery of nitraniline† having established the existence of basic nitro-substitutes, the compound in question was hitherto believed to be nitro-naphtylamine.

The red crystals have of late been minutely investigated in my laboratory by Mr. Charles Wood, whose experiments have led to an unexpected result, which I beg to lay before the Society.

A current of sulphuretted hydrogen transmitted through a boiling solution of dinitronaphtalin in weak alcoholic ammonia slowly reduces the nitro-compound. The process is continued for two or three hours, during which time the greater part of the spirit distils off; the residue is acidified with dilute sulphuric acid, and the liquid heated to ebullition. The filtered liquid deposits on cooling a yellowish brown sulphate, which may be purified by several crystallizations from boiling water. The addition of ammonia to the solid sulphate immediately changes the colour to a fine dark carmine-red; the base thus liberated is washed with cold water, and finally purified by crystallization from water or very dilute alcohol.

Thus prepared, the substance, for which Mr. Wood proposes the name *ninaphtylamine*, is a light flocculent mass, composed of little acicular crystals, which are partially decomposed by exposure to a temperature of 100° C. It is difficultly soluble in boiling water, but extremely soluble in alcohol and ether.

In the analysis of the base dried *in vacuo* over sulphuric acid, Mr. Wood has obtained results which lead to the formula

$$C_{20} H_8 N_2 O_2.$$

This expression was confirmed by the examination of several of the salts of the new base.

Sulphate of ninaphtylamine is obtained either by recrystallizing the crude salt formed in the preparation of the body, or by dis-

* Compt. Rend. xxxi. 538.

† Muspratt and Hofmann, Memoirs of the Chemical Society, vol. iii. 111.

solving the pure base in dilute sulphuric acid. It forms white scales, which are apt to be decomposed by recrystallization from pure water. The salt dried *in vacuo* over sulphuric acid contains

$$2(C_{20}\,H_8\,N_2\,O_2),\quad H_2\,S_2\,O_8.$$

Hydrochlorate of ninaphtylamine forms acicular crystals; they are obtained like the sulphate, which they resemble in their general deportment. Composition:

$$C_{20}\,H_8\,N_2\,O_2,\,H\,Cl.$$

The *platinum-salt of ninaphtylamine* forms rather soluble yellowish-brown crystals, which are obtained by adding a concentrated solution of dichloride of platinum to an alcoholic or ethereal solution of the base. It has the usual constitution, containing

$$C_{20}\,H_8\,N_2\,O_2,\,H\,Cl,\,Pt\,Cl_2.$$

If it be permitted, in the absence of further experimental evidence, to speculate upon the molecular constitution of the body which forms the subject of this note, the simplest interpretation of its composition and formation would be to view it as a substitution-product of naphtylamine, but differing from the ordinary nitro-substitutes, by containing the elements of binoxide, instead of tetroxide of nitrogen.

$$\text{Naphthylamine} \;..\; C_{20}\,H_9\,N = \left.\begin{matrix} C_{20}\,H_7 \\ H \\ H \end{matrix}\right\}N,$$

$$\text{Ninaphthylamine}..\; C_{20}\,H_8\,N_2\,O_2 = \left.\begin{matrix} C_{20}\,(H_6\,NO_2) \\ H \\ H \end{matrix}\right\}N.$$

Its formation would then be represented by the equation

$$\underbrace{C_{20}\,[H_6\,(NO_4)_2]}_{\text{Dinitronaphtalin.}} + 4\,H_2S_2 = 3\,H_2O_2 + 8\,S + \underbrace{C_{20}\,[H_8\,NO_2]\,N}_{\text{Ninaphthylamine.}}$$

Bodies in which binoxide of nitrogen figures as a material of substitution are as yet extremely rare, whilst nitro-substitutes containing the elements of hyponitric acid are of the most general occurrence.

Some chemists have considered nitrous ether as a binoxide of nitrogen derivative of alcohol.

$$\text{Alcohol} \;................\; C_4\,H_6\,O_2,$$

$$\text{Nitrous ether} \;............\; C_4\begin{pmatrix} H_5 \\ NO_2 \end{pmatrix}O_2.$$

The most interesting illustrations of this kind of substitution, how-

ever, have been furnished by Messrs. Church and Perkin* in the colouring matters produced by the action of nascent hydrogen on dinitro-substitutes, or of nitrous acid upon certain monamines.

$$\text{Phenylamine} \ldots\ldots\ldots\ldots \quad C_{12}\,H_7\,N,$$

$$\text{Nitrosophenyline} \ldots\ldots\ldots \quad C_{12}\begin{pmatrix} H_6 \\ NO_2 \end{pmatrix}N.$$

$$\text{Naphthylamine} \ldots\ldots\ldots\ldots \quad C_{20}\,H_9\,N,$$

$$\text{Nitrosonaphtylin} \ldots\ldots\ldots \quad C_{20}\begin{pmatrix} H_8 \\ NO_2 \end{pmatrix}N.$$

Expressed by these formulæ, the substances in question appear to be closely allied to Mr. Wood's base; in fact, nitrosonaphtylin has the same composition as ninaphthylamine. But a superficial comparison of the properties of the two bodies excludes any idea of their being identical. The formulæ of nitrosophenyline and nitrosonaphtylin have not as yet been finally established by the analysis of their compounds, these substances, like colouring matters in general, being of an indifferent character. It is probable that they are formed by the association of several molecules, a supposition which receives considerable support from the discovery of ninaphthylamine.

The formation of ninaphthylamine promises to add considerably to the number of nitro-derivatives of the aromatic monamines. To each of these substances probably corresponds a nitrous and a nitric substitution-base, but as yet we are unacquainted with a single one in which both derivatives are known, as shown by a glance at the groups best examined.

Phenyl. Group.		Naphtyl. Group.	
Phenylamine....	$\left.\begin{matrix} C_{12}\,H_5 \\ H \\ H \end{matrix}\right\}N,$	Naphthylamine	$\left.\begin{matrix} C_{20}\,H_7 \\ H \\ H \end{matrix}\right\}N.$
Unknown ..	$\left.\begin{matrix} C_{12}\,(H_4\,NO_2) \\ H \\ H \end{matrix}\right\}N,$	Ninaphthyl-amine....	$\left.\begin{matrix} C_{20}\,(H_6\,NO_2) \\ H \\ H \end{matrix}\right\}N.$
Nitrophenyl-amine....	$\left.\begin{matrix} C_{12}\,(H_4\,NO_4) \\ H \\ H \end{matrix}\right\}N,$	Unknown	$\left.\begin{matrix} C_{20}\,(H_6\,NO_4) \\ H \\ H \end{matrix}\right\}N.$

* Journal of Chemical Society, vol. ix. 1.

III. "Rectification of Logarithmic Errors in the Measurements of Two Sections of the Meridional Arc of India." In a Letter to Professor STOKES, Sec. R.S. By Colonel EVEREST, F.R.S. Received December 22, 1858.

It will be in your recollection, that some years ago, at the request of the Court of Directors of the East India Company, I compiled from the General Report of the Great Trigonometrical Survey of India, a work entitled "An Account of the Measurement of two Sections of the Meridional Arc of India," executed by myself and my assistants, an impression of which (printed in 1847) was presented to the Royal Society.

In reference to the work in question, I now have to mention that in the computation of the meridional triangles (*vide* pp. 240 to 248) there have been some errors committed in taking out the logarithms of the twelfth triangle (p. 241) and the twentieth triangle (p. 243); and as from the nature of that series, the purpose of which is to project the sides of the principal triangles on the meridian, any such error must run through all the computations subsequent to it, I have had the whole series recomputed, and now forward for submission to the Royal Society, a sheet containing a revised synopsis (*vide* p. 248), such as it would have been, but for the errors adverted to, to which it is my wish that the utmost publicity should be given.

I must here say, that I owe the detection and correction of the said errors entirely to the industry of Colonel Waugh, F.R.S., and the able computing establishment at his disposal; for myself it remains only to urge, that though mistakes of this nature are not creditable, but, on the contrary, much to be regretted, yet it is all but impossible for the chief of two departments, that of Surveyor-General of India, and that of Superintendent of the Great Trigonometrical Survey of India, each involving a vast array of business peculiar to itself, to enter into all the minutiæ of computation; I took the precaution to have every portion of the work gone over by two computers acting independently, and it is singular that both should have fallen into the same errors.

As the existence of these errors, in the instance of the northern of the two sections or series A, would naturally lead to the supposition that like errors might lurk in series B (pp. 46 to 53), an equally

rigorous recomputation has been kindly instituted by Colonel Waugh in the latter case; and as no mistake is found therein, the synopsis B (p. 53) will need no alteration on that account.

I have now to advert to another subject, also relating to the work in question. In trigonometrical operations, I need hardly mention that the absolute lengths of all linear quantities depend on those of the measured bases reduced to their equivalents at the level of the sea, the reduction on which account may be expressed by $\frac{Bh}{R}$, where B is the measured base, h its mean height above the sea, and R the radius of curvature: now as we do not know *a priori* the value of h, unless the measurements be actually made on the sea-shore, the only mode of commencing the work of computation is to assume the nearest value we are in possession of, and when the operations are connected with the sea-coasts, to apply a correction for any excess or defect of our assumption.

Damargida, the southernmost point included in my book of 1847, is an inland station, the height of which is given in the general report of my predecessor, Colonel Lambton, at 2026 feet; and as all the triangulation to the south of that station had been concluded in 1815, nearly four years before I joined the department, I had no choice but to refer to that as one of my established data: my computations start from the Sironj base, and on reaching Damargida it appeared that the assumed value of h, which I had used in correcting that base line, was 24·5 feet in excess of what it ought to have been, for which the correction is applied of 2·578 feet to the linear value of the terrestrial arc in synopsis B, and 2·295 in synopsis A.

Colonel Lambton's operations, from which the height of Damargida station is determined, abut on the sea-coast at three different points, viz. Madras on the east coast, Mangalore on the west coast, and Cape Comorin on the southernmost extremity of the Peninsula, and the results thence derived were at that time the most trustworthy data I had access to.

Subsequent to the completion of my computations, the western longitudinal series which connects Damargida with the sea-coast near Bombay, was finished by Captain Jacob, then one of my assistants, and there is a note on that subject at page clxxi. of my book of 1847, which is, I presume, quite sufficient to prepare any

geodist for the probability of a future correction being needed: as to Captain Jacob's performance, I may distinctly state that I place the greatest confidence in its accuracy, for he was furnished with an excellent altitude and azimuth instrument by Dollond, which had a good vertical circle, and he was not only a highly-talented mathematician, but a most careful and skilful observer.

Since I left India, in December 1843, the trigonometrical operations have abutted on the sea-coast at two other places, viz. at Calcutta, by the completion of my north-east longitudinal series, and at Karachi, by Colonel Waugh's western longitudinal series; so that now there are several additional data relative to the numerical value of *h*, which it becomes necessary to take into consideration in the determination of the proper corrections to be applied to the measured bases, and the lengths of my two terrestrial arcs, as given in synopses B and A.

I have for some time been in correspondence with Colonel Waugh on this subject, and in due time hope to be able to communicate what final conclusion we arrived at; but, in the meanwhile, as it may be interesting to the Fellows of the Royal Society to know the provisional state of the question, I here subjoin some extracts of his last received communication.

"By the completion of the Calcutta meridional series, as well as the north-east longitudinal series, we obtain a continued chain of triangulation, extending from the sea-level near Calcutta along the Calcutta meridian to the Sonakoda base, thence along the north-east longitudinal series to Dehra Dun, thence down the great arc, and along the Bombay longitudinal series to the sea-level at Bombay. The result of trigonometrical levelling along this course of 2127 miles, comprising 1171 miles of hills, and 956 miles of plains, becomes verified; the discrepancy in height being 6·76 feet or 0·003 foot per mile.

"By the completion of the great longitudinal series, we get another continuous chain of triangulation from the sea-level near Calcutta to the same level at Karachi. This chain embraces the Calcutta meridional series, the north-east longitudinal series, the northern section of the great arc from Dehra Dun to Sironj, and the great longitudinal series from Sironj to Karachi. This series is 2082 miles in length, comprising 1041 miles of plains, and 1041 miles of

hills, and the error by trigonometrical levelling is 16·50 feet, or 0·008 foot per mile.

"When the great Indus series is finished, the sea-level will be brought up to the Chueh base line, and from thence along the north-west Himalayan series to Dehra Dun. The result of this series will give another verification, which will be especially valuable, as it is intended to level along the flat valley of the Indus by special levelling operations. It is also proposed to carry special levelling operations from Dehra Dun to Calcutta. Pending the completion of these undertakings, it has been found necessary to correct provisionally the results given by trigonometrical levelling, in order to disperse the discrepancies above shown, and reduce the results of the three data for sea-level near Calcutta, Bombay, and Karachi. The results, so reduced, become comparative, *inter se*, and are required for record on the general maps."

The upshot of all this is, that Colonel Waugh has provisionally applied the following corrections to the two sections of the great arc, as given in synopsis A, p. 248, and synopsis B, p. 53, of my book of 1847, in lieu of those formerly applied; and I must say that I think they will be nearer the truth than those given in that book, because the discrepancy specified in the note at the foot of p. clxxi., taken into combination with all the results of the general trigonometrical survey since obtained, certainly point to some error in Colonel Lambton's operations to the south of Damargida. The two sections, applying Colonel Waugh's corrections, will stand as follows:—

Section A.

	Feet.
Length of the arc deduced from the Sironj base	1961155·422
Add correction for 111·14 feet in height at ditto	10·412
Add correction η, as explained in p. clxxi.	12·387
Total arc, Kalianpur to Kaliana, by provisional data	1961178·221

Section B.

	Feet.
Length of the arc deduced from the Sironj base	2202914·322
Add correction for 111·14 feet in height at ditto	11·695
Add correction η, as explained in p. clxxi.	8·679
Total arc, Kalianpur to Damargida, by provisional data	2202934·696

Series A, Synopsis, showing the length of the Terrestrial Arc comprised between Kalianpur and Kaliana.

No.	Points of Intersection.			Distances in Feet.		Results.
				Positive.	Negative.	
1	Kalianpur	to	μ^{i}	78993·100	....	
2	μ^{i}	to	μ^{ii}	....	53204·174	
3	μ^{ii}	,,	μ^{iii}	164528·038	....	304969·223
4	μ^{iii}	,,	μ^{iv}	....	4238·479	
5	μ^{iv}	,,	μ^{v}	118890·738	....	
6	μ^{v}	,,	μ^{vi}	....	17782·494	
7	μ^{vi}	,,	μ^{vii}	90318·962	....	
8	μ^{vii}	,,	μ^{viii}	22495·673	....	
9	μ^{viii}	,,	μ^{ix}	128257·147	....	297104·734
10	μ^{ix}	,,	μ^{x}	....	21907·376	
11	μ^{x}	,,	μ^{xi}	102857·688	....	
12	μ^{xi}	,,	p^{i}	....	7134·866	
13	p^{i}	,,	μ^{xii}	11359·821	....	
14	μ^{xii}	,,	μ^{xiii}	66041·591	....	
15	μ^{xiii}	,,	μ^{xiv}	41613·757	....	
16	μ^{xiv}	,,	μ^{xv}	47331·477	....	427109·613
17	μ^{xv}	,,	μ^{xvi}	62497·787	....	
18	μ^{xvi}	,,	μ^{xvii}	49466·835	....	
19	μ^{xvii}	,,	μ^{xviii}	135674·688	....	
20	μ^{xviii}	,,	p^{ii}	13123·657	....	
21	p^{ii}	,,	μ^{xix}	....	11798·757	
22	μ^{xix}	,,	p^{iii}	113184·729	....	
23	p^{iii}	,,	μ^{xx}	....	67673·046	328204·457
24	μ^{xx}	,,	μ^{xxi}	166111·919	....	
25	μ^{xxi}	,,	μ^{xxii}	....	47224·277	
26	μ^{xxii}	,,	μ^{xxiii}	175603·889	....	
27	μ^{xxiii}	,,	μ^{xxiv}	....	4171·424	
28	μ^{xxiv}	,,	μ^{xxv}	136368·819	....	
29	μ^{xxv}	,,	μ^{xxvi}	....	43939·700	320212·458
30	μ^{xxvi}	,,	μ^{xxvii}	169944·819	....	
31	μ^{xxvii}	,,	μ^{xxviii}	....	45108·400	
32	μ^{xxviii}	,,	μ^{xxix}	107118·344	....	
33	μ^{xxix}	,,	μ^{xxx}	....	2623·485	
34	μ^{xxx}	,,	μ^{xxxi}	94298·944	....	
35	μ^{xxxi}	,,	μ^{xxxii}	42013·658	....	
36	μ^{xxxii}	,,	μ^{xxxiii}	79728·356	....	283554·937
37	μ^{xxxiii}	,,	μ^{xxxiv}	66898·846	....	
38	μ^{xxxiv}	,,	p^{iv}	3238·631	....	
39	p^{iv}	,,	p^{iv}	....	0·013	

Length of the Arc, deduced from the Seroz Base......	1961155·422
Add correction for 24·5 feet difference of height......	2·295
Add correction η, as explained elsewhere..............	13·108
Total Terrestrial Arc from the two Bases in feet......	**1961170·825**

I must call attention to the fact, that all the remarks of Colonel Waugh have reference to the operations carried on since 1832 by me, himself, and our assistants, and are quite disconnected from Colonel Lambton's results to the south of Damargida, which were deduced in the early part of 1815. Now, as the two sets of operations unite at Damargida, it would be illogical to correct the southern portion of the Indian arc by one value of h, and the northern portion by another value of h; for, manifestly, no spot on the earth's surface can have two distinct heights above the sea differing from each other by 100 feet or so at the same instant.

It is rather a delicate matter for me to speak in terms of comment of the labours of my predecessor, and I had much rather that anybody else should undertake the invidious task; but thus much is very certain, that the instruments employed prior to 1832, were not such as we should use with confidence now-a-days, whilst those in use since that period are certainly not surpassed by any in existence.

In 1842 I was sensibly alive to the probable inaccuracy of the operations between Damargida and Cape Comorin, and, as the only effectual remedy, proposed to the Government of India to revise that work with the new instruments; but the proposal was rejected; so that instead of forming, as it would and ought to have done, one unbroken series from Cape Comorin to the Himalayan Mountains, the great arc of India now consists of two distinct patches; one to the south of Damargida for ever uncertain as to its unit of measure, and executed with instruments which we should now pronounce to be crazy and unserviceable; whilst the other to the north depends on a unit tolerably well defined, and was performed with instruments as perfect as can be desired. Of course, it is not for me to offer any remarks about the propriety of the decision of the Right Honourable the Governor-General of India in Council, and the Court of Directors of the East India Company, of those days, in a communication of this nature, except that, as far as science is concerned, perhaps it will now and hereafter be lamented that all my arguments for a revision were urged in vain.

It will appear, from what I have above stated, that the question of the amount of correction to be applied to my two sections A and B, is not yet finally settled, and is still uncertain to the extent of

about 10 feet; for which reason I have employed in the synopsis A, now forwarded, the same values as before, correcting only the obvious errors to which I have drawn attention in the second paragraph of this letter; at the same time I have endeavoured to put within the reach of those who interest themselves in the problem of the figure of our planet, all the data at my command, and if any fresh light should hereafter be thrown on the subject, shall be very happy to communicate it.

GEORGE EVEREST.

IV. "On the Thermodynamic Theory of Steam-engines with dry saturated Steam, and its application to practice." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E., Pres. Inst. Eng. Scot., Regius Professor of Civil Engineering and Mechanics in the University and College of Glasgow. Received December 27, 1858.

(Abstract.)

In 1849 it was demonstrated, contemporaneously and independently, by Professor Clausius and the author of this paper, from the laws of thermodynamics, that when steam or other saturated vapour in expanding performs work, and receives no heat from without, a portion of it must be liquefied.

That theoretical conclusion has since been confirmed by practical experience.

The principal effect of the "steam-jacket" invented by Watt is to prevent that liquefaction.

The presence of liquid water in any considerable quantity in the cylinder of a steam-engine acts injuriously, by taking heat from the steam while it is being admitted, and giving out that heat to the steam which is about to be discharged. Most of the heat so transferred is wasted.

The only *exact thermodynamic* formulæ for the work of steam hitherto published (by the author in the Phil. Trans. 1854, and by Professor Clausius in Poggendorff's 'Annalen,' 1856), are adapted to steam which receives no heat in expanding.

The present paper, after recapitulating the general equation of thermodynamics, and the special formulæ for the pressure, volume,

and latent heat of steam, proceeds to the investigation of the exact formulæ for the work of steam which is supplied during its expansion with just enough of heat to prevent any appreciable portion of it from condensing, for the expenditure of heat in producing and using that steam, and for its efficiency in producing motive power.

There is explained a convenient approximation to the exact formulæ, founded on the facts, that for initial pressures of steam of from 30 to 120 lbs. on the square inch (*including* atmospheric pressure), and for ratios of expansion up to *sixteen,* the pressure of saturated steam varies nearly as the *seventeenth power* of the *sixteenth root* of its density, and that the expenditure of heat in an engine in which dry saturated steam is used, expressed in units of energy, is nearly equal to *fifteen-and-a-half* times the product of the initial pressure and volume of the steam expended.

Lastly, there are given examples of the application of the formulæ to the engines of three steam-vessels lately experimented on by the author. The displacements of those ships are from 700 to 1100 tons; the indicated horse-power of their engines from 226 to 1180; the initial absolute pressures of steam in their cylinders range from 32 to $108\frac{1}{2}$ lb. on the square inch, and the ratios of expansion from 4 to 16. In each case the difference between the results of calculation and experiment is within the limits of error of observation, and ranges from $\frac{1}{80}$ to $\frac{1}{200}$ of the actual work of the steam.

The author has computed Tables of the results of the formulæ, exact and approximate, which are now in the course of being printed.

Summary of Formulæ.—*Notation and Constants.*

t, absolute temperature in degrees of Fahrenheit, $=$ temperature measured from the ordinary zero $+ 461^{\circ}\cdot 2$.

p, pressure in pounds on the *square foot.*

v, volume of one pound of steam in cubic feet.

t_1, p_1, v_1, refer to the admission of steam into the cylinder.

t_2, p_2, v_2, to the end of the expansion.

$r = v_2 \div v_1$, ratio of expansion.

$p_3 =$ pressure of exhaustion.

t_4, absolute temperature of feed-water.

J, "Joule's equivalent," or specific heat of one pound of liquid water, $=772$ foot-pounds per degree of Fahrenheit.

W, work of one pound of steam.

$\mathfrak{H}$, expenditure of heat per pound of steam in foot-pounds.

a=1109550 foot-pounds.

b=540·4 foot-pounds per degree Fahrenheit.

Efficiency of steam, $W \div \mathfrak{H}$.

Exact Formulæ.

$$W = a \text{ hyp. log } \frac{t_1}{t_2} - b(t_1 - t_2) + v_2(p_2 - p_3).$$

$$\mathfrak{H} = a\left(1 + \text{hyp. log } \frac{t_1}{t_2}\right) - bt_1 + J(t_2 - t_4).$$

Approximate Formulæ.

$$W \div v_2 = p_1(17r^{-1} - 16r^{-\frac{17}{16}}) - p_3.$$

$$\mathfrak{H} \div v_2 = \frac{15\frac{1}{2}p_1}{r}.$$

In applying the exact formulæ, the relations between p, v, and t may be found by means of known formulæ or Tables.

February 3, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "On Platinized Graphite Batteries." By C. V. WALKER, Esq., F.R.S., F.R.A.S., &c. Received January 4, 1859.

In a short note communicated to the Royal Society on March 9th, 1857, and which was read on March 19th, reference was made to the voltaic combination that I had adopted for certain telegraphic purposes; namely, zinc-graphite. Graphite in its crude state had for some years been of great service to me, especially for batteries whose resting time is great in proportion to their working time. Since the date of that notice, I have considerably increased the value of graphite for electrical purposes by platinizing it according to the process first described by Mr. Smee, whose platinized silver battery has been long known and much used. The material to which I refer by the term "Graphite," is the crust or corrosion that is collected from the interior of iron gas retorts that have been long in use.

My first crude graphite battery of twelve pairs of plates was set up on April 5th, 1849, for working the telegraph from my residence at Tonbridge to the Telegraph Office about a mile distant. It was charged with sand saturated with diluted acid; and had not been dismounted in March 1851, when I changed my abode. During the interval, the sand was from time to time moistened with acid water or water only. The plates in this case had been roughly chipped out and rubbed on stone into something like shape. In the mean time I had some sets of plates cut at the Locomotive Works, Ashford, and was thus enabled to obtain further results. I forwarded a graphite battery to the Great Exhibition in 1851, for which a prize medal was awarded. The introduction of graphite into anything like general use was for a long period no easy matter, on account of the difficulty of finding any one who would undertake to cut it into plates, its hardness destroying the tools; and the then limited demand did not encourage any one to construct special machinery for the purpose. My wants at length reached the ear of Mr. J. Robinson of Everton, Liverpool, who took the matter thoroughly in hand, and has succeeded perfectly in cutting plates into any form and to comparatively any size, at a very moderate cost, for which I am much indebted to him. I have before me plates 12 inches $\times$ 10 inches, of smooth texture and uniform thickness, and have seen some of double that size.

The plates in common use for bell signals are $7\frac{1}{2}$ inches $\times$ 3 inches and $\frac{3}{8}$ inch thick, of which about 2000 are in daily use on the South Eastern Railway, and the greater portion of these are now platinized. The plates are delivered to me in their crude state, that is to say, they are merely cut into form. Immediately on arrival they are placed in a stone pan, and covered with a mixture of 1 sulphuric acid + 4 water, in which they are allowed to remain for three or four days or more. They are taken out as required, and are washed under a tap of running water; this operation dissolves out any foreign matter that might be pernicious in a voltaic combination wherein sulphuric acid was employed; they are then partially dried. A hole for a rivet is next drilled in the middle, near the top of each plate—a belt of varnish one inch wide is applied to the top on both sides of each plate—a blank one inch square, having the rivet hole for its centre, being left unvarnished on each side—electrotype copper is

then deposited on the blank square in the usual way. The deposited metal is then tinned, no part of the copper being left bare; a connecting slip of copper, 6 inches × 1 inch is prepared and also entirely tinned; this is riveted to the graphite plate with a copper rivet, also tinned. The soldering iron is now applied, and a little solder run in between the two surfaces. By thus protecting all the exposed copper with tin, the formation of sulphate of copper and its attendant inconveniencies are prevented. The plate is now platinized.

A mixture of 1 sulphuric acid + 10 water is placed in a vertical glass cell, to this are added a few crystals of chloride of platinum till the solution presents a faint straw colour. The battery power employed for platinizing is three cells of platinized graphite and zinc. The positive electrode is platinum or graphite itself, and is presented to both sides of the plate that is to be platinized. The action is allowed to go on for about twenty minutes. Each finished plate is tested as to its power of liberating the hydrogen of electrolysis, by placing it in acid water in contact with an amalgamated zinc plate.

I have drawn out the above description in the presence of our assistant, who attends to this department of the telegraph establishment, in order to be correct in the small details.

The battery-cells for the plates above described are quart jars of stone-ware that resists acid. The exciting solution is 1 sulphuric acid + 8 to 12 water. Zinc plates are riveted to the other end of the copper connecting slip, also with tin rivets. The zinc is strongly amalgamated. It is dipped in a vessel containing 1 sulphuric acid + 4 water, and after a few seconds, more or less, is withdrawn and thrust in its then condition into a trough of mercury, and set aside to drain. On the following day it is treated in a similar manner. When the batteries are being put together, and before the zincs are placed in the jars, the foot of each is placed in a trough or slipper of gutta percha, 3 inches by $\frac{1}{4}$ inch, containing about a couple of ounces of mercury. A battery thus carefully prepared will stand for an indefinitely long period with little perceptible waste, and be ready for use at all times. Under ordinary circumstances it is not necessary to dismount the batteries employed for telegraph signaling more than once a year. Mercury is added during the interval, and the jars are filled up as occasion requires. The greater portion of the mercury is recovered: when old plates come home, a considerable

quantity of rich amalgam is scraped from the plates; this is placed in jars of acid water, and a few pieces of graphite are thrown in; the electro-chemical action makes the amalgam poorer of zinc, and mercury is easily expressed. By continuing the operation, more mercury, to the amount in all of nearly three-fourths, is recovered.

As an illustration of the economic importance of this material in applied science, I am informed that the silver plates of the batteries constructed for the Atlantic Telegraph cost £2520 or more. On my having directed the attention of the Company to graphite as a substitute for silver, a set of plates were ordered, equal in number and size, which were supplied (furnished with electrotype copper and connecting wires) for £216.

The following Table illustrates the effective working powers of platinized graphite, as compared, under like circumstances, with platinized silver, given in lifting powers in pounds, A third column is added, giving the results when table salt is dissolved in the water employed with the graphites.

Table I.

Electro-magnet; 10 yards No. 16 wire.

12 cells in series.				12 cells in double series of 2 sixes.			
Resistance.	Silver.	Graphite.	Graphite.	Resistance.	Silver.	Graphite.	Graphite.
yds.	lbs.	lbs.	lbs.	yds.	lbs.	lbs.	lbs.
10	14·75	14	15	10	22·5	20·5	20
147	10	12·5	9	147	14	10	9
284	7	9	8	284	8·25	7	7
421	6	7	6	421	5	5	4
558	5	5	4	558	3·25	3	2·5
695	4·5	3	3	695	2	2·25	1·5
832	3	2·5	2·5	832	2	1·5	1·25
6 cells in series.				6 cells in double series of 2 threes.			
10		12·25	10	10	14	14	14
147	9	6·25	7	147	4	4	5
284	5	4	4	284	2·5	2·25	2·5
421	3·5	3	2·5	421	2	1·75	2
558	2·25	2	2	558	1·5	1·25	1
695	2	1·5	1	695	1	1	1
832	1·5	1·25	·75	832	·75	·75	·75

Table II.

Electro-magnet; 137 yards No. 16 wire.

12 cells in series.

Resistance.	Silver.	Graphite.	Graphite.
yds.	lbs.	lbs.	lbs.
137×2	14	18	22·5
137×3	12·75	15·75	14
137×4	10	13	11
137×5	9	12·5	11
137×6	9	10·75	11
137×7	9·5	9·5	9
137×8	8·75	9·5	8·75
	6 cells in series.		
137×2	9·75	12·75	11
137×3	8	10·75	10
137×4	7·25	10	9·5
137×5	7·75	9	9
137×6	7	8	9
137×7	6·75	9	8·75
137×8	7	8·75	8
	6 cells in double series of 2 threes.		
137×2	8·75	10	11
137×3	7·25	9	9
137×4	6	9	9
137×5	7·75	8	7
137×6	4·25	6	5
137×7	4	6	4·75
137×8	4·25	5	6

In all the above experiments the cells were charged with 1 sulphuric acid + 13 water (salt-water in the third column); and 13·5 square inches of surface were immersed. The silver-zinc pairs were 1 inch apart, the graphite-zinc, 2 inches. The lifting powers were not read off more closely than to quarter-pounds. The electro-magnet used in Table I. was a small horse-shoe containing about 10 yards of No. 16 wire; that used in Table II. was one of the electro-magnets used in the construction of the signal bells before described (*vide* Proc. Roy. Soc., vol. viii. p. 419), and containing 274 yards of No. 16 copper wire. The resistance added in each successive experiment was one bobbin of a similar electro-magnet or 137 yards of wire. The resistances in the Table include the resistance of the electro-magnet. The total resistances in Table II. are all multiples

of the contents of a single bobbin or 137 yards. A glance from left to right on the same horizontal line shows the comparative value of each combination in the several experiments. One or two small irregularities in Table II. in the six-cell results, are doubtless due to the poles of the magnet not having been ground true.

With respect to durability, the graphite plates in use since 1850 are in as good condition as the new ones now in course of manufacture. Silver plates employed by us under like circumstances, commenced perishing after twelve months or more of use; they crumble away in great measure, they cut apart at the surface level, and they get eaten into holes throughout.

II. "On the Aquiferous and Oviductal Systems in the Lamellibranchiate Mollusks." By George Rolleston, M.D., Lee's Reader in Anatomy, and Charles Robertson, Esq., Curator of the Museum, Christ Church, Oxford. Communicated by Dr. Acland. Received January 6, 1859.

(Abstract.)

In this paper the authors bring forward two views as to the anatomy of the Lamellibranchiata.

1. The first part of the communication is devoted to an examination of the commonly-received opinion as to the outlet of the ovarian system, and arguments are brought forward to show that the orifices usually supposed to discharge this office are in reality the exhalant orifices of a water-vascular system. The positive arguments drawn from the way in which fine injections thrown in by these orifices distribute themselves throughout the visceral mass, and from the relative position of orifices acknowledged to belong to a water-vascular system in other mollusks, are confirmed by a consideration of the improbability attaching to the old view, which regarded as oviducts in mollusca two canals, which lying one on either side of the body, yet communicate freely with each other at no great distance from their termination, and which lie far away from the lower segment of the intestinal tube. The inhalant aquiferous orifices are considered to be indicated by a belt of parasitic animals impacted in the foot tissue, as represented in one of the figures.

2. In the second part of the communication, the structures are indicated which the authors hold to be the true oviducts. One large band which is seen at the spawning season as a prominent ridge projecting into the calibre of the lower segment of the intestinal tube, and two smaller ones, which are traceable from the commencement of the intestine down to a point where its upper coils are in close proximity to that part of its lower segment where the former band ends in a club-shaped dilatation, are shown to discharge this function. The method of dissection to be adopted for the demonstration of these structures is given at some length, and the following arguments are adduced in support of the view which regards them as oviducts. A fine injection thrown into the largest of the bands in question is seen to pass into the ovary, and is recognizable under the microscope as contained within the limitary membrane of its ultimate follicles. Its distribution, therefore, as detectable at once by the naked eye and by the microscope, contrasts strongly with that of a similar injection thrown in by either of the aquiferous orifices. *Secondly*: The condition of distention, prominence, and intumescence of this band, coincides with similar conditions in the ovary; and from an acquaintance with the condition of the branchial marsupium's contents we are enabled always to predict what will be found to be that of this band. *Thirdly*: At periods when ova are being rapidly secreted by the ovary, ova are to be found at all points within the whole length of these three bands. The double oviduct at the oral and the single at the anal extremity of the Lamellibranchiata, is what our knowledge of their development would lead us to anticipate; and the close connexion of the principal oviduct with that latter outlet, and with the lower segment of the intestinal tube, brings the anatomy of these bivalve mollusks into exact correspondence with that of higher tribes in the same series.

What is said of the ovarian secretion and outlets, applies, *mutatis mutandis*, to the testicular.

The paper is illustrated by drawings taken from dissections of the common fresh-water muscle, *Anodonta Cygnea*.

III. "On the Action of Nitric Acid and of Binoxide of Manganese and Sulphuric Acid on the Organic Bases." By A. MATTHIESSEN, Ph.D. Communicated by Prof. STOKES, Sec. R.S. Received February 3, 1859.

In the Proceedings of the Royal Society (vol. ix. p. 118), I stated that by the action of nitrous acid on aniline I had obtained ammonia and nitrophenasic acid; since then I have acted on several other of the organic bases with the same reagent, as well as with nitric acid, and with binoxide of manganese and sulphuric acid; and I will now shortly enumerate the experiments.

1. *Action of Nitrous Acid on Amylaniline.*

The dilute solution of the nitrate of amylaniline was acted on by nitrous acid at 100° C. for 12 hours. Amylaniline and ammonia were obtained, but in quantities too small to be quantitatively determined.

2. *Action of Nitric Acid on Amylaniline.*

Amylaniline was boiled with dilute nitric acid (1 part acid to 2 of water) until the reaction began, which was immediately stopped by adding cold water to the solution. This was filtered when cold from the nitrophenasic acid, and after potash had been added, it was again filtered to separate any undecomposed amylaniline. The filtrate was distilled, the distillate redistilled *per ascensum* into hydrochloric acid, and the acid solution evaporated to dryness. The residue was then extracted with absolute alcohol, and the filtrate evaporated to dryness. This operation was repeated four or five times. A platinum-salt* made with the chloride, which was soluble in absolute alcohol, gave 33·55 per cent. platinum. The chloroplatinate of amylamine requires 33·66 per cent. The platinum-salt of that chloride which was insoluble in absolute alcohol gave 43·9 per cent. platinum. The chloroplatinate of ammonia requires 44·2 per cent.

The above reaction may be explained as follows:—

$$\left.\begin{matrix} C_{12}\,H_5 \\ C_{10}\,H_{11} \\ H \end{matrix}\right\} N + H_2\,O_2 + NO_5\,HO = \left.\begin{matrix} C_{10}\,H_{11} \\ H \\ H \end{matrix}\right\} N + C_{12}\,H_6\,O_2 + NO_5\,HO$$

* All the platinum-salts determined were recrystallized in water.

and

$$\left.\begin{matrix} C_{10}H_{11} \\ H \\ H \end{matrix}\right\} N + H_2O_2 + NO_5\,HO = \left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N + C_{10}H_{12}O_2 + NO_5\,HO.$$

The free nitric acid present converts the phenylic alcohol into nitrophenasic acid, and the amylic alcohol into nitrite of amyl.

3. *Action of Nitric Acid on Ethylaniline.*

Ethylaniline was treated in the same manner as amylaniline. The platinum-salt of the chloride which was soluble in absolute alcohol gave in two experiments 39·6 and 39·5 per cent. platinum. The percentage of platinum in chloroplatinate of ethylamine is 39·3.

The platinum-salt of the chloride which was insoluble in absolute alcohol gave 44 per cent. platinum, which agrees with the number required for the chloroplatinate of ammonia.

4. *Action of Nitric Acid on Diethylaniline.*

Diethylaniline was treated with dilute nitric acid (1 part of acid to 4 of water), and heated till the temperature reached 54° C., when, on being left to itself, it soon became turbid, and the temperature rose about 10° C. After a while the solution cleared itself again, but remained very dark. When quite cold, it was filtered from the nitrophenasic acid, and the filtrate was treated in the same manner as described in Experiment 2. The solution of the chlorides (5–6 grms. were obtained from about 50 of diethylaniline), which were soluble in absolute alcohol, was partially precipitated with bichloride of platinum (precipitate No. 1); then another portion was precipitated, which was not used; after this, more bichloride was added (precipitate No. 2); and lastly, an excess of bichloride was added (precipitate No. 3). The platinum found in the three precipitates was

No. 1.	No. 2.	No. 3.
39·2 per cent.	35·5 per cent.	35·4 per cent.

The chloroplatinate of ethylamine contains 39·3 per cent., and of diethylamine 35·3 per cent. platinum. A platinum-salt made with chloride insoluble in absolute alcohol, gave 44·2 per cent. platinum, which is exactly the amount contained in the chloroplatinate of ammonia.

The reaction was as follows:—

$$\left.\begin{matrix}C_{12}H_5\\ C_4H_5\\ C_4H_5\end{matrix}\right\}N+H_2O_2+NO_5HO=\left.\begin{matrix}C_4H_5\\ C_4H_5\\ H\end{matrix}\right\}N+C_{12}H_6O_2+NO_5HO;$$

$$\left.\begin{matrix}C_4H_5\\ C_4H_5\\ H\end{matrix}\right\}N+H_2O_2+NO_5HO=\left.\begin{matrix}C_4H_5\\ H\\ H\end{matrix}\right\}N+C_4H_6O_2+NO_5HO;$$

and

$$\left.\begin{matrix}C_4H_5\\ H\\ H\end{matrix}\right\}N+H_2O_2+NO_5HO=\left.\begin{matrix}H\\ H\\ H\end{matrix}\right\}N+C_4H_6O_2+NO_5HO.$$

As in the case of amylaniline, the phenylic alcohol and the alcohol are converted into nitrophenasic acid and nitrite of ethyl; the ammonia being in both cases partially oxidized.

5. *Action of Binoxide of Manganese and Sulphuric Acid on Aniline.*

Aniline was dissolved in an excess of dilute sulphuric acid (1 part of acid to 6 of water) and heated to boiling, when a small quantity of binoxide of manganese was added. The reaction was allowed to continue for 3 or 4 minutes, and it was stopped by cooling the flask in water. Potash was then added, and the solution filtered. The filtrate was distilled as described in experiment 2, and the distillate evaporated to dryness with hydrochloric acid, and extracted with absolute alcohol, to dissolve any chloride of aniline present. A platinum-salt made from the chloride, insoluble in absolute alcohol, gave 44·0 per cent. platinum, which corresponds to the platinum in the chloroplatinate of ammonia.

6. *Action of Binoxide of Manganese and Sulphuric Acid on Diethylaniline.*

Diethylaniline was treated in the same manner as aniline, but only for about one minute. Potash was added, &c., as in the foregoing experiment. The platinum-salt partially precipitated, as in Experiment 4, from the chloride soluble in absolute alcohol, gave:—

Precipitate No. 1.	Precipitate No. 2.
37·6 per cent.	35·4 per cent. platinum.

No. 1 appears to be only a mixture of the chloroplatinates of ethylamine and diethylamine; the quantity of salt first precipitated being too small to be properly recrystallized (on account of former experiments showing that the quantity of ethylamine present was very small). No. 2 corresponds with diethylamine, and of this salt

there was a large quantity, so that it was recrystallized twice. A platinum-salt made from the chloride insoluble in absolute alcohol (of which there was only a very small quantity), gave 44·3 per cent. platinum, which is almost the same as the amount in chloroplatinate of ammonia. No phenylic alcohol was found nor any of its compounds; and according to an experiment of Long (not yet published), on the oxidation of phenylic alcohol, that chemist always, excepting when he used spongy platinum, obtained a resinous mass.

From the above experiments, it appears that by the action of nitrous acid, nitric acid, binoxide of manganese and sulphuric acid, permanganate of potash*, potash†, and in some cases by the presence of acids alone (as sulphuric or hydrochloric)‡, on the organic bases in the presence of water, water only is decomposed in the first stage of the reaction; and the fact that the radicals contained in the bases are replaced by hydrogen by degrees, makes it plausible that by these means we may be able to determine the constitution of the natural organic bases.

I am now experimenting with narcotine, and to all appearance, I shall succeed in determining its constitution.

In conclusion, I may here be allowed to thank Dr. Holzmann for his assistance in carrying out the above experiments.

February 10, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communication was read:—

"Experiments on the Action of Food upon the Respiration." By Edward Smith, M.D., LL.B., L.R.C.P., Assistant-Physician to the Hospital for Consumption, Brompton. Communicated by Sir Benjamin C. Brodie, Bart. Received January 6, 1859.

(Abstract.)

The author had proved in his former Paper that the maximum influence of food is observed within two and a half hours after its

* By its action on aniline, ammonia is obtained. † In its action on the amides.
‡ In the case of asparagine, benzamide, &c.

exhibition; also that the action of food is in two degrees; viz. that which sustains the respiratory changes to the minimum line (or that which occurs with complete abstinence), and that which is observed as the maximum point to which the respiratory function is increased after ordinary meals. His aim in this communication was to show the variations in the influence of food between these two lines. His method of inquiry was to take a moderate quantity of a single article of food alone, before breakfast, whilst the body was at rest and in the sitting posture, and to determine the influence every ten or fifteen minutes during a period of about two hours. He noted the amount of carbonic acid exhaled and of air inhaled, with the rate of respiration and pulsation, and also the temperature and the barometric pressure of the atmosphere. The apparatus employed was that described in his former Paper, and the gentlemen who submitted themselves to the investigation were chiefly the author and Mr. Moul, with Professor Frankland, F.R.S., Mr. Hoffman, and Mr. Reid, who engaged in a few experiments.

The following foods were subjected to inquiry:—

1. *The starch series*, viz. arrowroot, arrowroot and butter, arrowroot and sugar, commercial starch, wheat starch, gluten, bread, oatmeal, rice, rice and butter, potato.

2. *The fat series*, viz. butter, olive oil, cod-liver oil.

3. *Sugars*, viz. cane sugar, cane sugar and butter, cane sugar with acids and alkalies, grape sugar, sugar of milk.

4. *The milk series* (cows' milk), viz. new milk, skimmed milk, casein, casein and lactic acid, lactic acid, sugar of milk and lactic acid, cream.

5. *Alcohols*, viz. alcohol, brandy, whisky, gin, rum, sherry wine, port wine, stout, and ale.

6. *The tea series*, viz. tea, green and black, hot and cold, in various quantities, and with acids and alkalies; coffee, coffee-leaves, chichory, and cocoa.

7. *Some other nitrogenous substances*, viz. gelatin, albumen, fibrine, almond-emulsion.

The author found that pure starch scarcely increased the amount of carbonic acid evolved, but the combination of starch with gluten and sugar in the cereals caused an increase of about 2 grains per

minute. Wheat flour, oatmeal, and rice had similar effects, but potato had a less enduring influence.

Fats lessened the amount of carbonic acid evolved, and when taken with starch, the cereals, or sugar, somewhat lessened their power to produce carbonic acid. Fats increased pulsation.

Sugars increased the carbonic acid evolved to the maximum extent of from $1\frac{1}{2}$ to $2\frac{1}{2}$ grains per minute in about half an hour. Cane sugar was more powerful than milk sugar, and still more so than grape sugar. Acids increased the maximum influence of sugar.

Milk increased both the pulsation and the carbonic acid, and the latter to a maximum of nearly 2 grains per minute. All the component elements except lactic acid had a similar influence, but new milk was much more powerful than any of its elements separately, or than any artificial combination of its elements. The effect of milk differed in degree, and of casein in direction, upon the author and Mr. Moul.

Tea and coffee increased the production of carbonic acid to the extent of from $1\frac{1}{2}$ to 3 grains per minute. Tea was more powerful than coffee, and coffee than chichory. Cocoa was as powerful as coffee. Coffee-leaves lessened the amount of carbonic acid. Acids added to tea rendered it more stimulating, and alkalies made it more soothing.

Alcohols differed in their effect, according both to different kinds and samples of the same kind. Spirits of wine always increased the quantity of carbonic acid evolved to a maximum of less than 1 grain per minute. Rum commonly increased it, and sometimes to $1\frac{1}{2}$ grain per minute. Ale and stout increased it to upwards of 1 grain per minute. Sherry wine (3 oz.) commonly slightly increased it. Brandy and gin, and particularly the latter, always decreased it. Whisky varied in its effects. The inhalation of the volatile elements of wine and spirits, and particularly of fine old port wine, lessened the quantity of carbonic acid, and increased the amount of vapour exhaled.

Gluten, casein, gelatin, albumen, and fibrin increased the amount of carbonic acid exhaled, the two former to about 1 grain per minute, and the last to about $\frac{1}{2}$ grain per minute. Almond-emulsion did not increase it.

From these facts the author infers that there is a class of foods which might be called "excito-respiratory;" a class which embraces

nearly all nitrogenous foods, and is almost entirely composed of these substances.

The *non-excitants* are starch, fat, some alcohols, and coffee-leaves.

The *respiratory excitants* are sugar, milk, the cereals, potato, tea, coffee, chichory, cocoa, alcohol, rum, ales, some wines, gluten, casein, gelatin, fibrin, and albumen.

Of the hydrocarbons, sugar acted very differently from starch and fat.

All the "respiratory excitants" increased the *depth*, but not the *rate* of respiration.

Some of them acted with great rapidity; as, for example, sugar and tea, which sometimes caused an increase of 1 grain of carbonic acid per minute in from five to eight minutes. Others, as gluten and casein, acted with less rapidity. In many, as tea and gluten, there was not a proportionate increase in the carbonic acid with increase in the quantity of the "excitant." Some of them, as tea, produced much greater effect when a small dose was frequently repeated, than when the whole quantity was given at once, and caused a much greater evolution of carbon than they supplied. The duration of the increase was very different with different foods, but that with sugar was the least, and then that with tea, while that with the cereals and rum and milk was the greatest. The amount of carbonic acid progressively increased at each examination until the maximum was attained; after which it remained nearly stationary for some time, as with the cereals, or subsided rapidly to the basis quantity, as with sugar.

The paper was illustrated by a series of diagrams, and accompanied by tables.

February 17, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The Lord Bishop of London and the Lord Bishop of Ripon were admitted into the Society.

The following communications were read:—

I. "Statement of Facts relating to the Discovery of the Composition of Water by the Hon. H. CAVENDISH." In a Letter from J. J. BENNETT, Esq., F.R.S., to Sir B. C. BRODIE, Bart., P.R.S., dated February 12, 1859. Received February 14, 1859.

Since the death of our late excellent and lamented friend Mr. Robert Brown, several appeals have been made to his executors to publish certain evidence presumed to have been in his possession relating to the much-agitated question of the priority of Cavendish or Watt in the discovery of the composition of water. As the executor to whom Mr. Brown entrusted his papers, and having been for many years honoured with his entire confidence, I feel called upon to respond to these appeals, and I therefore request that you will kindly lay before a Meeting of the Royal Society the following brief statement on the subject.

The date and nature of Cavendish's communication to Priestley have always been considered as essential elements in the determination of the question; and it was the evidence which Mr. Brown possessed in regard to these particulars, which, in his estimation, "placed Cavendish's claims as the discoverer of the composition of water beyond dispute." That evidence, however, was not derived from any unpublished document, but formed part of a section of Deluc's "Idées sur la Météorologie," which although especially entitled,—"*Anecdotes relatives à la découverte de l'Eau sous la forme d'Air,*"—appears entirely to have escaped the notice of those

who have advocated Cavendish's claims. It is the more conclusive as coming from Deluc, the "*ami zélé,*" as he justly terms himself, of Watt, and who, in relation to this question, believed himself "à portée d'en connoître toutes les circonstances."

The testimony of Deluc is as follows:—

Vers la fin de l'année 1782 j'allai à *Birmingham*, où le Dr. Priestley s'étoit établi depuis quelques années. Il me communiqua alors, que M. Cavendish, d'après une remarque de M. Warltire; qui avoit toujours trouvé de *l'eau* dans les vases où il avoit brûlé un mélange *d'air inflammable* et *d'air atmosphérique*; s'étoit appliqué à découvrir la source de cette eau, et qu'il avoit trouvé, "qu'un mélange *d'air inflammable* et *d'air déphlogistiqué* en proportion convenable, étant allumé par l'étincelle électrique, se convertissoit tout entier en *eau*." Je fus frappé au plus haut degré de cette découverte*.

The italics and inverted commas are Deluc's own.

In this communication made by Cavendish to Priestley the theory of the composition of water is clearly indicated. The two gases (known to have been hydrogen and oxygen) were mixed together *in due proportion*, and by means of the electric spark were *entirely converted* into water. Referring to one of Cavendish's experiments, as recorded in his journal, Lord Jeffrey, the most candid and judicious of Watt's advocates, has said: "if he [Cavendish] had even stated in the detail of it, that the airs were *converted*, or *changed*, or *turned* into water, it would probably have been enough to have secured to him the credit of this discovery, as well as to have given the scientific world the benefit of it, in the event of his death, before he could prevail on his modesty to claim it in public.†" The evidence which this distinguished critic and judge regarded as sufficient to establish Cavendish's claim is now afforded, not by a note in his private journal, but by the testimony of the zealous friend of Watt, who states that it was communicated to Priestley towards the end of the year 1782, that is to say, several months before Watt drew his own conclusions from Priestley's bungling repetition of Cavendish's experiments. It was, moreover, published to the world, and suffered to remain uncontradicted, while

* Idées sur la Météorologie, tome ii. 1787, pp. 206–7.

† Edinburgh Review, vol. lxxxvii. p. 125.

all the parties were alive and in frequent intercourse with the author and with each other.

I have only further, in Mr. Brown's name also, to do an act of justice to the memory of Lavoisier, by relieving it from the obloquy which has rested upon it from his supposed persistence in unjustly claiming priority for himself. The following extract from a Report to the Academy of Sciences on M. Seguin's experiments, dated 28th August 1790, and signed Lavoisier, Brisson, Meusnier, and Laplace, the last named being the reporter, will prove that Lavoisier was not unmindful of the appeal which had been addressed to him by Blagden some years previously, and that he distinctly resigned the priority of discovery to Cavendish :—

"M. Macquer a observé dans son Dictionnaire de Chimie que la combustion des gaz hydrogène et oxygène produit une quantité d'eau sensible ; mais il n'a pas connu toute l'importance de cette observation, qu'il se contenta de présenter, sans en tirer aucune conséquence. M. Cavendish paroît avoir remarqué le premier que l'eau produite dans cette combustion est le résultat de la combinaison des deux gaz, et qu'elle est d'un poids égal au leur. Plusieurs expériences faites en grand et d'une manière très-précise, par MM. Lavoisier, La Place, Monge, Meusnier, et par M. Lefevre de Gineau, ont confirmé cette découverte importante, sur laquelle il ne doit maintenant rester aucun doute."—*Annales de Chimie*, tome 7, pp. 258–9.

John J. Bennett.

II. "On the Influence of White Light, of the different Coloured Rays and of Darkness, on the Development, Growth, and Nutrition of Animals." By Horace Dobell, M.D., Licentiate of the Royal College of Physicians, &c. &c. Communicated by James Paget, Esq. Received January 10, 1859.

(Abstract.)

In this communication the author laid before the Society the particulars of a series of experiments, having for their object to discover what influence is exerted by ordinary light, by the different coloured rays, and by darkness on the development, growth, and nutrition of animals.

After referring to the experiments of Edwards, Higginbottom, E. Forbes, Morren, Wöhler, Hannon, Moleschott, and Béclard, the results of which were shown to be somewhat contradictory, the author described the precautions taken by himself to avoid sources of fallacy.

The original experiments detailed in this Paper were conducted in the years 1855, 1856, 1857, 1858. The subjects selected were the Ova and Larvæ of the Silkworm (*Bombyx mori*) and of the Frog (*Rana temporaria*). A comparative experiment in the vegetable kingdom was also made on the Sweet Pea (*Lathyrus odoratus*). An apparatus contrived for the experiments on Tadpoles was described and figured; it secured the following desiderata:—

1. That each of six compartments or cells should be supplied with water from the same source, at the same time, subject to the same changes, and capable of being refreshed without interfering with the cells.

2. That each of the cells should be placed in the same condition with respect to the supply of air and of food.

3. That during exposure for examination of the animals, the whole series should be opened the same length of time and to the same extent.

4. That each cell should receive no light but that transmitted by its proper cover.

One of these six cells was open to the air and to light; one was covered with ordinary white glass; one was made completely dark by a covering of blackened opaque glass; one was covered with blue, one with greenish yellow, and one with red glass. The transmitting and absorbing powers of these glasses were detailed from experiments made upon them by Mr. Cornelius Hanbury, jun., and by the author.

The apparatus used for the Silkworms was, in all essential particulars, the same as that for the Tadpoles, only without water.

A tabulated analysis of the daily journal kept during the experiments was given, and its separate items compared and discussed; after which the author concluded his Paper with the following *résumé*.

"If we may venture to reason on so small a number of observations, so far as the results of these experiments are concerned, the following propositions may be advanced.

"*All other conditions being the same,* (1.) *The Ova of Insects* are not directly influenced in their development by white light, by the different coloured rays, or by darkness.

"(2.) *The Larvæ of Insects* are not directly influenced in their development, growth, nutrition, or metamorphoses by white light, by the different coloured rays, or by darkness.

"(3.) *The Larvæ of Batrachia Reptiles* are not directly influenced in their development, growth, nutrition, or metamorphoses by white light, by the different coloured rays, or by darkness.

"(4.) *The Materials necessary to the Colour of Insects and Reptiles* are prepared equally under the influence of white light, of the different coloured rays, and of darkness."

These results are so opposed to preconceived ideas upon the subject to which they relate, that they cannot fail to excite some surprise and incredulity; when, however, they are carefully considered, they assume a theoretical probability, which assists us in believing that the practical results are without fallacy.

(*a.*) With regard to the *development of the ovum,* when we consider the unity of plan which appears to preside over the germs of the simplest and of the most complicated forms, and the infinite variety of external conditions in which these germs are placed throughout the animal kingdom, we are led to the conclusion that their development must be so arranged as to be independent of the direct influence of light.

(*b.*) That *after emerging from the ovum* the animal is not directly influenced by light, is more difficult, at first, to believe, because experience seems to have taught us that "to live without light is to live without health;" but this familiar fact may be at once disposed of in the argument and explained by its coincident, that, under ordinary circumstances, the admission of light is inseparably connected with,

1. The regulation of external temperature.
2. The free circulation of a respiratory medium.
3. Those processes of vegetable life and of inorganic change upon which the proper condition of the respiratory medium depends.

Speaking generally then, it must be admitted that light is essential to the development, growth, and nutrition of animals, but only *in-*

directly. In the foregoing experiments, the usual coincidents of light, a proper supply of food, a due aëration of the respiratory medium, a properly regulated external temperature, &c. having been provided in each cell, the direct influence of light only being changed, no corresponding change occurred in the animal life.

In the vegetable kingdom the case is quite different, and the experiments on *Lathyrus odoratus* recorded in this paper demonstrate again, what has been shown by numerous other experimenters, that *light as a direct agent* is essential to the nutritive processes of plants. An interesting exception occurs, however, in the vegetable kingdom, which serves to strengthen the probability that the conclusions arrived at concerning animals are correct, viz. that fungi—which derive their nutriment, like animals, from organic compounds already prepared for them—perform their vital functions without dependence on the influence of light.

Under the head of colour, it would seem that the familiar phenomenon of *etiolation* witnessed in plants which have been deprived of light, has led to erroneous anticipations as to the effect which alterations of light would produce upon the development of the colouring materials in animals.

In the experiments here recorded, it is seen that neither white light, nor the different coloured rays, nor darkness altered the development of those materials, necessary to the exhibition of colour, when the animal was seen in ordinary light. The experiments of Dr. Gladstone, on plants, also show that the development of colouring matter in the petals of flowers is independent of the influence of light; that flowers raised under the different coloured rays and in darkness have the same colour in their petals as when raised in ordinary light. Thus, even in vegetables, *etiolation* is confined to those parts of the plant which depend for their colour upon the condition of the chlorophyl, to the green appearance of which some portion of the solar beam is evidently essential.

Although, therefore, at first sight, the results of my experiments under the head of colour may appear questionable, I think we must rather throw the question upon the correctness of our preconceived notions on the subject; and the facts elicited by Prof. Edward Forbes (referred to in the Paper), while retaining all their value and interest as assistants in determining the depths of primeval seas,

cannot, I think, be taken as evidence against the correctness of my observations. On the other hand, the results of my experiments may be found to put a new construction upon the facts observed by Prof. Forbes. He discovered that increased depth of sea corresponds with diminished light, and that both of these conditions again correspond with peculiar changes in colour, and ultimately with loss of colour in the shells inhabiting these depths; but there is no evidence to show that these colourless shells have developed any materials capable of manifesting colour after exposure to the influence of light; whereas my own and other experiments show that the *etiolated* stalks and leaves of plants speedily manifest the characteristic colour of the chlorophyl if placed in the sun's rays.

So far, therefore, as our present knowledge on the subject justifies any conclusion, the varieties of colour and the absence of colour in the mollusks are physiologically separated from the phenomena of etiolation in plants, and may be placed in the same category as the varieties of colour and the absence of colour in the corollas of flowers, which depend upon the development of materials having certain optical properties.

The beautiful facts observed by Prof. Forbes, instead of being regarded as the consequence of imperfect exposure to light, must, I think, take rank with the phenomena of coloration observed throughout the animal kingdom, such as the peculiar markings of reptiles, birds, and wild animals, according to their different habitats and modes of life; the colours of the upper and lower surfaces of fish, and the like, which cannot be shown to depend upon the exposure or non-exposure to light with which they frequently, but not always, coincide. These facts appear only to form a part of the vast and perfect plan of creation, in which everything that exists is suited in every particular to the conditions of its existence; thus, those mollusks which are designed to inhabit depths scarcely permeable to light, can have no need, and hence have no provision, for elements, to the manifestation of which light is an essential condition.

III. "On the Intensification of Sound through Solid Bodies by the interposition of Water between them and the distal extremities of Hearing-Tubes." By S. SCOTT ALISON, M.D., Assistant-Physician to the Hospital for Consumption. Communicated by Dr. TYNDALL. Received January 20, 1859.

(Abstract.)

In this Paper the author gives an account of various experiments which he has recently made on sounds proceeding through solid bodies. He has found that sounds which are faint, when heard by a hearing-tube applied directly to solid sounding bodies, become augmented when water is interposed between these bodies and the distal extremity of the hearing-tube. He has been able, by the employment of water, to hear the sound of a solid body, such as a table, which, without this medium, has been inaudible. Experiments have been made upon water in various amounts and in different conditions. Thus a very thin layer, a mere ring round the edge of the hearing-tube, masses of water in larger or smaller vessels, and a bag of water, have been employed. The results have been the same as regards augmentation. The degree of augmentation was greatest when the hearing-tube was immersed freely in water. In experimenting upon water in vessels, it was found necessary to close the extremity of the tube to be immersed, by tying over it a piece of bladder or thin india-rubber; for the entrance of water into the interior interfered greatly with the augmentation.

The effect of water in augmenting sound is materially reduced if even a small amount of solid material be interposed between the water employed and the mouth of the hearing-tube. A piece of wood, not much thicker than a paper-cutter, materially interferes with the augmenting power of water.

The augmentation of sound thus obtained by water seems to be due to the complete fitting of the liquid on the solid body and also round the mouth of the hearing-tube, whereby the column of air is thoroughly enclosed; also to the less impediment to the vibrations of the instrument when held in contact with water, than when held

in contact with a solid body, the water yielding in a greater degree than a solid.

The mode of judging of the augmentation was twofold: 1st, one sensation was compared with another perceived by the same ear, the one sensation following immediately upon the other; 2nd, the differential stethophone was employed, by which two impressions are simultaneously made upon the two ears; in which case, if one impression be materially greater than the other, sound is perceived in that ear only on which the greater impression is made. To obtain the advantage of the differential stethophone,—or "Phonoscope," as it might here perhaps be more correctly designated—when sounds at some distance from the ear were being examined, its length was increased by the addition of long tubes of india-rubber.

Experiments were made upon other liquids besides water, such as mercury and ether.

Other materials besides liquids were found to afford a similar intensification of sound from solid bodies, such as laminæ of gutta-percha, or of india-rubber, and sheets of writing paper, but the amount of augmentation was less.

The hearing-tubes employed were various. Many of the experiments were performed with the author's ordinary differential stethophone, an instrument described in No. 31 of the 'Proceedings of the Royal Society.' India-rubber tubes fitted with ivory ear-knobs, and with wooden or glass cups (the size of the cup or object-extremity of ordinary stethoscopes), and having an ear-extremity to pass into the meatus, and brass tubes, were also in turn employed. Tubes closed at their distal extremity with solid material, such as glass, did not answer so well as those closed with membrane.

The water-bag increases the impression conveyed to the ear by the wooden stethoscope, if it be placed between the flat ear-piece and the external ear. It may be employed alone to reinforce sound. The name of Hydrophone has been given to it.

A postscript is added, in which the author records an experiment made on the bank of the Serpentine river. A sound produced upon the land was heard at a point in the water when it could not be heard at an equal distance on the ground, if the two limbs of the differential stethophone were employed simultaneously.

The sensation upon the ear, connected, by means of a hollow tube,

with water in sonorous undulations, was found to be much greater than that upon the ear connected with the same water by means of a solid rod. When both tube and solid rod were employed simultaneously, sound was heard in that ear only supplied with the tube

February 24, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "Researches on the Phosphorus-Bases."—No. V. Diphosphonium-Compounds. By A. W. HOFMANN, LL.D., F.R.S., &c. Received January 20, 1859.

In a note* on the deportment of dibromide of ethylene with triethylphosphine, I have stated that the reaction between these two substances gives rise to the production of

Bromide of triethylbromethylene-phosphonium $\Big\} \; [(C_4H_5)_3(C_4H_4Br)P]Br$,

whilst two other bromides, viz.

Bromide of triethylphosphonium $[(C_4H_5)_3HP]Br$ and

Bromide of triethylvinyl-phosphonium $\Big\} \; [(C_4H_5)_3(C_4H_3)P]Br$,

are generated in consequence of secondary processes. But I did not fail to remark in the same note, that in addition there is formed in this reaction a fourth bromide, the nature of which, at that time, I had been unable to fix by experiment.

I have continued the study of this substance, which has led to the following results.

All attempts to eliminate the bromide in question by frequently recrystallizing the direct product of the action of dibromide of ethylene on triethylphosphine have entirely failed. Considerable sacrifice of precious material and often repeated analyses of the different crops of crystallization taught me nothing, except that the body

* Proceedings, vol. ix. p. 287.

which I endeavoured to grasp is most abundantly produced when the triethylphosphine is rather in excess. Indeed, it would appear, that under those conditions, the bromide in question constitutes the principal product of the reaction.

Not more successful was an attempt to increase the chances of separation by reducing the number of the bromides.

As I have previously stated, treatment with oxide of silver destroys the triethylated-bromethylene-phosphonium, converting it into a basic compound, which contains no longer any bromine, whilst the same agent transforms the bromide of triethylphosphonium into the oxide of argento-triethylphosphonium, and the dioxide of triethylphosphine. On saturating again by hydrobromic acid the liquid thus produced, the solution now contained only the new bromide, the bromide of the debrominetted body, and the dibromide of triethylphosphine, the extreme solubility of which rendered its presence almost harmless. The task was thus virtually reduced to the separation of two bromides. Unfortunately, the two substances resemble each other to such an extent, that this hope also had to be abandoned.

A modification, however, of this process led to the solution of the difficulty. On saturating the alkaline solution, produced by the action of oxide of silver upon the crude bromides, with hydriodic instead of hydrobromic acid, a mixture of the corresponding iodides was obtained, the separation of which could be easily accomplished.

On moderately concentrating this solution, a beautiful iodide of limited solubility was deposited. This substance readily dissolved in boiling water, from which it crystallized on cooling in long white needles. It was less soluble in alcohol, insoluble in ether.

The analysis of this compound, carefully purified by repeated crystallizations, led to the following atomic expression:—

$$C_{14}H_{17}PI.$$

This formula received ample confirmation by the examination of a platinum- and gold-compound. Converted into chloride and precipitated by dichloride of platinum, the new body furnished a crystalline, difficultly-soluble platinum-salt, differing from the platinum-salts of all the other compounds of this group. This salt dissolves in boiling concentrated hydrochloric acid without decomposition, and crystallizes on cooling in beautiful yellow needles containing

$$C_{14}H_{17}PCl, PtCl_2.$$

The gold-salt is a bright yellow crystalline precipitate, difficultly soluble in boiling water, and not recrystallizable without some alteration. The gold-determination agreed with the formula

$$C_{14}H_{17}PCl, AuCl_3.$$

The preceding formulæ are simple translations of the analytical results, but they convey no idea regarding the nature of the new body. Legitimate interpretation of these expressions, and a due appreciation of the conditions in which the new compounds are formed, unavoidably lead us to the conclusion that the formulæ must be doubled. The molecule of the new iodide thus becomes

$$C_{28}H_{34}P_2I_2,$$

corresponding to an original bromide,

$$C_{28}H_{34}P_2Br_2,$$

which is simply formed by the association of 2 equivalents of triethylphosphine and 1 equivalent of dibromide of ethylene,

$$2C_{12}H_{15}P + C_4H_4Br_2 = C_{28}H_{34}P_2Br_2.$$

The formulæ of the platinum-salt and of the gold-salt of course have likewise to be doubled:

Platinum-salt	$C_{28}H_{34}P_2Cl_2, 2PtCl_2$,
Gold-salt	$C_{28}H_{34}P_2Cl_2, 2AuCl_3$;

the number of platinum- and gold-equivalents which respectively exist in these compounds being apparently determined by the number of triethylphosphine-equivalents associated in the new salt. I have vainly endeavoured to produce compounds containing only one equivalent of platinum and gold, but have succeeded in procuring a well-defined silver-compound:

$$C_{28}H_{34}P_2Br_2, AgBr,$$

which is formed by treating the new bromide with a quantity of oxide of silver insufficient for complete decomposition. This compound is a double salt of equal equivalents of the proximate constituents.

The deportment of triethylphosphine with dibromide of ethylene, and more particularly the formation of the new bromide, is not with-

out theoretical interest. The molecule of dibromide of ethylene, equivalent to 2 molecules of hydrobromic acid, fixes in this reaction 2 molecules of triethylphosphine, equivalent to 2 molecules of ammonia, the result being a compound saline molecule, equivalent to 2 molecules of sal ammoniac.

$$\underbrace{\left[\left.\begin{matrix}H_2\\H_2\\H_2\\H_2\end{matrix}\right\} N_2\right] Cl_2}_{\substack{\text{2 molecules of}\\ \text{sal ammoniac.}}} \qquad \underbrace{\left[\left.\begin{matrix}(C_4H_5)_2\\(C_4H_5)_2\\(C_4H_5)_2\\(C_4H_4)''\end{matrix}\right\} P_2\right]'' Br_2}_{\substack{\text{Molecule of the diatomic}\\ \text{bromide.}}}$$

It is not quite easy to frame a name for this complex body, in which, under the influence of the diatomic ethylene, 2 molecules of triethylphosphine are, if I may say so, *dovetailed* together. We have in this case to deal with a compound molecularly representing 2 equivalents of chloride of ammonium, with phosphorus in the place of nitrogen, bromine in the place of chlorine, 6 equivalents of ethyl and 1 equivalent of diatomic ethylene being substituted for the 8 equivalents of hydrogen; in fact, the compound is a dibromide of hexethylethylene-diphosphonium, *sit venia verbo*.

Those who have accorded some attention to the direction of these researches, cannot have failed to observe that the conception of the compound which forms the subject of this note was the point from which I started in examining the deportment of triethylphosphine with dibromide of ethylene. In a note on polyammonias, presented to the Royal Society about a year ago*, I first pointed out the existence of similar compounds in the nitrogen-series, adducing in favour of this view such experimental evidence as I was enabled to collect from the materials at hand. I have since endeavoured to expand this evidence by the realization of a variety of bodies of analogous constitution. For this purpose I have examined the action of ammonia on dibromide of ethylene; a process, which, owing to the number of bodies simultaneously produced, presents considerable difficulties. With the view of simplifying the reaction, I have passed step by step to the primary, secondary, and tertiary monamines, in which the advancing state of substitution promised a reduction of the number of compounds capable of being generated under the influence of

* Proceedings, vol. ix. p. 150.

dibromide of ethylene. These experiments, some of which have been laid already before the Royal Society, whilst others are still incomplete, have furnished many additional illustrations of the group of polyammonias; but most of these reactions are complicated, and the compounds produced are far from always presenting the salient characters which I could have desired. In fact, it was not until I pursued the inquiry into the phosphorus-series, and relying on the promptness and precision with which these substances act, examined the deportment of dibromide of ethylene with triethylphosphine, that the experiments were attended by the desired success.

The new diphosphonium-compounds which form the subject of this note are remarkable for their well-defined characters, and for their stability. They may be heated to 250° C. without undergoing the slightest change. Even the dioxide, which is readily liberated by the action of oxide of silver upon either the bromide or the iodide, is a very stable compound. The solution of this substance, which obviously corresponds to 2 molecules of water,

$$\underbrace{\left.\begin{matrix} H_2 \\ H_2 \end{matrix}\right\} O_4}_{\substack{\text{2 molecules} \\ \text{of water.}}} \qquad \underbrace{\left.\begin{matrix} [(C_4H_5)_6(C_4H_4)''P_2]'' \\ H_2 \end{matrix}\right\} O_2,}_{\substack{\text{Molecule of the diphosphonium-} \\ \text{compound.}}}$$

is a powerfully alkaline liquid, attracting with great avidity the carbonic acid of the atmosphere, and precipitating the metallic oxides like potassa. The solution may be evaporated without change to a syrup-like liquid, and it is only at a very high temperature that decomposition actually takes places. At one time I had hoped to see this body splitting under the influence of heat into the ethylene-alcohol (glycol) and triethylphosphine, but the transformation ensues in another form, only traces of phosphorus-base being liberated, while the principal product is the dioxide of triethylphosphine, which, in the latter stages of the distillation, coats the neck of the retort with a network of beautiful needles; a small quantity of gas (hydride of ethyl?) being simultaneously evolved.

The reaction is probably

$$\left.\begin{matrix} [(C_4H_5)_6(C_4H_4)''P_2]'' \\ H_2 \end{matrix}\right\} O_4 = C_4H_6 + 2[(C_4H_5)_3PO_2];$$

this equation, however, is not experimentally established.

The molecule of the diphosphonium-bromide contains the elements

of 1 molecule of bromide of triethylphosphonium and 1 molecule of triethyl-vinyl-phosphonium,

$$[(C_4H_5)_6(C_4H_4)''P_2]''Br_2=[(C_4H_5)_3HP]Br+[(C_4H_5)_3(C_4H_3)P]Br;$$

I have endeavoured to split the latter in accordance with the above equation, but without success.

Triethylphosphine acts with energy upon the homologues of dibromide of ethylene; I have not yet examined, however, any of the products thus obtained. Mr. W. Valentin, to whom I am indebted for much valuable assistance during my experiments, has found, moreover, that triethylarsine unites with dibromide of ethylene. He has not yet completed the investigation of the crystalline body which is generated in this reaction.

II. "On the Different Types in the Microscopic Structure of the Skeleton of Osseous Fishes." By A. Kölliker, Professor of Anatomy and Physiology in the University of Würzburg. Communicated by Dr. Sharpey, Sec.R.S. Received January 20, 1859.

After having been occupied for several months with observations on the minute structure of the bones of fishes, I now take the liberty to present the results of my studies to the Royal Society.

The principal fact which I have to mention is, *that a great many genera of osseous fishes possess no bone-corpuscles, radiated or fusiform, in their skeleton, and therefore no real osseous tissue.* That there exist fish-bones without bone-corpuscles must have been long known in England to those who have collections of microscopic preparations of the hard tissues of animals, as Owen, Tomes, Williamson, Quekett, and others; but nobody seems to have mentioned the fact before Williamson, Quekett, Dr. Mettenheimer of Frankfort, and myself*. In the year 1850 Professor Williamson pointed out the absence of bone-corpuscles from the bones of the Cod, Haddock, Perch, Plaice, Pike, and various other fish, distinguishing them in

* Since this communication was read to the Society, Dr. Sharpey has directed my attention to a statement by the late Professor J. Müller, to the effect that in the Pike and many other fish the bones are destitute of bone-corpuscles. This statement occurs in Müller's Annual Report of the progress of Anatomical and Physiological Science in 1835, and is repeated in his addition to the work of Miescher, "De Inflammatione Ossium, eorumque Anatome Generali," Berlin, 1836, p. 269.

this respect from the bones of the Eel, in which such corpuscles are abundant*; in 1853 I made known† that the bones of *Leptocephalus* and *Helmichthys* contain no trace of bone-corpuscles; a year later, Mettenheimer showed that the same was true of the bones of *Tetragonurus Cuvieri*‡; and in 1855 Quekett mentions, in the second volume of the 'Histological Catalogue of the College of Surgeons of England,' fishes belonging to eighteen genera, in the bones of which he had not succeeded in finding bone-corpuscles—viz. *Vogmarus islandicus, Lophius piscatorius, Gadus morrhua, Ephippus, Sparus, Trigla cuculus, Belone vulgaris, Pleuronectes platessa, Trachinus vipera, Orthagoriscus mola, Exocœtus, Scarus, Esox, Sphyræna barracuda, Tetrapturus, Zeus faber, Perca fluviatilis, Gobio fluviatilis*. But, notwithstanding these most valuable observations, little or no progress seems to have been made in the more general treatment of this matter, as is best shown by the 'Comparative Histology' of Leydig (1857), in which (p. 157) the *Leptocephalidæ, Tetragonurus*, and *Orthagoriscus* are the only cases mentioned, in which the radiated bone-corpuscles are wanting.

On commencing a series of more extended investigations into the minute structure of fish-bones, in October last, I found that the genera which possess real osseous tissue are rather scarce, whilst, on the other hand, I fell in with a great many types in which the bones contained no trace of lacunæ. And as this fact not only appeared to me of interest with regard to the development of the bones of fishes, but also promised to become of great value in systematic zoology, and in the determination of fossil remains, I devoted my whole time to this question. Now that I have investigated more than 200 species belonging to nearly all tribes of osseous fishes, and mounted about 500 microscopic preparations of their hard structures, I hope to be able to treat this question more comprehensively than has been possible hitherto, and in such a way as to lead to some general conclusions.

In giving the results of my observations, I begin with an enumeration of the fishes which belong to the one, and those which belong to the other type.

* Phil. Trans. 1851, p. 693.

† Zeitschr. f. wiss. Zool. iv. p. 361.

‡ Anat.-histol. Untersuch. ü. d. *Tetragonurus Cuvieri*, in den Abh. d. Senkenberg. Gesellschaft, i. p. 241.

I. *Fishes whose bones contain no bone-corpuscles.*

Ordo I. ACANTHOPTERI.

Fam. 1. *Percoidei.*
- Perca fluviatilis.
- Apogon Rex mullorum.
- Pomatomus telescopium.
- Lucioperca sandra.
- Serranus cabrilla.
- Anthias buphthalmus.
- Acerina vulgaris.
- Centrarchus sparoides.
- Priacanthus macrophthalmus.
- Therapon servus.
- Trachinus vipera.
- Trachinus draco.
- Uranoscopus scaber.
- Pomotis gibbosus.
- Polynemus paradiseus.
- Sphyræna spet.
- Sphyræna barracuda.
- Mullus barbatus.

Fam. 2. *Cataphracti.*
- Trigla cuculus.
- Trigla lyra.
- Prionotus carolinus.
- Platycephalus insidiator.
- Dactyloptera volitans.
- Cottus gobio.
- Aspidophorus europæus.
- Monocentris japonicus.
- Gasterosteus trachurus.

Fam. 3. *Sparoidei* incl. *Mænides.*
- Sargus annularis.
- Sargus ovis.
- Chrysophrys aurata.
- Pagrus vulgaris.
- Pagellus centrodontus.
- Boops salpa.
- Boops vulgaris.
- Dentex vulgaris.
- Smaris vulgaris.
- Smaris insidiator.
- Gerres Plumieri.

Fam. 4. *Sciænoidei.*
- Corvina nigra.
- Corvina lobata.
- Micropogon undulatus.
- Otolithus regalis.
- Hæmulon formosum.
- Pristipoma stridens.

Fam. 5. *Labyrinthiformes.*
- Anabas scandens.
- Helostoma Temminckii.
- Ophicephalus striatus.
- Trichopus trichopterus.
- Polyacanthus Hasseltii.
- Spirobranchus capensis.

Fam. 6. *Mugiloidei.*
- Mugil cephalus.
- Mugil, spec.
- Atherina Humboldtii.
- Atherina vulgaris.
- Atherina macrophthalma.

Fam. 7. *Notacanthini.*
- Mastacemblus pancalus.

Fam. 8. *Scomberoidei.*
- Scomber scomber.
- Xiphias gladius.
- Tetrapturus belone.
- Naucrates ductor.
- Lampugus pelagicus.
- Lampugus siculus.
- Seriola, spec.
- Chorinemus saltans.
- Caranx trachurus.
- Caranx carangus.
- Centrolophus pompilus,
- Lichia glauca.
- Equula insidiatrix.
- Argyreiosus vomer.
- Vomer Brownii.
- Zeus faber.
- Capros aper.
- Coryphæna hippurus.
- Astrodermus guttatus.
- Tetragonurus Cuvieri.

Fam. 9. *Squamipennes.*
- Scatophagus argus.

Holacanthus, spec.
Toxotes jaculator.
Ephippus faber.

Fam. 10. *Tænioidei.*
Lepidopus argyreus.
Trichiurus haumela.
Trachypterus tænia.
Trachypterus repandus, *Costa.*
Trachypterus Spinolæ.
Cepola rubescens.

Fam. 11. *Gobioidei* et *Cyclopteri.*
Gobius capito.
Gobius cruentatus.
Gobius longiradiatus, *Risso.*
Amblyopus Hermannianus.
Eleotris humeralis.
Tripauchen vagina.
Anarrhichas lupus.
Lepadogaster Gouani.
Echineis remora.

Fam. 12. *Blennioidei.*
Blennius gattorugine.
Blennius Montagui.
Blennius galerita.
Salarias quadricornis.
Cristiceps, spec.
Clinus argenteus.
Callionymus lacerta.

Fam. 13. *Pedunculati.*
Lophius piscatorius.
Chironectes histrio.
Malthe vespertilio.
Batrachus tau.

Fam. 14. *Theutyes.*
Naseus longicornis.
Acanthurus nigricans.
Amphacanthus javus.

Fam. 15. *Fistulares.*
Fistularia tabaccaria.
Fistularia immaculata.
Centriscus scolopax.
Aulostoma sinense.
Amphisile scutata.

Ordo II. ANACANTHINI, *J. Müll.*

Fam. 1. *Gadoidei.*
Gadus æglefinus.
Gadus morrhua.
Lota vulgaris.
Motella tricirrhata.
Lepidoleprus trachyrhynchus.

Fam. 2. *Pleuronectides.*
Rhombus maximus.
Rhombus podas.
Platessa flesus.
Plaguria, spec.
Achirus mollis.

Fam. 3. *Ophidini.*
Ophidium barbatum.
Fierasfer imberbis.
Ammodytes tobianus.

Fam. 4. *Leptocephalidæ,* Bp.
Helmichthys punctatus.
Oxystomus hyalinus.
Leptocephalus pellucidus, *Bp.*
Hyoprorus messanensis, *Köll.*

Ordo III. PHARYNGOGNATHI, *J. Müll.*

Fam. 1. *Labroidei cycloidei.*
Labrus variegatus.
Labrus scrofa.
Julis vulgaris.
Julis pavo.
Crenilabrus pavo.
Xirichthys novacula.
Scarus creticus.

Fam. 2. *Labroidei ctenoidei.*
Pomacentrus fuscus.
Dascyllus araucanus.
Heliases castaneus.
Glyphisodon rhati.

Fam. 3. *Chromides.*
Chromis nilotica.
Chromis surinamensis.
Chromis, spec.
Cichla Deppii.

Fam. 4. *Scomberesoces.*
Belone vulgaris.
Belone caudimacula.
Tylosurus imperialis, *Bp.*
Sayris Camperi.
Hemirhamphus, spec.
Exocœtus exsiliens.

Ordo IV. Physostomi, *J. Müll.*
Fam. 1. *Siluroidei.*
Subfam. *Eremophilini,* Bp.
Trichomycterus punctulatus.

Fam. 4. *Cyprinodontes.*
Pœcilia vivipara.
Anableps tetrophthalmus.
Cyprinodon calaritanus.
Molienesia latipinnis.
Orestias tæniatus.
Fundulus nigrescens.

Fam. 6. *Esoces.*
Esox vulgaris.
Umbra Krameri.

Fam. 7. *Galaxiæ.*
Galaxias truttaceus.

Fam. 9. *Scopelini.*
Saurus lacerta.
Myctophum elongatum, *Bp.*
Ichthyococcus Poweriæ, *Bp.*
Gonostoma denudata, *Raf.*
Argyropelecus hemigymnus, *Cocco.*
Odontostoma Balbo.

Fam. 10. *Chauliodontidæ, Bp.*
Chauliodus setinotus, *Schn.*
Stomias barbatus, *Risso.*

Fam. 12. *Heteropygii.*
Amblyopsis spelæus.

Fam. 15. *Symbranchii.*
Symbranchus marmoratus.
Symbranchus immaculatus.
Amphipnous cuchia.
Monopterus javanicus.

Ordo V. Plectognathi.
Fam. 1. *Balistini.*
Balistes capriscus.
Monacanthus geographicus.
Aluteres lævis.
Triacanthus brevirostris.

Fam. 2. *Ostraciontes.*
Ostracion triqueter.

Fam. 3. *Gymnodontes.*
Diodon, spec.
Tetraodon fahaca.
Tetraodon lineatus.
Orthagoriscus mola.

Ordo VI. Lophobranchii.
Syngnathus typhle.
Hippocampus guttulatus.
Pegasus draco.

II. *Fishes whose bones contain bone-corpuscles.*

Subclassis. I. **Teleostei,** *J. Müll.*
Ordo I. Acanthopteri.
Fam. 8. *Scomberoidei.*
Thynnus vulgaris.
Thynnus alalonga.
Auxis bisus.

Ordo IV. Physostomi.
Fam. 1. *Siluroidei.*
Silurus glanis.
Silurus bicirrhis.
Schilbe mystus.
Synodontis serratus.
Malapterurus electricus.
Malapterurus beninensis.
Heterobranchus anguillaris.
Chaca lophioides.
Plotosus unicolor.
Clarias fuscus.
Pimelodus, spec.
Arius, spec.
Bagrus, spec.
Callichthys, spec.
Loricaria cataphracta.

Auchenipterus furcatus.
Heteropneustes fossilis.
Aspredo lævis.

Fam. 2. *Cyprinoidei.*
Phoxinus lævis.
Cobitis barbatula.
Aspius bipunctatus.
Alburnus lucidus.
Gobio fluviatilis.
Rhodius amarus.
Cyprinus carpio.
Abramis blicca.
Leuciscus rutilus.
Leuciscus tincella.
Tinca chrysitis.
Barbus vulgaris.
Barbus elongatus.
Barbus obtusirostris.
Barbus marginatus.
Chondrostoma risella, *Ag.*
Dangila lipocheila.
Labeo niloticus.
Catostomus, spec.

Fam. 3. *Characini.*
Citharinus Geoffroyi.
Distichodus niloticus.
Hydrocyon Forskahlii.
Alestes dentex.
Tetragonopterus mexicanus.
Anodus cyprinoides.
Leporinus, spec.
Pacu tæniurus.
Pacu nigricans.
Erythrinus unitæniatus.
Macrodon trahira.
Piabuca bimaculata.
Gasteropelecus sternicla.
Chirodon, *Girard*, n. spec.
Brycon, *Müll. Tr.*, n. spec.

Fam. 5. *Mormyri.*
Mormyrus bane.
Mormyrops anguilloides.
Mormyrus longipinnis.
Mormyrus oxyrhynchus.
Mormyrus cyprinoides.
Mormyrus, spec.

Fam. 8. *Salmones.*
Salmo salar.
Salmo trutta.
Argentina silur.

Fam. 11. *Clupeini.*
Clupea harengus.
Alosa vulgaris.
Alosa melanura.
Coilia Grayi.
Engraulis encrasicholus.
Engraulis Brownii.
Notopterus Pallasii.
Macrostoma angustidens, *Risso.*
Melettā thryssa.
Elops saurus.
Megalops cyprinoides.
Chatoessus cepedianus.
Chatoessus punctatus.
Gnathobolus mucronatus.
Chirocentrus dorab.
Pristigaster, spec.
Lutodeira chanos.
Butirinus macrocephalus.
Hyodon claudulus.
Heterotis niloticus.
Osteoglossum Vandellii.
Osteoglossum formosum.
Sudis gigas.
Alepocephalus rostratus.

Fam. 13. *Murænoidei.*
Anguilla vulgaris.
Conger myrus.
Ophisurus serpens.
Nettastoma melanura.
Sphagebranchus imberbis.

Fam. 14. *Gymnotini.*
Gymnotus electricus.
Carapus brachyurus.

Subclassis II. **Ganoidei.**

Ordo I. HOLOSTEI.

Fam. 1. *Lepidosteini.*
Lepidosteus platyrhynchus.

Fam. 2. *Polypterini.*
Polypterus bichir.
Fam. 3. *Amiidæ.*
Amia calva.

Ordo II. Chondrostei.
Fam. 1. *Acipenserini.*
Acipenser nacarii.
Scaphyrhynchus Rafinesquii.
Fam. 2. *Spatulariæ.*
Spatularia folium.

Subclassis III. **Dipnoi.**
Fam. 1. *Sirenoidei.*
Lepidosiren annectens.

From these facts it follows that the osseous fishes, notwithstanding their great number, are separated in a very remarkable way into two groups, as shown in the following enumeration :—

Fishes with bone-corpuscles.

I. All the extensive and higher-organized tribes of *Physostomi,* J. Müll.; viz. the
   Siluroidei (except *Trichomycterus*).
   Cyprinoidei.
   Characini.
   Mormyri.
   Salmones.
   Clupeini.
   Murænoidei.
   Gymnotini.
II. All the *Ganoidei.*
III. The *Sirenoidei.*
IV. Of the *Acanthopteri,* only the genus *Thynnus,* Cuv.

Fishes without bone-corpuscles.

I. All the numerous tribes of the *Acanthopteri,* with the exception of the genus *Thynnus.*
II. All the *Anacanthini,* J. Müll.
III. The *Pharyngognathi,* J. Müll.
IV. Some smaller and lower-organized tribes of *Physostomi,* as the
   Cyprinodontes.
   Esoces.
   Galaxiæ.
   Scopelini.
   Chauliodontida, *Bp.*
   Heteropygii.
   Symbranchii.
And of the Siluroids, only the genus *Trichomycterus.*
V. The *Plectognathi.*
VI. The *Lophobranchii.*

As there can be no doubt that most of the higher-organized fishes are amongst those with bone-corpuscles, and as we know that amongst the higher vertebrata, even the lowest, viz. the Perennibranchiata, possess real osseous tissue, it seems to follow that the peculiar distribution of real osseous tissue and of the "osteoid" structure, as the osseous tissue without corpuscles may be called, has a deeper signification. This will be found by studying the development of the bones in both groups; and I hope to be able, before long, to present to the Royal Society some new facts with regard to this matter also; but in the mean time, until my observations are more complete, I must abstain from further explanation.

The facts exposed hitherto have had reference only to a great and fundamental structural difference between two extensive groups of osseous fishes. I may now add, that there exist also greater or lesser structural discrepancies amongst the different tribes of each group. But as this is not a suitable occasion for an exposition of the details of this question, I will only say this much:—In the higher fishes, those with real osseous tissue, there exist differences, especially with regard to the *form* and *size* of the bone-corpuscles; and I hope to be able to show that there are peculiar and tolerably well characterized types of them amongst the Ganoids, Siluroids, *Salmonidæ*, Cyprinoids, *Clupeini*, &c. In the second group there are more varieties. In some tribes the bones are quite structureless homogeneous masses, as in the *Leptocephalidæ*; in others they have a peculiar fibrous appearance, and consist of a singular mixture of cartilage and osteoid structures, as Quekett first showed in the genera *Orthagoriscus* and *Lophius*, to which I may add some *Balistini*; but in the great majority of the tribes of this group, the bones contain peculiar *tubes* more or less similar to those of dentine. If these tubes are well developed, the bones acquire a structure *which can in no way be distinguished from that of dentine*,—a fact, which also did not escape the perspicacity of Quekett, who mentions its occurrence in the genus *Fistularia*, the Barracuda Pike (*Sphyræna barracuda*), and the Gar-fish (*Belone vulgaris*). I found the same structure in many other genera of this group, especially among the *Plectognathi*, *Pharyngognathi*, *Sparidæ*, and *Squamipennes*; but in the greater number this tubular structure is not so well developed, and is intermingled with more structureless parts. Another fact deserving of mention with regard to the bones of this group is, that there very frequently occur also structures, formed by the agglomeration of calcareous globules of different sizes, which resemble in a remarkable degree the lower layers of common fish-scales.

My observations have also extended to the *hard structures of the skin* of fishes, and of *the rays of the fins*; and I may say that in general the same laws, which apply to the structure of the endoskeleton, hold good also for the exoskeleton. Evidence of this is especially afforded by the fins, the rays of which, independently of their hard or soft condition, contain bone-corpuscles in all the tribes where the internal bones are provided with them, whilst in the other

case these rays are formed of a homogeneous osteoid substance or of a tubular structure, which may also in some fishes, as Williamson first showed in the Ostracionts, assume the structure of real dentine, as in many Plectognaths (*Triacanthus, Monacanthus, Aluteres, Tetraodon,* and others), and in certain *Acanthopterygii* (*Equula, Ephippus, Hæmulon, Pristipoma, Scatophagus, Centrarchus*). With regard to the skin, we may at least go so far as to say that in no fish whose endoskeleton is destitute of bone-corpuscles do they exist in the hard structures of the skin; but, on the other hand, the tribes which have real osseous tissue do not all present it also in the skin. Scales or plates with bone-corpuscles are found amongst living Ganoids, *e. g.* in *Polypterus, Lepidosteus,* and even *Amia* (in whose scales J. Müller erroneously supposed them to be wanting), and also in the *Acipenserini* and *Spatulariæ*; they exist also in the fossil Ganoids, as the excellent observations of Williamson have shown.

In many Ganoids, moreover, as Williamson and Quekett have shown, the scales often contain dentinal tubes, or even portions of real dentine ("Kosmine" of Williamson) amidst true bone. In the scales of *Lepidosiren*, also, I find bone-corpuscles, but mostly fusiform, and only here and there having a simple stellate figure. Of the other fishes which have bone-corpuscles in their skeleton, little has hitherto been noted as to the coexistence of such corpuscles in their scales, but I find it to prevail to a considerable extent among them. The presence of bone-corpuscles has been long known, it is true, in the larger scales of the "corselet" of *Thynnus*, also in the dermal plates of certain Siluroids (*Loricaria* and *Callichthys*), and was pointed out by J. Müller in the scales of *Sudis*. Leydig, too, states that true bone-corpuscles exist in the walls of the grooves and semicanals upon the scales of the lateral line in certain Cyprinoids (Carp, Tench, and Barbel). This statement I am able fully to confirm, and to add the following genera in which I find the same thing to occur; viz.—*Hydrocyon, Alepocephalus, Macrostoma,* Risso, *Piabuca, Serrasalmo, Xiphorhamphus, Tetragonurus, Salminus, Chalcinus, Pygocentrus, Labeo,* and *Catostomus*. But, besides the instance of *Sudis* and certain Siluroids above referred to, I find that many other *Physostomi* have true bone-corpuscles in their scales; not only those of the lateral line, but all of them. From the results of my examinations up to this time, which, however, on account of

the want of materials, are by no means complete, I am able to make out the following list :—

1. Characini.

Of this family I have had the means of examining nearly all the genera, including forty-one species.

Characini with bone-corpuscles in all their scales.

Erythrinus unitæniatus, *Spix.*
Erythrinus microcephalus, *Agass.*
Macrodon trahira, *J. Müll.*
Macrodon auritus, *Val.*
Pacu tæniurus (Prochilodus tæniurus, *Val.*).
Pacu nigricans, *Spix.*
Pacu lineatus, *Val.*
Distichodus niloticus, *Müll. Tr.*
Alestes dentex, *Müll. Tr.*
Anodus cyprinoides, *Müll. Tr.*
Anodus edentulus, *Agass.*
Anodus leucos, *de Fil.*
Schizodon fasciatus, *Agass.*
Chilodus punctatus, *Müll. Tr.*
Rhaphiodon (Cynodon) vulpinus, *Agass.*
Leporinus fasciatus, *Müll. Tr.*
Leporinus elongatus, *Val.*
Citharinus latus, *Ehr.*

Characini without bone-corpuscles in their scales.

*Hydrocion Forskahlii, *Cuv.*
*Piabuca bimaculata (*Hyrtl. misit*).
Gasteropelecus sternicla, *Bl.*
Gasteropelecus securis, *de Fil.*
Cheirodon Girard, nov. sp., *de Fil.*
Brycon falcatus, *Müll. Tr.*
Brycon, nov. sp., *de Fil.*
Serrasalmo rhombeus, *Cuv.*
*Serrasalmo marginatus, *Val.*
Xiphorhamphus falcatus, *Müll. Tr.*
*Xiphorhamphus hepsetus, *Müll. Tr.*
Myletes rubripinnis, *Müll. Tr.*
Myletes rhomboidalis, *Cuv.*
Tetragonurus mexicanus, *de Fil.*
*Tetragonurus argenteus, *Art.*
*Tetragonurus maculatus, *Müll. Tr.*
*Salminus Orbignyanus, *Val.*
*Chalcinus Mülleri, *de Fil.*
Pygocentrus nigricans, *Müll. Tr.*
Epicyrtus gibbosus, *Müll. Tr.*
Piabucina erythrinoides, *Val.*
Exodon paradoxus, *Müll. Tr.*
Leporinus, spec.

With regard to the members of the second division, it is to be observed, that probably in all of them the canals attached to the scales of the lateral line are formed of true osseous tissue; in those marked with an asterisk I have found this by actual examination.

The *Characini* are thus divisible into two groups, according to the nature of their scales; at the same time, these are not to be regarded as natural divisions in other respects, and the less so as one and the same genus, such as *Leporinus*, for example, may include species which differ in the composition of their scales. The presence of corpuscles, though connected partly with the size of the scales, does not depend solely on this, for they may be wanting in large scales

(*Hydrocyon, Chalcinus, Salminus*), and present in small ones (*Anodus edentulus, Chilodus*).

2. Mormyri.

Mormyrus longipennis, *Rüpp.*
Mormyrus oxyrhynchus.
Mormyrus bane.
Mormyrus cyprinoides.
Mormyrus, spec.
Mormyrops anguillaris.

3. Clupeini.

Megalops cyprinoides.
Elops saurus.
Coilia Grayi.
Notopterus Pallasii (corpusc. very scanty).
Butirinus macrocephalus.
Hyodon claudulus.
Osteoglossum Vandelii.
Osteoglossum bicirrosum.
Heterotis niloticus.

The plates of the abdominal carina in many *Clupeini* are formed throughout of true bone, but do not belong to the present category.

I am unable to find corpuscles in the scales of *Lutodeira chanos, Chatoessus punctatus* and *cepedianus*, and *Alosa vulgaris*. In several Cyprinoids (*Labeo, Catastomus, Barbus*), I have, in like manner, failed to discover corpuscles in the scales proper; on the other hand, I have found very distinct dentinal tubes in the scales of *Barbus*, at their hinder part.

True osseous tissue will doubtless hereafter be found in the scales of many other *Physostomi* which have it in their skeleton, but it is not to be supposed that this will apply to all.

In the *Physostomi*, as in the Ganoids, the bone-corpuscles lie in the lower stratum of the scale; still they are situated above the fibrous layer, and immediately beneath the structureless layer, to which in all scales I apply the name of "ganoin-layer," inasmuch as it has in all cases the same signification.

From the foregoing observations we are able to show still more positively than could be done by J. Müller, that the scales of Ganoids have no peculiarity of structure to distinguish them from those of the *Teleostei*. Nay, certain Ganoids, as *Amia*, have scales, which in respect even of pliancy, rounded contour, and the surface-marking of the ganoin-layer, agree with those of other fishes.

In reference to those fishes which want bone-corpuscles in their skeleton, I have still to remark,—1, that the corpuscles are also invariably wanting in the semicanals upon the scales of the lateral line; for what Leydig designates as rudimentary bone-corpuscles in the

Perch are in fact the tubules of the osteoid substance; 2, that amongst the group of fishes in question, there are some which have beautiful dentine in their skin-bones, *e. g. Amphisile scutata* and the Ostracionts.

To the foregoing remarks on the microscopic structure of the hard tissues of fishes, I may add, that there also exists a third group of fishes, in which the endoskeleton is composed only of common cartilage, or of cartilage with depositions of earthy salts, viz. the *Cyclostomi* and *Selachii.* None of these fishes, not even the *Plagiostomi* and *Chimæra,* possess real bone-cells in their hard parts; for these are formed only, as J. Müller showed many years ago, by ossified cartilage, that is to say, cartilage-cells in an ossified matrix. Even the hard spines of the fins and of the skin of these animals are not real bone, but dentine, as was demonstrated long since by Agassiz and Quekett.

If now we sum up all that has been said, we arrive at the following conclusions:—

I. There exist *three types* of structure in the skeleton of fishes, viz.:

1. *Type of the Selachii.*

The skeleton is formed of cartilage or ossified cartilage.

Selachii, Cyclostomi.

2. *Type of the Acanthopterygii.*

The skeleton is formed of a homogeneous or tubular osteoid substance, often of true dentine.

Teleostei (*J. Müll.*), with the exception of the greater part of the Physostomi (*J. Müll.*).

3. *Type of the Ganoidei.*

The skeleton is formed of real osseous tissue.

Most of the Physostomi, the Ganoidei, and Sirenoidei.

II. The *exoskeleton* follows in some respects the same laws as the endoskeleton, and shows the following types:—

1. *Exoskeleton formed of a homogeneous and fibrous osteoid substance.*

Scales of the majority of the Teleostei.

2. *Exoskeleton formed of dentine.*

Spines of Selachii and scales of Plectognathi, and of *Amphisile,* in part.

3. *Exoskeleton formed of real bone*; partly in association with homogeneous osteoid substance (*ganoin*) and dentinal tubes.

Scales of Ganoidei, of *Lepidosiren*, some Siluroidei, of Mormyri, many Characini and Clupeini, also of *Thynnus*.

In terminating this communication, I think it proper to mention that the great liberality with which my friend Mr. Tomes of London, and Professor Williamson of Manchester, put their large collections of microscopic preparations of teeth, bones, and scales at my disposal, proved of great assistance in my investigations, and, accordingly, I am only fulfilling an agreeable duty in now publicly expressing my obligations to them. I am also greatly indebted to my friends Filippo de Filippi of Turin and Henry Müller of Würzburg, also to Dr. Hyrtl of Vienna, and Dr. Peters of Berlin, who supplied me with many of the rarer Mediterranean and foreign fishes. But, in order that my observations may yield the results which may not unreasonably be expected from them, I need more aid; and as England is the country in which not only the largest zoological collections of fishes, but also the greatest number of microscopic preparations of the hard tissues of recent and fossil animals, are to be found, I take the liberty to ask the possessors of such collections who may be interested in this matter to favour me with such specimens as may seem to them calculated to give to this series of observations the greatest possible extension.

III. "On the Physical Phenomena of Glaciers."—Part II. By Dr. Tyndall, F.R.S. Received February 24, 1859.

[Abstract.]

The main portion of this Paper deals with the *veined structure* of glacier ice. The author refers to his observations in the Mer-de-glace in 1857, and his reasons for withholding them, and visiting the glaciers once more in 1858.

He describes the general aspect of the structure, and examines the two theories of the phenomenon which are now deserving of attention; one of these considers the blue veins to be a continuation of the bed-

ding of the Névé, the other regards them as being produced by pressure.

Wishing to confer upon the inquiry the character of an *experimental* one, his desire in 1858 was to examine a great number of glaciers, which should exhibit the ice under different mechanical conditions, thus accepting the combinations made by nature as a substitute for those, which, under ordinary circumstances, are made by the experimentalist himself. He therefore first visited and examined the glaciers of Grindelwald. Crossing the Strahleck, he descended the glacier of the Aar to the Grimsel, and proceeded thence to the glacier of the Rhone. He subsequently spent eight days in the neighbourhood of the great Aletsch glacier, and afterwards eleven days exploring the range of glaciers which stretches from Monte Rosa to the Mont Cervin. From Zermatt he proceeded to Saas, and spent five days in the vicinity of the Allalein glacier: he afterwards visited the Fée glacier, and completed his expedition by a visit to the Mer-de-glace and its tributaries, and a second expedition to the summit of Mont Blanc.

The Paper contains an account of the observations made upon all these glaciers, and these observations go unitedly to prove that the production of the structure is independent of the stratification of the Névé, and is the result of intense pressure acting upon the glaciers. The author points out the place at which the structure is manufactured, and whence it is sent forward, giving a character to other portions of the glacier which have no share in its formation.

The observations include some which leave no doubt as to the general independence of structure and stratification. On the Furgge glacier, for example, fine ice sections are exposed, which show the bedding in a perfectly distinct and beautiful manner; while *crossing the beds at a high angle,* we have the true veined structure. The coexistence of both is exactly analogous to that of cleavage and bedding in slate rocks. While however the independence of both is thus proved, it is not asserted that the direction of structure and stratification never coincide. As the cleavage of rocks is sometimes parallel to the bedding, so may the strata of glaciers coincide with the structure; and this is probably the case in many of the so-called *secondary* glaciers of the Alps.

The author divides the questions of structure into three *principal* cases :—

1st. *Marginal Structure,* produced by pressure due to the swifter motion of the centre of the glacier.

2nd. *Longitudinal Structure,* produced by pressure consequent on the mutual thrust of two tributary glaciers ; developing veins which run parallel to the direction of the trunk stream.

3rd. *Transverse Structure,* produced by pressure due to the change of inclination,—and the thrust from behind, endured by glaciers at the bases of the ice-falls.

The author also gives a physical analysis of the mode in which the pressure produces the structure. He shows experimentally that planes of liquefaction are produced in ice at right angles to the direction of a pressure acting upon the mass. In the glacier these planes of liquefaction are the channels by which the air is ejected, and the blue veins produced.

A section of the Paper is devoted to the consideration of the shape of the bubbles entangled in glacier ice ; as affording evidence of pressure. The author has endeavoured to refer the observed facts to their true cause, and to show that the conclusions hitherto drawn from this remarkable phenomenon are untenable. The shape of the bubbles furnishes no ground for any conclusion regarding the pressure to which the mass containing them has been subjected.

The Paper also includes a short section containing remarks on glacier motion ; in which it is shown that this motion is of a composite character ; being partly due to the sliding of the glacier over its bed, and partly to the yielding of the ice under severe pressure. A brief section is also devoted to the explanation of the Dirt-bands of the Mer-de-glace.

March 3, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates for election into the Society:—

Frederick Augustus Abel, Esq.
Somerville Scott Alison, M.D.
Charles Spence Bate, Esq.
Henry Foster Baxter, Esq.
Samuel Husbands Beckles, Esq.
Thomas William Burr, Esq.
Frederick Crace Calvert, Esq.
Henry J. Carter, Esq.
William White Cooper, Esq.
Richard Cull, Esq.
Thomas Rowe Edmonds, Esq.
Alexander John Ellis, Esq.
S. W. Fullom, Esq.
Douglas Galton, Capt. R.E.
Samuel Gurney, Esq.
William Bird Herapath, M.D.
George Murray Humphry, M.B.
Thomas Sterry Hunt, Esq.
Waller Augustus Lewis, M.D.
John Denis Macdonald, Esq.
David Macloughlin, M.D.
Henry Maudslay, Esq.
The Rev. Walter Mitchell, M.A.
Robert William Mylne, Esq.
William Odling, Esq.
Robert Patterson, Esq.
John Penn, Esq.
Sir Robert Schomburgk.
Edward Smith, M.D.
Henry Ward, Capt. R.E.
Thomas Watson, M.D.
Col. Frederick Eardley Wilmot, R.A.
Bennet Woodcroft, Esq.
Col. William Yolland, R.E.

The following communications were read:—

I. "On an Experiment in which the Stratifications in Electrical Discharges are destroyed by an interruption of the Secondary Circuit." By J. P. Gassiot, Esq., F.R.S. Received February 17, 1859.

The author having referred to his former Papers*, and to Mr. Grove's view of the phenomenon in question, performed an experiment to demonstrate the fact stated in the title of the present communication.

* Philosophical Transactions, 1858, and Proceedings, January 13, 1859.

II. "Remarks on Organo-Metallic Bodies; 4th Memoir." By EDWARD FRANKLAND, Ph.D., F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

(Abstract.)

In a former Memoir* the author described the production of a new series of organic compounds containing the metal tin in combination with the radicals methyl, ethyl, and amyl. His attention was at that time especially directed to the compound formed by the union of tin with ethyl, and to which the name stanethyl was given. The iodide of stanethyl was prepared by exposing iodide of ethyl to light or heat in the presence of tinfoil; and, by acting with zinc upon the aqueous solution of this iodide of stanethyl or of the chloride of the same body, stanethyl itself (C_4H_5Sn) was obtained.

In accordance with a theory of the constitution of all organo-metallic bodies which the author then suggested, the above compounds were respectively represented as the analogues of the protiodide and biniodide of tin, thus—

$$\underbrace{Sn\,I}_{\text{Stannous iodide.}} \qquad \underbrace{Sn(C_4H_5)}_{\text{Stannous ethide.}}$$

$$\underbrace{Sn\left\{\begin{matrix}I\\I\end{matrix}\right.}_{\text{Stannic iodide.}} \qquad \underbrace{Sn\left\{\begin{matrix}C_4H_5\\I\end{matrix}\right.}_{\text{Stannic ethiodide.}}$$

It is evident that the application of this theory to the above bodies would receive considerable additional support if the second equivalent of iodine in the stannic iodide could be replaced by ethyl, or some other analogous organic group. In the Memoir already alluded to, it was mentioned, that in studying the behaviour of stanethyl under the influence of heat, evidence was obtained of the existence of this very compound,—stannic ethide, or *binethide of tin,* as it was then named. This body obviously bears the same relation to stannic iodide as stanethyl bears to stannous iodide.

$$\underbrace{Sn\left\{\begin{matrix}I\\I\end{matrix}\right.}_{\text{Stannic iodide.}} \qquad \underbrace{Sn\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Stannic ethide.}}$$

* Transactions of the Royal Society for 1852, p. 418.

Although there could be little doubt of the formation of stannic ethide by heating stanethyl to 150° C., yet the author could not succeed in obtaining the former body in a state of purity from this source: it seemed probable, however, that stannic ethiodide would be easily converted into stannic ethide by bringing it into contact with zincethyl; and a preliminary experiment completely realized this expectation. The results of this reaction, together with its extension to other analogous organo-metallic compounds, form the subject of the present Memoir.

I. *Action of Zincethyl upon Iodide of Stanethyl.*

About two ounces of crystals of iodide of stanethyl were gradually added to a strong solution of zincethyl in ether, care being taken to preserve an excess of zincethyl. On submitting the resulting syrupy liquid to distillation, it began to boil at 70° C.; but the thermometer rapidly rose to 180° C., between which temperature and 200° C. the greater part of the product passed over, solid iodide of zinc containing a little zincethyl being left in the retort. The distillate was washed with dilute acetic acid, and the dense ethereal liquid which separated was dried over chloride of calcium, and rectified. The greater portion of it distilled at 181°, and was collected apart. Submitted to analysis, it yielded results leading to the formula—

$$Sn\left\{\begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}\right. .$$

The following equation, therefore, expresses the action of zincethyl upon iodide of stanethyl:—

$$\left.\begin{matrix} Sn(C_4H_5)I \\ Zn(C_4H_5) \end{matrix}\right\} = \left\{\begin{matrix} Sn(C_4H_5)_2 \\ ZnI \end{matrix}\right. .$$

Stannic ethide or binethide of tin is a limpid colourless liquid even at −13° C., possessing a very faint ethereal odour, resembling that of oxide of stanethyl, and a slightly metallic, though not unpleasant taste. Its specific gravity is 1·187 at 23° C. A determination of the specific gravity of its vapour gave the number 8·021, showing that stannic ethide consists of one volume of thin vapour and four volumes of ethyl, the five volumes being condensed to two. Stannic ethide boils at 181° C., and distils unchanged, thus differing from stannous ethide, which decomposes at 150°, chiefly into metallic tin and

stannic ethide, a reaction calling to mind the behaviour of stannous oxide when boiled with a caustic alkali. Stannic ethide is inflammable, burning with a lurid flame fringed with deep blue and evolving white fumes of stannic oxide. In oxygen it burns much more brilliantly, with a white light fringed with blue.

It was important to ascertain the deportment of stannic ethide with negative elements, since, if it were found to be capable of direct combination, its analogy to inorganic stannic compounds would be, to a great extent, disproved. Like zincethyl, however, stannic ethide is incapable of combining with any other element without the expulsion of at least an equivalent amount of its ethyl. Treated with iodine, the latter dissolves with a deep brown colour, which, however, gradually disappears; and if the addition of iodine be continued until decolorization be no longer effected, the resulting liquid, on being submitted to distillation, is found to consist of iodide of ethyl, which distils over, and an iodine salt, possessing the unbearably pungent odour of one of the products of the action of tin upon iodide of ethyl at 160° C., and described by MM. Cahours and Riche as iodide of distannous ethyl, $Sn_2(C_4H_5)_2I$. The iodine salt appears, in fact, to be either identical with this body or to consist of *stannic iodotriethide* $(Sn_2(C_4H_5)_3I)$*.

Stannic ethide does not decompose water, and is not acted upon by strong aqueous hydrochloric acid in the cold. When, however, heat is applied to the mixture of the two liquids, bubbles of gas are slowly evolved; but it requires from twelve to eighteen hours to complete the reaction. The gas was found to be pure hydride of ethyl, and the quantity evolved was such as to show that exactly one equivalent of ethyl was expelled in the form of hydride from two equivalents of stannic ethide, indicating the following reaction:—

$$\left.\begin{matrix} 2Sn(C_4H_5)_2 \\ HCl \end{matrix}\right\} = \left\{\begin{matrix} C_4H_5H \\ Sn_2(C_4H_5)_3Cl. \end{matrix}\right.$$

II. *Action of Zincmethyl upon Iodide of Stanethyl.*

About three ounces of crystallized iodide of stanethyl were gra-

* Whilst I was engaged with these experiments, Mr. Buckton announced the formation of stannic ethide (Proceedings of the Royal Society, vol. ix. p. 315), and mentioned his intention to study the salts formed by the action of iodine, bromine, &c., upon that body; I have not, therefore, prosecuted the inquiry further in this direction.

dually added to a solution of zincmethyl in ether. Considerable heat was evolved, and the vessel in which the reaction was performed required to be plunged into cold water. On treating the product as before described, a liquid was obtained boiling between 143° and 148° C., and yielding, on analysis, numbers closely corresponding with the formula

$$Sn \left\{ \begin{matrix} C_4 H_5 \\ C_2 H_3. \end{matrix} \right.$$

The action of zincmethyl upon iodide of stanethyl may therefore be thus expressed:—

$$\left. \begin{matrix} Sn \left\{ \begin{matrix} C_4 H_5 \\ I \end{matrix} \right. \\ Zn\, C_2 H_3 \end{matrix} \right\} = \left\{ \begin{matrix} Sn \left\{ \begin{matrix} C_4 H_5 \\ C_2 H_3 \end{matrix} \right. \\ Zn\, I. \end{matrix} \right.$$

The new body thus formed, and for which I propose the same *stannic ethylomethide*, is a colourless limpid liquid, undistinguishable in appearance from stannic ethide. It possesses, like the latter, a very faint ethereal odour and a slightly metallic taste. Its specific gravity is 1·2319 at 19° C. It does not solidify at −13° C. Stannic ethylomethide boils between 144° and 146° C. The specific gravity of its vapour is 6·838, showing that its constitution is similar to that of stannic ethide. It is easily inflammable, and exhibits the same deportment as stannic ethide with chlorine, iodine, and bromine; its combination with these elements being always attended with the expulsion of methyl. Stannic ethylomethide dissolves iodine, assuming a magnificent crimson colour, which disappears with extreme slowness unless heat be applied. When, however, action has once been set up, it goes on with considerable rapidity, even in the cold. The products of this reaction were proved to be iodide of methyl and iodide of distanethyl:—

$$2 \left\{ \begin{matrix} C_4 H_5 \\ C_2 H_3 \\ 3I \end{matrix} \right\} = \left\{ \begin{matrix} 2 C_2 H_3 I \\ Sn \left\{ \begin{matrix} C_4 H_5 \\ C_4 H_5 \\ I. \end{matrix} \right. \end{matrix} \right.$$

Iodide of distanethyl, which has already been partially examined, although with discordant results, by M. Löwig and by MM. Cahours and Riche, is a dark straw-coloured, somewhat oily liquid, which does not solidify at −13° C. It possesses an extremely pungent and intolerable odour, resembling oil of mustard. Its specific

gravity at 15° C. is 2·0329. At 208° C. it enters into ebullition, but cannot be distilled without decomposition.

Treated with hot hydrochloric acid, stannic ethylomethide is decomposed, yielding a crystallisable salt and a gaseous mixture, consisting of—

Hydride of ethyl	81·21
Hydride of methyl	18·79
	100·00

III. *Action of Zincethyl upon Iodide of Mercurymethyl.*

The formation of stannic ethylomethide in the manner just described, encouraged the author to attempt a similar reaction with iodide of mercurymethyl, $Hg\left\{\begin{matrix}C_2H_3 \\ I\end{matrix}\right.$. Mr. Buckton's announcement* of the formation of mercuric ethide, $Hg\left\{\begin{matrix}C_4H_5 \\ C_4H_5\end{matrix}\right.$, by an analogous reaction, tended also to strengthen the hope that a mercuric ethylomethide might be thus obtained.

Iodide of mercurymethyl is readily acted upon by zincethyl, but no mercuric ethide was produced, the reaction being expressed by the following equation:—

$$\left.\begin{matrix}Hg\,(C_2H_3)\,I \\ 2\,Zn\,(C_4H_5)\end{matrix}\right\} = \left\{\begin{matrix}Hg\,(C_4H_5)_2 \\ Zn\,(C_2H_3) \\ Zn\,I.\end{matrix}\right.$$

IV. *Action of Zincmethyl upon Chloride of Mercuryethyl.*

Although the above reaction failed to produce mercuric ethylomethide, it was still possible that this body might be formed by acting upon a mercuryethyl compound with zincmethyl. About five ounces of chloride of mercurous ethyl ($Hg\,(C_4H_5)\,Cl$) were added to four ounces of a strong ethereal solution of zincmethyl. Considerable heat was evolved; and after forty-eight hours the product was distilled. All the volatile portion came over below 140° C. The distillate was washed with weak acetic acid, dried over chloride of calcium, and then rectified. A considerable portion distilled between 127° and 137° C., and was collected apart. The last few drops came over at 156° C. Repeated rectifications of the product

* Proceedings of the Royal Society, vol. ix. p. 312.

boiling between 127° and 137° C. did not serve to isolate any portion of the distillate, having a fixed boiling point; on the contrary, it was evident that the range of the temperature of distillation became wider each time the operation was repeated. A section boiling between 127° and 133° gave, on analysis, 13·68 per cent. of carbon, whilst another section, boiling between 141° and 143°, gave 16·71. The formula $Hg\left\{\begin{matrix} C_4 H_5 \\ C_2 H_3 \end{matrix}\right.$ requires 14·75 per cent. of carbon. Mercuric methide boils at 96°, and mercuric ethide at 159° C.; consequently mercuric ethylomethide might be expected to boil at about 128°. The author considers it more than probable that mercuric ethylomethide was formed in the above reaction; but subsequent distillations gradually transformed it, more or less perfectly, into a mixture of mercuric ethide and mercuric methide.

$$2\,Hg\left\{\begin{matrix} C_4 H_5 \\ C_2 H_3 \end{matrix}\right\} = \left\{\begin{matrix} Hg\,(C_4 H_5)_2 \\ Hg\,(C_2 H_3)_2. \end{matrix}\right.$$

V. *Action of Zinc upon a Mixture of the Iodides of Ethyl and Methyl.*

In a former memoir* the author pointed out that the vapour volume of zincethyl indicated the constitution of that body to be represented by the formula $\left.\begin{matrix} C_4 H_5 \\ C_4 H_5 \end{matrix}\right\} Zn_2$; but it is evident that this formula would receive important confirmation if the double equivalent of zinc could be made to combine with two radicals of different composition. An attempt was made to produce such a body by submitting simultaneously the iodides of methyl and ethyl, mixed with an equal volume of ether, to the action of zinc at 100° C. In eighteen hours the decomposition of the iodides was complete, and the distilled product, on being rectified, began to boil at 38°, ether and zincmethyl distilling over; the thermometer then gradually and uniformly rose to 120° C., at which temperature the remainder of the product, consisting of pure zincethyl in considerable quantity, distilled over. No evidence whatever was obtained of the existence of an intermediate compound containing both ethyl and methyl.

* Transactions of the Royal Society for 1855, p 266.

VI. *Zincmethyl.*

The experiments detailed in the foregoing pages requiring the use of considerable quantities of zincmethyl, the author's attention was directed to the preparation of this body in much larger quantities than could be obtained by the operations in sealed glass tubes previously described by him. He found that the preparation of a strong ethereal solution of zincmethyl succeeded most satisfactorily in a copper digester, heated to 100° C.; in fact, the decomposition by zinc of an ethereal solution of iodide of methyl is much more quickly and perfectly effected than that of a similar solution of iodide of ethyl; but on submitting the product to rectification, a liquid was obtained boiling at about 51° C., spontaneously inflammable to the last degree, and possessing the intolerable odour of zincmethyl. On analysis, however, it yielded numbers closely agreeing with the formula—

$$2\left(\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\}\, Zn_2\right) + \left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \end{matrix}\right\}\, O_2.$$

The specific gravity of its vapour was 3·1215, a number which does not correspond with the theoretical specific gravity of a *compound* of the above formula, unless the exceedingly improbable assumption be adopted, that it contains two volumes of zincmethyl vapour, united with one volume of ether vapour, *without* condensation. On the other hand, it accords closely with the specific gravity of the vapour of a *mixture* of zincmethyl and ether in the above proportions.

Without at present offering any decided opinion as to the nature of this body, the author states that in repeated operations with large quantities of materials he has entirely failed in obtaining *pure* zincmethyl by this method of proceeding.

Similar repeated attempts to produce pure zincethyl from zinc and iodide of methyl, without the intervention of ether, were also unsuccessful, although this method generally succeeds in small glass tubes. This anomaly in the results obtained from the same materials heated in a copper digester and in glass tubes, is doubtless due to the difference of the conditions in the two cases. In a glass tube half immersed in a heated oil bath, a constant distillation of the internal liquid is going on, the liquid condensed in the upper portion of the tube flowing over an extensive surface of zinc in its

descent; whilst, in a digester of thick copper, the different parts of the vessel, owing to the high conductivity of the metal, are maintained at so uniform a temperature as to prevent any such circulation of the liquid from taking place.

The body just described being regarded as a mere mixture of zincmethyl and ether, incapable of being separated on account of the close proximity of their boiling points, a more successful result was anticipated by mixing the iodide of methyl with methylic ether instead of vinic ether. As methylic ether boils at $-21°$ C., it was thought that no such difficulty of separation could arise; the bodies employed would then, in fact, be exactly homologous with those so successfully used in the preparation of pure zincethyl on the large scale. It was found, however, that although a large quantity of zincmethyl was produced, yet it was impossible to obtain it free from methylic ether. A large portion of the product boiled at 43°, a small residuum only distilling between this temperature and 48°; both portions yielded, on analysis, results approaching the formula

$$2\left(\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\end{matrix}\right\}Zn_2\right)+\left.\begin{matrix}C_2\,H_3\\C_2\,H_3\end{matrix}\right\}O_2.$$

This result is, therefore, homologous with that obtained by the decomposition of iodide of methyl mixed with vinic ether.

In conclusion, the author states, that after an expenditure of many pounds of iodide of methyl, he has been unable to obtain even the smallest quantity of pure zincmethyl by the use of a copper digester, although a much larger product of the ethereal solution is obtained than in the corresponding preparation of zincethyl.

March 10, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. Letter from JAMES P. MUIRHEAD, Esq., to Sir BENJAMIN C. BRODIE, Bart., Pres. R.S., dated March 8, 1859, relating to the Discovery of the Composition of Water. Communicated by Sir B. C. BRODIE.

I have now, with your permission, to request you to lay before the

Royal Society the following brief remarks on the quotation from De Luc's "Idées sur la Météorologie," which has been referred to as fresh evidence in the controversy as to the discovery of the Composition of Water.

It is only at first sight, and when taken in an isolated form apart from the rest of De Luc's narrative, that the passage in question could bear the interpretation now sought to be put upon it; for Dr. Priestley's communication of Cavendish's experiment is said by De Luc to have been made "vers la fin de l'année 1782." But in the same section of the same volume he distinctly and positively says, that when in September [1783] he returned to Birmingham, "Nous ignorions, M. Watt et moi, que M. Cavendish eût eu des idées fort semblables aux siennes sur la Cause de ce Phénomène *."

Now, we may well ask, how could this possibly have been the case with De Luc in 1783, if Priestley's communication to him in 1782 had extended to the *conclusions*, as well as to the *experiments*, of Cavendish?

De Luc adds, on the next page of his work, that "Au mois de Juin" (an evident mistake for Janvier), "1784, M. Cavendish remit à la Société Royale un Mémoire, dans lequel il joignit, au récit de ses Expériences de 1781, sa théorie sur la formation de l'*Eau*†." Here, for the first time in De Luc's narrative (with the exception of an allusion to Blagden's statement at Paris in June 1783), occurs a clear and distinct notice of Cavendish's *theory* or *conclusions*, as distinguished from his *experiments*. What M. De Luc's opinion of the memoir was, in which those *conclusions* were announced, when he perused it in March 1784, and sent an analysis of it to Mr. Watt, is well known from his letters already published ‡.

We are thus enabled to set against the interpretation attempted to be put on the quotation from the "Météorologie," the most conclusive of all testimony; that, namely, of De Luc himself: for if he had intended to say that in the end of 1782 the *conclusions* of Cavendish had along with his *experiment* been communicated by Priestley, he could not possibly have gone on to say, as he has done a few pages later in the same volume, that in September 1783 he was ignorant of Cavendish having entertained any such ideas; nor

* "Idées sur la Météorologie," tome ii. p. 224.

† Ibid. p. 225.

‡ M. De Luc to Mr. Watt, 1st and 4th of March, 1784.

would he have felt the astonishment, and entertained the suspicions which he so strongly expresses, on his perusal of Cavendish's memoir in March 1784.

De Luc's account in the "Météorologie," it must also be observed, is not a contemporaneous one, published at the time of Priestley's communication in 1782, and before the conclusions of Watt were made known; but is given from memory, at an interval of several years, when such a mistake as that of June for January shows how little it can be relied on.

I am, &c.

JAS. P. MUIRHEAD.

II. "New Volatile Organic Acids, from the Berry of the Mountain Ash." By A. W. HOFMANN, LL.D., F.R.S. Received February 3, 1859.

Whoever has been engaged in the preparation of malic acid from the juice of the unripe berries of the Mountain Ash (*Sorbus Aucuparia*), cannot have failed to perceive the peculiar powerful odour evolved during the evaporation of the liquid partially saturated with lime. The body to which this odour belongs was hitherto unknown, and only lately, my friend and former pupil, Dr. George Merck of Darmstadt, when preparing malic acid on a large scale, conceived the happy idea of evaporating the liquid in a distilling apparatus. He thus obtained an acid distillate, from which he succeeded in separating an oily body possessed of acid properties. To the kindness of Dr. Merck I am indebted for an appreciable quantity of this remarkable body, which has enabled me to examine its properties and establish its composition.

The preparation of the oil from the aqueous acid obtained by distilling the mother liquor of the bimalate of calcium, presents no difficulty. The liquid is saturated with soda, evaporated and mixed with dilute sulphuric acid, when the oil rises as a brown layer to the surface of the liquid. It is separated by ether, and after the volatilization of the latter, submitted to distillation. The first portions of the distillate contain appreciable quantities of water; the thermometer, however, rapidly rises above 200° C. What now distils is a perfectly pure compound, which, on redistillation, exhibits a con-

stant boiling point at 220° C. Freshly distilled, the oil is colourless, but it soon acquires a yellowish tint. It has a peculiar aromatic odour, not disagreeable when dilute, but rather offensive when concentrated. The specific gravity is 1·0681. It is somewhat soluble in water, very soluble in alcohol and ether; these solutions are distinctly acid. The oil dissolves in potassa and ammonia, also in the carbonated alkalis, without, however, expelling their carbonic acid. Mineral acids separate it again from these compounds.

The analysis of the oil shows that it contains carbon, hydrogen, and oxygen in the ratio of

$$C_3\,H_2\,O\,;$$

but the determination of the silver in a white amorphous silver compound, obtained by adding nitrate of silver to the ammoniacal solution of the oil, shows that this expression must be quadrupled, and that the acid and silver salt are represented by the following formulæ:—

$$\text{Acid} \quad . \quad . \quad C_{12}\,H_8\,O_4$$
$$\text{Silver-salt} \; C_{12}\,[H_7\,Ag]\,O_4.$$

The acid oil of the mountain-ash berry exhibits a very remarkable deportment with the alkalis and acids. When gently heated (a temperature of 100° is sufficient) with solid hydrate of potassa, or when boiled with concentrated hydrochloric or moderately dilute sulphuric acid, the oil is readily converted into a splendid crystalline acid, greatly resembling benzoic acid in its general characters, which has the same composition as the oil itself. I have established this remarkable isomerism both by careful observation of the conditions of transformation, and by the analysis of the crystalline acid as well as of some of its salts and derivatives. I propose to designate this new compound by the term sorbic acid, thus reviving an old name which had at one time been used for malic acid. The isomeric oil obtained by distilling the juice of the mountain-ash berry, the acid properties of which are much less pronounced, may then be called Parasorbic acid.

Sorbic acid.—This substance is readily soluble in alcohol and ether, less so in water. Heated with a quantity of water insufficient for solution, it fuses; the aqueous solution, saturated by ebullition, solidifies on cooling into a network of interlaced needles. The acid crystallises best from a mixture of alcohol and water, in which

the latter predominates; from this solvent it is often deposited in magnificent needles several inches in length. Sorbic acid fuses at 134·5°. It boils at a much higher temperature, and may be volatilised without decomposition.

The alkaline sorbates are all soluble in water, the potassium, sodium, and ammonium compounds are extremely soluble, and crystallise with difficulty; the barium and calcium salts are less soluble, and may be obtained in splendid scaly crystals of the lustre of silver. Their crystallisation is facilitated by the addition of a small quantity of alcohol. Both salts are anhydrous, their analysis agreeing with the formulæ

Sorbate of barium . . . . . $C_{12}\,[H_7\,Ba]\,O_4$

Sorbate of calcium . . . . $C_{12}\,[H_7\,Ca]\,O_4$.

The silver-salt is a white amorphous precipitate, extremely insoluble in water, readily obtained by the decomposition of the ammonium compound by nitrate of silver. Both combustion and silver-determination proved this salt to be

Sorbate of silver . . . . . $C_{12}\,[H_7\,Ag]\,O_4$.

The ether of sorbic acid is readily procured by treatment of the alcoholic solution of sorbic acid with hydrochloric acid, or by the action of chloride of sorbyl upon alcohol. It is a colourless liquid of an agreeable aromatic odour resembling that of benzoic ether, boiling at 191° C., and containing:—

Sorbate of ethyl $C_{16}\,H_{12}\,O_4 = C_{12}\,[H_7(C_4\,H_5)]\,O_4$.

The experiments which I have quoted are sufficient to fix the composition of sorbic acid. I have nevertheless produced some additional derivatives of the acid.

Chloride of sorbyl is obtained by the usual processes; by the action of pentachloride of phosphorus upon the acid, or of the trichloride of phosphorus upon the potassium compound. The limited amount of acid at my disposal did not permit me to procure this substance in a state of purity, and to establish analytically the formula

$$C_{12}\,H_7\,O_2\,Cl$$

assigned to it by theory; but this formula is indirectly proved by the deportment of the crude product, still containing chloride of

phosphorus—with water, when sorbic acid is at once reproduced;—with alcohol, when sorbic ether is obtained;—with ammonia and phenylamine, when respectively, sorbamide and phenyl-sorbamide are generated. The chloride is not volatile without considerable decomposition.

Sorbamide.—This substance is formed by the action of dry carbonate of ammonium upon the crude chloride of sorbyl. White, readily fusible needles, soluble in water and alcohol. Composition of

$$\text{Sorbamide } H_{12}H_9NO_2 = \left.\begin{matrix} C_{12}H_7O_2 \\ H \\ H \end{matrix}\right\} N.$$

Phenyl-sorbamide is obtained by replacing the ammonia in the previous process by phenylamine. After treatment with water an oily liquid remains, which gradually solidifies into a crystalline mass. I have not analysed it, its composition being sufficiently characterized by theory.

When distilled with an excess of hydrate of baryta, sorbic acid exhibits the deportment of the acids with four equivalents of oxygen; carbonate of barium is produced, whilst an aromatic hydrocarbon distils over. The limited amount of material has precluded for the present the possibility of a more minute examination of this body.

Sorbic acid is obviously the first term of a new series of well-characterized organic acids, closely allied to the ordinary fatty and aromatic acids, occupying, in fact, a sort of intermediate position between the two. On comparing sorbic acid with the terms of the fatty and aromatic acid-series containing equal quantities of carbon, the hydrogen of sorbic acid stands in the middle

$$\underbrace{C_{12}H_{12}O_4}_{\text{Caproic acid.}} \qquad \underbrace{C_{12}H_8O_4}_{\text{Sorbic acid.}} \qquad \underbrace{C_{12}H_4O_4}_{\text{Lower homologue of benzoic acid.}}$$

The same remark applies to the carbon of sorbic acid when contrasted with the fatty and aromatic acids containing an equal quantity of hydrogen,

$$\underbrace{C_8H_8O_4}_{\text{Butyric acid.}} \qquad \underbrace{C_{12}H_8O_4}_{\text{Sorbic acid.}} \qquad \underbrace{C_{16}H_8O_4}_{\text{Toluic acid.}}$$

III. "Further Remarks on the Organo-metallic Radicals Mercuric, Stannic, and Plumbic Ethyl."—No. III. By GEORGE BOWDLER BUCKTON, Esq., F.R.S., F.L.S., F.C.S. Received March 3, 1859.

(Abstract.)

On resuming my inquiries into the nature of these organo-metals, I have met with some interesting reactions, which I here wish briefly to notice.

The preparation of mercuric, stannic, and plumbic ethyls, through the action of zinc ethyl on various organic and inorganic salts, has been already detailed in my sketch published in the 'Proceedings of the Royal Society*;' but at that time, I was not able to fix, with certainty, the constitution of the compounds which were produced by acids on the different radicals. An appeal to analysis now enables me to state the following

Mercuric ethyl.

The reactions of this liquid are well-marked. Towards sulphuric and hydrochloric acids it follows the deportment of its homologue mercuric methyl. When assisted by gentle heat, one equivalent of ethyl is disengaged, which unites with the hydrogen of the acid, and forms hydride of ethyl, whilst the acid takes its place, and gives rise to the corresponding salt of mercurous ethyl.

The radical bursts into flame when poured into chlorine gas, and is almost entirely destroyed; but when it is slowly mixed under water with iodine or bromine, the disengagement of ethyl gas is scarcely perceived, and iodide or bromide of ethyl may be recovered by distillation.

$$\underbrace{4\,(Hg\,C_4\,H_5)}_{\text{Mercuric ethyl.}}+2\,H\,SO_4=\underbrace{2\,(Hg_2\,C_4\,H_5)\,S_2O_8}_{\text{Sulphate of mercurous ethyl.}}+\underbrace{2\,(C_4\,H_5\,H)}_{\text{Hydride of ethyl.}}$$

$$\underbrace{2\,(Hg\,C_4\,H_5)}_{\text{Mercuric ethyl.}}+2\,Br=\underbrace{Hg_2\,C_4\,H_5\,Br}_{\text{Bromide of mercurous ethyl.}}+\underbrace{C_4\,H_5\,Br.}_{\text{Bromide of ethyl.}}$$

From considerations connected with the vapour density of mercuric ethyl and mercuric methyl, as given by experiment, there seem to be reasons for believing that the formulæ of all the organo-metals of this

* Proc. Roy. Soc. vol. ix. p. 309.

group should, in correctness, be doubled. I have not, however, yet been able to satisfy these views by direct experiment. Zincethyl acts readily on salts of mercurous methyl; and in all probability gives a body compounded of ethyl and methyl with a double equivalent of mercury $\left.\begin{matrix} Hg\,C_4\,H_5 \\ Hg\,C_2\,H_3 \end{matrix}\right\}$. The substance, however, if produced, is obviously broken by distillation into the two radicals mercuric ethyl and mercuric methyl. Experiment may perhaps prove more successful if salts of stannic methyl be similarly treated.

The electro-negative character of the group $C_{n2}\,H_{n2+1}$ in the class of organo-metals to which zincethyl belongs, may now perhaps be considered as established. Some interest, nevertheless, attaches to the question whether sodium is capable of displacing ethyl from mercuric ethyl. An answer to this question would give us some means of judging the position of ethyl, as regards its electro-negative function towards the true metals.

At ordinary temperatures, sodium has only a slow action on mercuric ethyl, but after the lapse of a few hours a voluminous grey sponge is formed, whilst the liquid entirely disappears. This sponge-like body has the property of spontaneous combustibility in a marked degree, and is liable to explosion from apparently very slight causes. By the application of a gentle heat, a strong rush of gaseous matter is evolved which eudiometric experiments proved to be a mixture of ethylene and hydride of ethyl, obviously proceeding from the disintegration of a double molecule of ethyl.

From this experiment we should conclude that ethyl, methyl, &c. in these radicals are still negative to mercury, and therefore, that mercury, copper, &c. would not, as Mr. Wanklyn supposes, displace ethyl in sodiumethyl*. More probably, perhaps, sodiumethyl is first formed in the reaction, and then decomposed by heat

$$Hg\,C_4\,H_5 + Na = Na\,C_4\,H_5 + Hg$$

and by heat

$$2\,(Na\,C_4\,H_5) + 2\,Hg = 2\,Na\,Hg + C_4\,H_4 + C_4\,H_5\,H.$$

The mercury is supposed here to be inert, and in no way to determine the decomposition.

Stannic diethyl.

Much of the uncertainty which has attached to some of the for-

* Proc. Roy. Soc. vol. ix. p. 345.

mulæ of the salts of stanethyl, has originated, without doubt, from the mode adopted by Löwig in their elimination. Strecker has lately shown that many of these compounds may, with probability, be referred to the types of the inorganic oxyiodides and oxychlorides of tin.

The following experiments were undertaken with the impression that the pure salts of stanethyl might be more advantageously procured by acting directly on the radical itself.

Stannic diethyl, $Sn\,(C_4\,H_5)_2$, like mercuric ethyl, loses one of its equivalents of ethyl when digested with concentrated acids. The action, however, is very slow with hydrochloric acid, an oily body being first formed, possessed of an exceedingly pungent odour; but finally a chloride is obtained having the formula

$$Sn\,C_4\,H_5\,Cl.$$

This salt produces fine, hard crystals, which are soluble in water, and, when pure, almost inodorous. A more ready method of obtaining this chloride consists in adding the radical, drop by drop, to a layer of bromine covered with water, until the bromine is decolorized; the aqueous solution is then decomposed by potash, which precipitates oxide of stanethyl in the form of a white powder, from which the pure salts of stanethyl may be readily procured.

The solubility of these salts in aqueous potash has been rather variously stated by Löwig and Frankland, and also their characters as odorous and inodorous. The truth is, that unless the salts of stanethyl are formed from the oxide, they are almost always contaminated with the above-mentioned oily chloride, the oxide of which is soluble in potash. As oxide of stanethyl is not affected by alkaline solutions, these two bodies may be separated without difficulty.

The soluble oxide may be recovered from the alkaline solutions by distillation. It passes over, together with aqueous vapour, in the form of an exceedingly caustic and pungent oil, which blues litmus, and has all the characters of a powerful base. Water dissolves it in moderate quantities, but precipitates it again on the addition of common salt. When deprived of water, the oily base solidifies into a crystalline mass.

This oxyde forms definite salts with acids, all more or less pungent. With hydrochloric and hydriodic acids, uncrystallizable bodies,

insoluble in water, are produced, but with sulphuric acid it forms fine, colourless crystals, which by analysis gave the formula

$$Sn_2\,C_{12}\,H_{15}\,SO_4 \text{ or } Sn_2\,(C_4\,H_5)_3\,SO_4.$$

For this compound I propose the name of sulphate of distannic triethyl. It has, in a remarkable degree, the unusual property of being more soluble in cold than in hot water. A cold saturated solution becomes semi-solid by raising the temperature somewhat below ebullition.

A consideration of the elements of the above formula furnished an idea of these bodies being either double salts, compounded of one equivalent of stannic diethyl with one equivalent of any salt, $Sn\,C_4\,H_5\,X$, or else a combination of three equivalents of stannic diethyl, with one equivalent of an inorganic salt $Sn\,X_2$, resulting in two molecules of the sesqui-ethylated salt. Thus

$$3\,Sn\,(C_4\,H_5)_2 + Sn\,Cl_2 = 2\,Sn_2\,(C_4\,H_5)_3\,Cl.$$

Experiment proves that the former bodies mix, but do not combine chemically, at any moderate heat. The latter bodies, on the other hand, exhibit strong chemical action, and disengage great heat during combination.

Bichloride of tin forms an oily body with stannic diethyl, chiefly composed of chloride of distannic triethyl, which by treatment with potash may be made to furnish the corresponding salts without difficulty.

Iodide of distannic triethyl may often be found amongst the products of the action of tin on iodide of ethyl. It is very probably identical with the oil noticed by Riche and Cahours, and described by them as possessing the pungent odour of oil of mustard.

These salts also must be considered to be identical with those described by Löwig under the somewhat inappropriate name of "methylo-stanethyls." The present name is suggested as more in accordance with their true constitution. They finally pass, by the action of zincethyl, into the radical stannic diethyl.

The presence of iodide of distannic triethyl amongst the stannic bodies in Löwig's experiments can be satisfactorily accounted for, by presuming the incomplete reduction of the iodides by the alloy of tin and sodium, employed in the reactions.

The behaviour of zincethyl towards the chlorides of tin may be

expressed, step by step, by the following equations, the tin-salt being supposed to be added to the zincethyl :—

$$\begin{array}{rl} \text{I.} & 2Zn\,C_4\,H_5 + Sn\,Cl_2 = Sn\,(C_4\,H_5)_2 + 2Zn\,Cl \\ \text{II.} & 3Zn\,C_4\,H_5 + 2Sn\,Cl_2 = Sn_2\,(C_4\,H_5)_3\,Cl + 3Zn\,Cl \\ \text{III.} & Zn\,C_4\,H_5 + Sn\,Cl_2 = Sn\,C_4\,H_5\,Cl + Zn\,Cl\text{; also} \\ \text{IV.} & 2Zn\,C_4\,H_5 + Sn\,Cl = \underbrace{Sn\,C_4\,H_5 + Zn\,C_4\,H_5}_{\text{Double compound ?}} + Zn\,Cl. \end{array}$$

I have failed in satisfactorily separating the radical stannic ethyl from the excess of zincethyl, as represented in the last reaction. By the addition of water great heat is generated, and tin is thrown down in its metallic state.

By distillation also, the radical stannic ethyl is similarly broken up,

$$2Sn\,C_4\,H_5 = Sn\,(C_4\,H_5)_2 + Sn.$$

Plumbic diethyl.

In the abstract above alluded to, I have stated the difficulties which at that time prevented my obtaining the lead radical in a state of purity. This difficulty arises from its tendency to decompose suddenly at a point below that of ebullition. This disadvantage is entirely obviated by conducting the distillation *in vacuo*, or at least under reduced atmospheric pressure. The organo-metal was found to distil unchanged under a pressure of 7·5 inches of mercury at a temperature of 152° C., the barometer at 30·5 inches. This is a remarkable lowering of the boiling-point, which at ordinary atmospheric pressures appears to be a few degrees above 200° C.

Analysis gave numbers leading to the formula—

$$Pb\,C_8\,H_{10} \text{ or } Pb\,(C_4\,H_5)_2.$$

Plumbic diethyl is a limpid and colourless fluid, possessing a specific gravity of 1·62. It burns with an orange flame, tinged at the edges with pale green, and disengages whilst burning much oxide of lead.

The only salts hitherto prepared from this radical seemed formed on the type of the sesquioxides. By passing excess of hydrochloric-acid gas over the organo-metal, hydride of ethyl is liberated, and chloride of diplumbic triethyl is obtained.

$$2Pb\,(C_4\,H_5)_2 + H\,Cl = Pb_2\,(C_4\,H_5)_3\,Cl + C_4\,H_5\,H.$$

The chloride is a fine crystalline body, occurring in long needles,

which fuse at a gentle heat, and then take fire, with the characteristic lead flame.

Oxide of diplumbic triethyl may be obtained by heating any of the corresponding salts with strong potash, or by acting on a solution of the chloride with oxide of silver. It is a crystalline body, which fuses into an oil-like liquid, at a gentle heat.

Sulphuric acid forms an abundant crop of asbestos-like needles when mixed with a warm solution of the chloride of diplumbic triethyl. It may also be obtained by neutralizing a solution of the oxide, and also by the action of sulphate of silver on the chloride.

Analysis furnished numbers which pointed to the formula

$$Pb_2\,C_{12}\,H_{15}\,SO_4 \text{ or } Pb_2\,(C_4\,H_5)_3\,SO_4.$$

All the salts of this sesqui-ethylated base are volatile, and their vapours attack the eyes and mucous membrane of the throat. In this respect they imitate their homologues in the stannic series.

In concluding this short abstract, I will only express my belief that a wide field of research is still open for inquiry, and that some promising experiments are at present in hand, from the right understanding of which we may hope to throw additional light on these interesting substances.

IV. "On Muscular Action from an electrical point of view." By CHARLES BLAND RADCLIFFE, M.D., F.R.C.P., Physician to the Westminster Hospital, &c. Communicated by JAMES PAGET, Esq. Received February 6, 1859.

This Paper was read in part.

March 17, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The reading of Dr. RADCLIFFE's Paper, "On Muscular Action from an electrical point of view," was resumed and concluded.

(Abstract.)

The author begins by observing, that the signs of electrical action in living muscle die out *pari passu* with the signs of irritability; and, as with these latter signs, *their last trace has disappeared before the occurrence of rigor mortis.*

It would appear, also, (in so far as electrical action is concerned) that there is a close agreement between *ordinary muscular contraction* and *rigor mortis*, for *in ordinary muscular contraction, as Prof. Du Bois Reymond has so well shown, there is a partial disappearance of electrical action.* Professor Matteucci, however, is doubtful as to this, and he maintains, on the contrary, that at this time the "muscular current" is sometimes reversed, and sometimes increased in intensity without being reversed.

In his recent experiments Prof. Matteucci uses a galvanometer of which the ends are so arranged as to get rid of the disturbing influences of secondary polarity. Instead of being of platinum immersed in a saturated solution of common salt, as in Prof. Du Bois Reymond's arrangement, these ends are of amalgamated zinc immersed in saturated solution of neutral sulphate of zinc. This arrangement, originally proposed by Dr. Jules Regnault, the author agrees with Prof. Matteucci in regarding as a great improvement upon that used by Prof. Du Bois Reymond; for, says he, "not only is the disturbing influence of secondary polarity got rid of, but the entrance of currents into the coil of the galvanometer is greatly facilitated. Of this I am satisfied after many comparative trials *."

In the experiment in which Prof. Matteucci finds what he considers to be the proof of the reversal of the muscular current during contraction, he takes a prepared frog's thigh with a long portion of nerve attached, and watches the changes of the muscular current

* The galvanometer used by Dr. Radcliffe was made by Mr. Becker, then of Newman Court, after the pattern of the one used by Prof. Du Bois Reymond. The gauge of the wire forming the coil is No. 38, or as nearly as possible that of the pattern coil; the weight of the wire entering into the coil 1lb. 11oz., the layers of the coil 154, the number of coilings 20,020, and upwards of three English miles. The needles are cylindrical, with each end sharpened out into a long point, and the connecting piece, instead of being made of tortoiseshell, as in Du Bois Reymond's instrument, is made of aluminium—a difference by which the astatic system becomes a little lighter, namely, 4·5 grains instead of 4·9 grains. In the first instance, Dr. Radcliffe used electrodes consisting of a pair of platinum plates immersed in a saturated solution of common salt (an arrangement recommended by Du Bois Reymond); afterwards he used the electrodes recommended by Dr. Jules Regnault, and adopted by Prof. Matteucci—electrodes consisting of a pair of amalgamated zinc plates immersed in a saturated solution of neutral sulphate of zinc.

which is derived from two points of the uncut surface. On laying the thigh upon the cushions which form the electrodes of the galvanometer, the needle diverges under the current of the relaxed muscle; on producing contraction by irritating the nerve with a feeble interrupted current, the needle immediately travels back and passes to the other side of zero. The fact is undeniable, but, according to the author, the backward movement of the needle does not indicate, as Prof. Matteucci supposes, a reversal of the muscular current during contraction. There is, it is true, no secondary polarity in the galvanometer to drive the needle back, as in the case where platinum electrodes are used; but there may be a tendency to oscillate backwards, and the question is whether the mere movement of oscillation may not be sufficient to account for the phenomena. What must be done, then, is to compare the rate at which the needle moves backward during contraction with the rate at which the needle falls backward in simple oscillation; and when this is done, the author finds that the needle moves backward *more slowly* during contraction than it does when it is simply left to oscillate in the same direction. It is found, indeed, that there is no reverse current during contraction, for if there were, the impulse of this current would be added to the impulse of oscillation, and (as is the case where the platinum ends are used) the needle would go backwards *more quickly* during contraction than it does when left to fall backwards from the same point under the influence of simple oscillation. There is also another way of showing the non-existence of a reverse current during muscular contraction, namely, by modifying the experiment in the way which Prof. Du Bois Reymond employs to get rid of the secondary polarity of the platinum ends. The only difference between the experiment as modified and the original experiment is this—that the wire of one of the electrodes is broken, and the broken ends are connected by being dipped into a small cup of mercury. An arrangement is thus made by which the circuit may be easily broken and closed again. In performing the modified experiment, the degree and direction of the current of the relaxed muscle is first observed. Then the circuit is broken by removing the end of the divided electrode out of the cup of mercury, and the needle is allowed to return to zero. In the next place, the muscle is tetanized, *and while in this state*, it is included in the circuit of the galvanometer by replacing the end

of the divided electrode in the mercury. The result is simply this, that the needle moves in the same direction as that in which it moved under the current of the relaxed muscle, but not to the same distance from zero. In other words, the muscular current is weakened but not changed in direction when the muscle passes into the state of contraction.

In the experiment in which Prof. Matteucci sees an intensification of the muscular current during contraction without any change in direction, he takes a prepared frog, and after preserving a sufficiently long portion of the sciatic nerve, he amputates the thigh above the middle joint. Then, taking the lower portion of the amputated thigh with the nerve attached to it, and placing the cut surface against an electrode of the galvanometer and the uncut surface against the other electrode, he watches the needle as it diverges under the current of the relaxed muscle. After this, he brings about a state of contraction in the muscle, by irritating the nerve with a feeble interrupted current, and, looking at the needle, *he sees it move in the same direction as that in which it had already moved under the current of the relaxed muscle.* That is to say, the needle shows, not weakening or change of direction, but actual intensification. On repeating this experiment, the author finds that it is most difficult to draw any safe conclusion from it; for in a thigh prepared in this manner it is almost impossible to keep the same point of the cut transverse surface in steady opposition to the electrode. Indeed, the necessary effect of contraction is to draw away the cut end from the electrode, and in this way to interrupt the entrance of the current of the contracted muscle into the circuit of the galvanometer. The effect of contraction, moreover, is often to bring upon the electrode portions of muscle which have not entered into the state of contraction, and in this way the only current which finds admission into the galvanometer may be that which is derived from *relaxed muscle*. This is often the case, and hence the apparent intensification of the muscular current during contraction, which is now and then witnessed in this experiment (it is not always witnessed), *may*, after all, be due to the irruption of additional quantities of the current of the relaxed muscle into the galvanometer. At any rate, the experiment is one from which it is most difficult to draw any certain conclusions.

In ordinary muscular contraction, then, there is good reason to believe that the muscular current is *enfeebled*—enfeebled to a degree approaching very closely to extinction ; and in *rigor mortis* all traces of muscular current have disappeared. It appears, indeed, as if muscular contraction were antagonized by the muscular current.

In tracing out the history of muscular action from an electrical point of view, the author proceeds, in the next place, to consider the mode in which the muscular current is affected by the nerve-current. In doing this, after describing the peculiarities of the nerve-current, and relating a beautiful experiment of Prof. Du Bois Reymond, in which it is seen that the nerve-current agrees with the muscular current in exhibiting a positive loss of force during muscular contraction; he interprets the reactions which must take place between the nerve-current and the muscular current by appealing to the history of the electrical organ of the torpedo and its congeners. The interpretation is that the reactions during muscular contraction are, not between the *primary* nerve-current and the muscular curtent, but between the muscular current and the *secondary or induced currents* which may be supposed to spring into existence when the primary or inducing nerve-current is suspended or renewed. The fact that the nerve-current sinks during contraction, is appealed to as an argument that the primary nerve-current is actually suspended and renewed during muscular contraction, and that in this manner the occasions for the appearance of the secondary or induced currents are thus properly provided for. It is pointed out that the reactions between the uninterrupted nerve-current and the muscular current, and between the muscular current and the induced or secondary currents which come into play when the primary or inducing nerve-current is interrupted or renewed, must be altogether different. With respect to the reactions which take place between the uninterrupted nerve-current and the muscular current, there is reason to believe that these must result in mutual intensification, for the nerve-current and the muscular current pass in the same direction. At any rate this is the case in the hind-limbs of the frog, or the fore-limbs of the same animal, and in the hind-limbs of the rabbit, dog, cat, and mouse. With respect to the reactions which take place between the muscular current and the secondary currents which come into play when the primary or inducing nerve-current is suspended or renewed,

there is every reason to believe that the result is altogether different. In this case, it appears as if the secondary or induced current must involve, not the intensification, but the *discharge* of the muscular current in all the muscle which enters into the circuit of the secondary current. For what is the peculiarity of the secondary current? It is a current of momentary duration, disappearing almost in the very instant of its appearing, and carrying along with it in its discharge any electricity it may meet with in its circuit. Hence there is no difficulty in understanding why the galvanometer should afford evidence of abatement of the muscular current at the moment when the nerves are concerned in producing muscular contraction. Nor is there any difficulty in understanding how contraction should be brought about by this action of the nerves, if, as there has seemed some reason to believe, muscular contraction is antagonized by the presence of the muscular and nerve-currents.

The author proceeds, in the next place, to consider the phenomena which attend upon the action of the ordinary galvanic current upon the muscular current. In this part it is pointed out that there is the same broad line to be drawn between the effects of the primary galvanic current and of the secondary currents which spring into existence when the inducing or primary current is suspended or renewed, and by keeping this distinction in mind it is shown that an intelligible physical reason may be obtained for the differences of the "direct" and "inverse" currents for "voltaic alternatives," and so on. The argument is complicated and not easily reducible to a few words; it requires, moreover, certain diagrams which cannot be used in this abstract, and therefore we will only say that the conclusion to which it leads, is that there must be a distinct annihilation of the muscular current during muscular contraction when the muscle contracts under the galvanic current, and that the contraction is seen to be most marked when this annihilation is most complete.

"Reflecting upon these facts and considerations," the author continues, "there appears to be nothing in the history of ordinary muscular contraction which does not harmonize with the electrical history of *rigor mortis*. If the muscular current be present, *rigor mortis* is absent, and in this case it seems as if the state of muscular contraction is antagonized by the muscular current. In ordinary muscular

contraction, to all appearance, it is the same, and when the muscle is relaxed the muscular or the artificial current is present, and when the muscle is contracted the muscular current is weakened or annihilated. It seems, indeed, as if the direct effect of the uninterrupted current, whether natural or artificial, is to antagonize contraction; and that this is really the case may be argued finally from the fact (recently discovered by Dr. Eckhard) *that the state of tetanus is put an end to by the passage of a constant galvanic current through the tetanized parts.*

"Nor is there any reason to suppose that the contraction is produced by a kind of correlative transmutation of electricity into contractile force. In *rigor mortis* such an idea is scarcely tenable, for here the muscular current has died out *slowly*, and the contraction has not supervened until the last traces have disappeared. In ordinary contraction, it might be supposed that there had been some transmutation of the muscular current into contractile force, or that an electric discharge had served as a stimulus to some vital property of contractility. But this idea is contradicted (this among other ways) by the recent investigations of Dr. Harley upon the *modus operandi* of strychnia. These investigations prove conclusively that this poison acts by making the blood less able to appropriate oxygen, and by impairing the irritability of the muscles. They prove, that is to say, that strychnia produces contraction, by reducing the amount of stimulus supplied in the blood, and by rendering the muscles less capable of responding to any stimulus. I find also that strychnia exercises a directly depressing influence upon the nervous and muscular currents. I place the two hind limbs of the same frog, properly prepared, one in a weak solution of strychnia, the other in plain water, and, leaving them to themselves, I find that the nerve and muscular currents have died out much sooner in the limb which has been acted upon by the strychnia. Now, in this case, the limbs have been left to themselves, and it cannot be said therefore that the nerve and muscular currents have been changed into contractile force by any kind of correlative transmutation. Indeed, the facts would only seem to be intelligible on the supposition that, electrically considered, the strychnia has brought about contraction according to the mode which has been set forth in the preceding pages. Nor on this view is the fact less intelligible, that

the respiration of muscle is carried on more energetically in muscles which are made to contract than in muscles which are allowed to rest. Prof. Matteucci, who has recently ratified this fact by some very elaborate investigations, holds that the chemical actions of muscular respiration are transformed into electricity, and the electrical into contractile force; but there is just as good reason for supposing that the increased chemical action may be required to keep up the muscular current, which current is being continually annihilated by the actions which bring about contraction. And thus, after all, the increased respiration of muscles which are made to contract, may refer, not to the contraction, but to the renewal of the state of relaxation. At any rate, it is scarcely possible to refer to this fact as an objection to the view which is set forth in this paper.

"Regarded in an electrical point of view then, there appears to be good reason for concluding that the history of muscular action is in harmony with the theory which I have endeavoured to set forth at various times, and more recently in the second edition of a work having for its title, 'Epilepsy and other Convulsive Affections, their Pathology and Treatment:'—a theory, according to which, in every case, pathological as well as physiological, muscular contraction is produced, *not* by the stimulation of any vital property of contractility belonging to muscle, but by the simple cessation of the action of certain agents—electricity, nervous influence, and others, which had previously kept the muscle in a state of relaxation or expansion."

The following communications were also read:—

I. "On the Action of Carbonic Oxide on Sodium-alcohol." By J. A. Wanklyn, Esq. Communicated by Professor E. Frankland. Received February 15, 1859.

Dr. Geuther* found that sodium-alcohol $\left(\begin{matrix} C_4 H_5 \\ Na \end{matrix}\right\} \Big) O_2$ when gently warmed in a stream of carbonic oxide, yielded not pre-

* Annalen der Chem. und Pharm. Jan. 1859.

pionate of soda, but formiate of soda, with evolution of olefiant gas. The reaction, accordingly, might be represented thus:—

$$\left.\begin{matrix}C_4H_5\\Na\end{matrix}\right\}O_2+C_2O_2=\left.\begin{matrix}C_2O_2H\\Na\end{matrix}\right\}O_2+C_4H_4,$$

and would consist in the replacement of C_4H_4 by C_2O_2.

On inspection of Dr. Geuther's paper it appeared that the above reaction was not established with sufficient certainty. The presence of C_4H_4 as a gaseous product was not satisfactorily proved by direct experiment, but inferred from the production of formiate of soda.

Berthelot has shown that carbonic oxide is capable of uniting with the hydrated alkalies, so as to form alkaline formiates. Also, it is extremely difficult, and perhaps impossible, to obtain sodium-alcohol free from hydrate of soda. It seemed, therefore, not unreasonable to suspect that Dr. Geuther's formiate came from hydrate of soda accompanying the sodium-alcohol employed in his experiments. The investigation about to be described shows that such was really the case.

Sodium-alcohol, freshly prepared from sodium and anhydrous alcohol, was introduced into small glass bulbs, and hermetically sealed therein. One of the bulbs, containing ·406 gramme of crystallized sodium-alcohol, was placed in a flask of 155 cubic centimetres' capacity. The neck of the flask was contracted before the blowpipe. Carbonic oxide, after slow passage through potash solution, and then through sulphuric acid, was next made to fill the flask by displacement. Finally, the contracted neck of the flask was closed by fusion, and thus the bulb containing sodium-alcohol was inclosed in an atmosphere of pure oxide of carbon. By agitation the inclosed bulb was broken, and its contents came freely in contact with the carbonic oxide contained in the flask. Particular attention was paid during this stage of the process, and the fused sodium-alcohol was seen flowing over the inner surface of the flask.

After a digestion in the water-bath lasting for more than four hours, the flask was opened under mercury, when a slight contraction was observed in the volume of its gaseous contents. This contraction, amounting to about one-fifth of the entire contents, was due no doubt partly to absorption of carbonic oxide by traces of hydrate of soda, and partly to the difference between the temperature at the

time of sealing before the blowpipe, and that at the time of opening under mercury.

The following are the particulars of an examination of the gas contained in the flask after the four hours' digestion at 100° C.

In order to remove any alcohol vapour, the gas was agitated with about one-fifth of its volume of boiled distilled water, when it underwent very little diminution in volume—a circumstance, which shows that no volatile liquid capable of absorption by water had been generated during the reaction.

Some of the washed gas was then treated with a potash bullet and with pyrogallic acid, in order to remove any traces of carbonic acid and oxygen. The amount of these gases present was very trifling, as the readings show:—

Volume of gas taken (corrected (dry) at 0° C. and 1000 millims'. pressure)	65·091
Volume of gas after potash and pyrogallic acid (corrected (dry) at 0° C. and 1000 millims'. pressure)	64·734

After this treatment a portion of the gas was transferred to the eudiometer, in which it furnished the following readings:—

	Volumes.	Temperature. Cent.	Pressure.	Corrected volumes dry at 0° C and 1000 millims'. pressure.
		°	millims.	
Gas taken (moist)	148·0	4·9	202·9	29·500
After the addition of air (moist)	306·5	5·3	352·8	106·077
After explosion (moist)	282·1	5·7	330·6	91·357
After potash (dry)	222·1	5·4	290·0	63·161
After the addition of hydrogen (dry)	294·0	5·7	357·1	102·884
After explosion (dry)	286·4	5·9	350·1	98·150

From which is deduced:—

		In per-centage.
Gas taken	29·500	
Nitrogen	1·072	
Gas free from nitrogen	28·428	100·00
Carbonic acid	28·196	99·18
Contraction	14·720	51·78
Oxygen consumed	14·488	50·96

The theoretical numbers for carbonic oxide and for olefiant gas are as follows :—

	Carbonic oxide.	Olefiant gas.
Volume taken	100	100
Carbonic acid	100	200
Contraction	50	200
Oxygen consumed	50	300

Comparison of the analysis with these numbers will show that the gas was pure oxide of carbon. Furthermore, if we assume that the trifling departure from the theoretical quantities for pure oxide of carbon was due to the presence of olefiant gas, and if we calculate how much olefiant gas would be required, we obtain a negative value for the quantity of olefiant gas from one equation, and a positive one from the other equation, viz. :—

By employing for data the original volume and the carbonic acid generated, the value of $C_4 H_4$ is negative.

$$\text{Vol. of } C_4 H_4 = \text{vol. of } CO_2 - \text{original vol.} = -0{\cdot}82 \text{ per cent.}$$

By employing for data the original volume and the contraction, the value of $C_4 H_4$ becomes positive.

$$\text{Vol. of } C_4 H_4 = \tfrac{2}{3} \text{ contraction} - \tfrac{1}{3} \text{ original vol.} = 1{\cdot}19 \text{ per cent.}$$

This want of conformity shows that $C_4 H_4$ will not satisfy the conditions of the case, and may be regarded as excluding the supposition that a trace even of $C_4 H_4$ was present in the gas examined.

When it is considered that on Dr. Geuther's hypothesis every volume of carbonic acid absorbed should be replaced by an equal volume of olefiant gas, and when it is borne in mind that sodium-alcohol and carbonic oxide must have been less perfectly exposed to mutual action in Dr. Geuther's experiment than in the one just described, I think the conclusion cannot be avoided, that that experimenter's formic acid came not from sodium-alcohol, but from hydrate of soda.

In a previous experiment I failed to obtain propionic acid on exposing at 100° C. carbonic oxide along with sodium-alcohol, and in so far my result agrees with that of Dr. Geuther.

To resume: at 100° C. sodium-alcohol is without action on carbonic oxide.

This research was made in the laboratory of Prof. Bunsen.

II. Postscript to a Paper "On the Deflection of the Plumb-line in India, caused by the Attraction of the Himalayan Mountains." By the Venerable Archdeacon PRATT. Communicated by Professor STOKES, Sec. R.S. Received February 21, 1859.

(Abstract.)

Since transmitting his Paper "On the deflection of the Plumb-line in India caused by the attraction of the Himalaya Mountains*," the author has had the advantage of seeing the pages of Major R. Strachey's work on the physical geography of the Himalayas, now passing through the press; and being permitted to make use of them, he availed himself of the important information therein contained to add a postscript to his former communication.

Major Strachey thinks that none of the numerous ranges commonly marked on maps of Thibet, have any special definite existence as mountain chains, apart from the general mass of the table-land; and that this country should not be considered to be as if in the interval between the two so-called chains of the Himalaya and Kouenlun, but that it is in reality the summit of a great protuberance, above the general level of the earth's surface, of which the supposed Kouenlun and Himalaya are nothing more than the north and south faces, while the other ranges are but corrugations of the table-land more or less marked. The plains of India which skirt the foot of the table-land, to an extent of 1500 miles, nowhere have an elevation exceeding 1200 feet above the sea, the average being much less; and there is reason to think that the northern plateau of Yarkend and Khotan, like the country about Bukhara, lies at a very small elevation, probably not more than 1000 or 2000 feet above the sea, while on the borders of the Caspian the surface descends below the sea-level.

On comparing the information contained in Major Strachey's work with the assumptions as to height above the sea-level, adopted by the author in his former paper from the best data then in his possession, it appears that the mean height of the table-land was considerably under-estimated; while on the other hand attracting masses were supposed to lie still further north which really have no existence.

* Proc. Roy. Soc. vol. ix. p. 493.

The author has not recomputed the total attraction of all the disturbing masses, but has merely given in this postscript a computation of the meridional component of the attraction of the table-land *alone*. In accordance with the new information, the height of the table-land is assumed to be uniform, and to be equal to $2\frac{2}{3}$ miles above the level of Kaliana, that is, about 15,000 feet above the sea-level. The resulting northern deflections are as follows:—

At Kaliana	$19''\cdot85$
At Kalianpur	$10''\cdot28$
At Damargida	$4''\cdot29$

Hence the errors produced in the astronomical amplitudes will be $9''\cdot57$ and $5''\cdot99$, which much exceed the errors ($5''\cdot236$ and $-3''\cdot791$) brought to light by the survey, on the assumption that the ellipticity of the Indian arc is the mean ellipticity of the whole earth; and the discrepancy will be still further increased when the attraction of the nearer masses is also taken into account.

The new information regarding the nature of the country north of the Himalayas does not, it thus appears, relieve the subject of its difficulties; and no geodetic calculations can be of service in the problem of the figure of the earth, nor indeed in mapping the country with extreme precision, till these perplexities are removed by the deflection being found and allowed for.

March 24th, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "On the Conic of Five-pointic Contact at any point of a Plane Curve." By A. Cayley, Esq., F.R.S. Received March 1, 1859.

(Abstract.)

The tangent is a line passing through two consecutive points of a plane curve, and we may in like manner consider the conic which passes through five consecutive points of a plane curve; and as there are certain singular points, viz. the points of inflexion, where three consecutive points of the curve lie in a line, so there are singular

points where six consecutive points of the curve lie in a conic. In the particular case where the given curve is a cubic, the last-mentioned species of singular points have been considered by Plücker and Steiner, and in the same particular case, the theory of the conic of five-pointic contact has recently been established by Mr. Salmon. But the general case, where the curve is of any order whatever, has not, so far as I am aware, been hitherto considered;—the establishment of this theory is the object of the present memoir.

II. "On the Vertebral Characters of the Order Pterosauria (Ow.) as exemplified in the Genera *Pterodactylus* (Cuv.) and *Dimorphodon* (Ow.)." By Professor OWEN, F.R.S. &c. Received February 23, 1859.

(Abstract.)

After mentioning various considerations which have tended to invest the question of the vertebral characters of the Pterodactyles with peculiar interest; above all, in reference to carrying out the comparison of their skeleton with that of birds; the author alludes to the scanty information on the subject already on record, which—with the exception of a remark of Professor Quensted as to the apparently procœlian characters observed by him in a dorsal vertebra of *Pterodactylus Suevicus*, and the apparent want of the trochlear form in the cervical articulations of that animal—affords no available data for comparing the vertebral mechanism of these reptiles with that of other vertebrata adapted for flight; he then gives a summary of his own observations, made, as opportunities presented themselves, for some years past.

From investigations of species of *Pterosauria* extending from the period of the Lias, as exemplified by the *Dimorphodon macronyx*, to the upper green-sand, as exemplified by the *Pterodactylus Sedgwickii* and *Pter. Fittoni*, the author has ascertained the fact, that, with respect to the cervical and dorso-lumbar vertebræ, the terminal articular surfaces of the vertebral bodies are simply concave anteriorly and convex posteriorly, and that they consequently manifest the earliest known instance of the "procœlian" type which now prevails in the reptilian class. But in no other reptile are those arti-

cular surfaces so narrow vertically, in proportion to their breadth, as they are in the cervical vertebræ of the *Pterosauria*: in the dorsal series the cup and ball present more ordinary Saurian proportions.

Besides these principal and more general characters, those also which distinguish the vertebræ of the several regions of the spine, together with the specialities of the atlas and axis, and of other individual vertebræ, are pointed out and described.

The Paper is illustrated by numerous figures, which (excepting two from the *Aptenodytes*) belong to the Pterodactyle.

March 31, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "The Higher Theory of Elliptic Integrals, treated from Jacobi's Functions as its basis." By F. W. NEWMAN, Esq., M.A., Professor of Latin in University College, London. Communicated by the Rev. Dr. BOOTH. Received March 3, 1859.

(Abstract.)

The peculiarly beautiful properties of these integrals, as treated by Jacobi and (in his two supplements) by Legendre, are obtained through so very elaborate and difficult a process, that few students can afford the time to study them. Professor De Morgan, in his 'Integral Calculus,' declines to enter even the Lower Theory, on the ground that the subject requires a detailed treatise. That in some sense it is analogous to trigonometry, which no one would desire to be treated fully in the differential and integral calculus, has been recognized by several writers. Legendre, in his second supplement, sixth section, took the first steps toward treating Jacobi's functions (Λ and Θ) on a wholly independent basis, by investigating their properties from the series which they represent: but after only two pages of this sort, he aids his research by assuming their relations

to elliptic integrals as already established, which shows that he was not seeking for a new basis of argument, but only for new properties. The author of the present paper proposes (for didactic purposes) to commence the higher theory from these functions. The first division of his essay is purely algebraic and trigonometrical, not introducing the idea of elliptic integrals at all. Adopting as the definition of the functions Λ and Θ the two equations

$$\left.\begin{array}{l}\Lambda(q,x)=2q^{\frac{1}{4}}(\sin x-q^{1.2}\sin 3x+q^{2.3}\sin 5x-\&c.)\\ \Theta(q,x)=1-2q^{1.1}\cos 2x+2q^{2.2}\cos 4x-2q^{3.3}\cos 6x+\&c.\end{array}\right\},$$

it demonstrates by direct algebraic methods many properties of great generality, of which we shall here specify—

1. If $\surd b$ stands for $\dfrac{\Theta(q,0)}{\Theta(q,\frac{1}{2}\pi)}$ and $\surd c$ for $\dfrac{\Lambda(q,\frac{1}{2}\pi)}{\Theta(q,\frac{1}{2}\pi)}$, which is shown to yield $b^2+c^2=1$; and if, further, Λ^0 Θ^0 stand for $\Lambda(q,x+\frac{1}{2}\pi)$, $\Theta(q,x+\frac{1}{2}\pi)$; we get the four equations (equivalent to two only)

$$\begin{array}{ll}\Lambda^2+b\Lambda^{02}=c\Theta^2; & \Lambda^{02}+b\Lambda^2=c\Theta^{02};\\ \Theta^2-b\Theta^{02}=c\Lambda^2; & \Theta^{02}-b\Theta^2=c\Lambda^{02};\end{array}$$

from which it directly follows, that if ω is an arc defined by the equation $\surd b\tan\omega=\dfrac{\Lambda}{\Lambda^0}$, we shall have simultaneously $\surd c\sin\omega=\dfrac{\Lambda}{\Theta}$; $\surd c\cos\omega=\surd b\dfrac{\Lambda^0}{\Theta}$; $\surd(1-c^2\sin^2\omega)=\surd b\dfrac{\Theta^0}{\Theta}$. The symbol $\Delta(c,\omega)$, $\Delta(\omega)$ or Δ represents $\surd(1-c^2\sin^2\omega)$ in this theory.

2. It is further shown that

$$\Lambda^0\frac{d\Lambda}{dx}-\Lambda\frac{d\Lambda^0}{dx}=\Lambda^2(\tfrac{1}{2}\pi).\Theta\Theta^0;$$

whence is easily obtained

$$\frac{d\omega}{dx}\propto\Delta(c,\omega).\quad\text{Also }\Theta^0\frac{d\Theta}{dx}-\Theta\frac{d\Theta^0}{dx}=\Lambda^2(\tfrac{1}{2}\pi).\Lambda\Lambda^0.$$

3. By direct multiplication of two trigonometrical series, it is found that

$$\begin{aligned}\Lambda(q,x)\Lambda^0(q,y)&=\Lambda(q^2,x-y).\Theta(q^2,x+y)\\&\quad+\Theta(q^2,x-y).\Lambda(q^2,x+y);\\ \Theta(q,x)\Theta^0(q,y)&=\Lambda(q^2,x-y).\Lambda(q^2,x+y)\\&\quad+\Theta(q^2,x-y).\Theta(q^2,x+y).\end{aligned}$$

From the property marked (2) we obtain the connexion of the func-

tions Λ, Θ with elliptic integrals. For, if $F(c, \omega)$, as usual, stands for

$$\int_0 \frac{d\omega}{\sqrt{(1-c^2 \sin^2 \omega)}},$$

it yields

$$F(c, \omega) \propto x\,; \text{ or } \frac{F(c, \omega)}{F(c, \frac{1}{2}\pi)} = \frac{x}{\frac{1}{2}\pi}.$$

This introduces the second and principal part of the essay. An easy inference from (3) is, that

$$\frac{\Lambda(q^2, x+y)}{\Theta(q^2, x+y)} = \frac{\Lambda x \Lambda^0 y + \Lambda y \Lambda^0 x}{\Theta x \Theta^0 y + \Theta y \Theta^0 x};$$

and consequently that if η is related to q^2 and to $x+y$ by the same law as ω is to q and to x, while $c_{\prime}$ is to q^2 what c is to q, we obtain

4. $$\sqrt{c_{\prime}} \sin \eta = \frac{c \sin(\omega+\theta)}{\Delta(c, \omega) + \Delta(c, \theta)},$$

when $$\frac{F(c_{\prime}, \eta)}{F(c_{\prime}, \frac{1}{2}\pi)} = \frac{F(c, \omega) + F(c, \theta)}{F(c, \frac{1}{2}\pi)}.$$

This formula has the peculiarity of comprising Euler's integrals, with the integrations of Lagrange and of Gauss; namely, if $\omega=\theta$, we get the scale of Lagrange. If $\theta=0$, the scale of Gauss is obtained. But if we introduce a new variable ζ, such that

$$F(c, \zeta) = F(c, \omega) + F(c, \theta),$$

we eliminate η by aid of the last result

$$\left(\text{which is } \sqrt{c_{\prime}} \sin \eta = \frac{c \sin \zeta}{\Delta(c, \zeta)+1}\right),$$

and obtain

$$\sqrt{\frac{1-\Delta\zeta}{1+\Delta\zeta}} = \frac{c \sin(\omega+\theta)}{\Delta\omega + \Delta\theta};$$

which is equivalent to Euler's integration.

The author believes this generalization to be new.

5. He proceeds (assuming now the theory of Lagrange's scale) to prove the higher theorems by much simpler processes. E being the *second* elliptic integral, he writes G for $E - \frac{E_c}{F_c} F$, and V for $\int_0 G dF$, and out of the integration $\frac{1}{2} \log \Delta = \frac{1}{2} V_{\prime} - V$ (where $V_{\prime}$ is to q^2 and $2x$, what V is to q and x), he deduces

$$V = \log . \frac{\Theta(q, x)}{\Theta(q, 0)}$$

by a process fundamentally that of Legendre, Second Supplement, § 196. This is the equation by which E, and indirectly the third integral Π, is linked to the functions ΛΘ.

6. We may further point out, as perhaps new, the developments of ΛΘ in the case when q is very near to 1. Let r be related to b as q to c; then $\log\frac{1}{q}.\log\frac{1}{r}=\pi^2$. If $\log\frac{1}{q}=\pi a$, and $x=\pi u$,

$$\left.\begin{aligned}\sqrt{a}.\Theta(q,\pi u+\tfrac{1}{2}\pi)&=r^{u^2}+r^{(1\pm u)^2}+r^{(2\pm u)^2}+r^{(3\pm u)^2}+\&c\ldots\\ \sqrt{a}.\Lambda(q,\pi u+\tfrac{1}{2}\pi)&=r^{u^2}-r^{(1\pm u)^2}+r^{(2\pm u)^2}-r^{(3\pm u)^2}+\&c\ldots\end{aligned}\right\}$$

in which the double sign denotes *two* terms, which must both be included. But besides, if the symbol $\phi(r)$ stand for

$$[(1-r^2)(1-r^4)(1-r^6)\ldots\&c.]^{-1},$$

$$\left.\begin{aligned}\sqrt{a}.\phi(r).\Theta(q,\pi u+\tfrac{1}{2}\pi)&=r^{u^2}.(1+r^{1\pm 2u})(1+r^{3\pm 2u})(1+r^{5\pm 2u})\ldots\\ \sqrt{a}.\phi(r).\Lambda(q,\pi u+\tfrac{1}{2}\pi)&=r^{u^2}.(1-r^{1\pm 2u})(1-r^{3\pm 2u})(1-r^{5\pm 2u})\ldots\end{aligned}\right\}.$$

From these formulæ not only all of Gudermann's developments for calculating elliptic integrals in every case are deducible, but others also, it seems, of a remarkable aspect, in the difficult case of q and c being extremely near to 1.

We produce the two which seem to be simplest. Let B be to b what C is to c, and Tan x represent $\frac{\operatorname{Sin} x}{\operatorname{Cos} x}$, where 2 Sin x stands for $\epsilon^x-\epsilon^{-x}$ and 2 Cos x for $\epsilon^x+\epsilon^{-x}$. Then when c is very near to 1, we compute G and thereby E from the series,

$$\begin{aligned}\mathrm{B}.\mathrm{G}(c,\omega)=\frac{\pi-2x}{\pi}&-\left(1-\operatorname{Tan}\frac{x}{a}\right)+\left(1-\operatorname{Tan}\frac{\pi-x}{a}\right)\\ &-\left(1-\operatorname{Tan}\frac{\pi+x}{a}\right)+\left(1-\operatorname{Tan}\frac{2\pi-x}{a}\right)\\ &-\left(1-\operatorname{Tan}\frac{2\pi+x}{a}\right)+\left(1-\operatorname{Tan}\frac{3\pi-x}{a}\right)-\&c\ldots\end{aligned}$$

The *third* elliptic integral is in the same case deduced from a series of the form

$$\begin{aligned}\frac{\pi-2x}{\pi}.j&-\left\{j-\tan^{-1}\left(\tan j.\operatorname{Tan}\frac{x}{a}\right)\right\}+\left\{j-\tan^{-1}\left(\tan j.\operatorname{Tan}\frac{\pi-x}{a}\right)\right\}\\ &-\left\{j-\tan^{-1}\left(\tan j.\operatorname{Tan}\frac{\pi+x}{a}\right)\right\}+\left\{j-\tan^{-1}\left(\tan j.\operatorname{Tan}\frac{2\pi-x}{a}\right)\right\}\\ &-\&c\ldots\end{aligned}$$

Finally, the essay developes above thirty series which rise out of this theory, nearly all of which are believed to be new. The most elegant of them may find a place here. Writing, for conciseness, C so related to c that $F(c, \frac{1}{2}\pi)=\frac{1}{2}\pi C$, and $\therefore F(c\,\omega)=Cx$, we have

$$(a).\ \omega=x+\frac{\sin 2x}{\operatorname{Cos}\pi a}+\tfrac{1}{2}.\frac{\sin 4x}{\operatorname{Cos}2\pi a}+\tfrac{1}{3}.\frac{\sin 6x}{\operatorname{Cos}3\pi a}+\&c.$$

$$(b).\ C\Delta(c, \omega)=1+\frac{2\cos 2x}{\operatorname{Cos}\pi a}+\frac{2\cos 4x}{\operatorname{Cos}2\pi a}+\frac{2\cos 6x}{\operatorname{Cos}3\pi a}+\&c.,$$

$$(c).\ \tfrac{1}{8}C^2c^2\sin 2\omega=\frac{\sin 2x}{\operatorname{Cos}\pi a}+\frac{2\sin 4x}{\operatorname{Cos}2\pi a}+\frac{3\sin 6x}{\operatorname{Cos}3\pi a}+\&c..$$

$$(d).\ V(c, \omega)=\frac{1-\cos 2x}{\sin \pi a}+\tfrac{1}{2}.\frac{1-\cos 4x}{\sin 2\pi a}+\tfrac{1}{3}.\frac{1-\cos 6x}{\sin 3\pi a}+\&c...$$

$$(e).\ \tfrac{1}{2}C.G(c, \omega)=\frac{\sin 2x}{\sin \pi a}+\frac{\sin 4x}{\sin 2\pi a}+\frac{\sin 6x}{\sin 3\pi a}+\&c....$$

This is virtually eq. 49 of Legendre's Second Supplement, § 7. In eq. 53 of the same, he has a development of $\sin^2\omega$, which is given by Mr. Newman in a notation similar to eq. (c) above.

$$(f).\ -\tfrac{1}{2}\log\Delta(c, \omega)=\frac{1-\cos 2x}{\operatorname{Sin}\pi a}+\tfrac{1}{3}.\frac{1-\cos 6x}{\operatorname{Sin}3\pi a}+\tfrac{1}{5}.\frac{1-\cos 10x}{\operatorname{Sin}5\pi a}+\&c...$$

$$(g).\ \tfrac{1}{2}Cc\sin\omega=\frac{\sin x}{\operatorname{Sin}\frac{1}{2}\pi a}+\frac{\sin 3x}{\operatorname{Sin}\frac{3}{2}\pi a}+\frac{\sin 5x}{\operatorname{Sin}\frac{5}{2}\pi a}+\&c....$$

$$(h).\ \tfrac{1}{2}Cc\cos\omega=\frac{\cos x}{\operatorname{Cos}\frac{1}{2}\pi a}+\frac{\cos 3x}{\operatorname{Cos}\frac{3}{2}\pi a}+\frac{\cos 5x}{\operatorname{Cos}\frac{5}{2}\pi a}+\&c....$$

$$(i).\ \tfrac{1}{4}Cc\sqrt{\frac{1-\Delta}{1+\Delta}}=\frac{\sin x}{\operatorname{Sin}\pi a}+\frac{\sin 3x}{\operatorname{Sin}3\pi a}+\frac{\sin 5x}{\operatorname{Sin}5\pi a}+\&c....$$

Moreover, Jacobi's two celebrated theorems follow as a corollary from the general propositions here established.

II. "On the Comparison of Hyperbolic Arcs." By C. W. Merrifield, Esq. Communicated by the Rev. Dr. Booth. Received March 3, 1859.

(Abstract.)

If in common trigonometry we take one arc equal to the sum of two others, the cosine of the first arc is equal to the product of the cosines, diminished by the product of the sines of the other two.

If we pass from the circle to the ellipse, the addition of the arcs becomes more complicated, the product of the sines being multiplied by a radical. This relation was first obtained by Euler as the integral of a differential equation. I have called it elsewhere, for convenience, *the elliptic* equation between three amplitudes. Moreover, we are no longer able to take the simple sum of the arcs, but we have to add in an algebraic quantity, which is a multiple of the products of the sines of the three amplitudes.

The comparison of hyperbolic arcs hitherto has been matter of still greater complexity, as it has been usually handled simply by reducing each hyperbolic arc to two elliptic arcs. The complexity, of course, is thus doubled.

It seemed to me, however, that the ellipse is so completely the analogue of the hyperbola, and that it is so easy to pass from one to the other by an imaginary transformation, that a similar analogy ought to pervade the comparison of their arcs. Hence that there must exist some formula for the comparison of hyperbolic arcs, as simple as that for elliptic arcs.

A slight modification of Jacobi's second theorem has enabled me to find the analogue which I required.

If we take Euler's elliptic equation, and substitute the following changes,—

cosine of amplitude into *secant of amplitude*,
sine of amplitude into *tangent amplitude* $\times \sqrt{-1}$,
sine of modulus into *cosine of modulus*,

we leave the equation entirely unaltered, except in form. Even this form may be obtained directly by a simple transformation, of purely algebraic character. Hence it follows that all the consequences of this imaginary transformation are allowable.

If we apply these transformations to the elliptic integral of the first kind, we have Jacobi's second theorem.

If we apply them to the integral of the second kind, which represents the elliptic arc, we pass, after an obvious reduction, to the arc of the hyperbola. The algebraic addition to the sum of the arcs is simply changed from a product of sines to a product of tangents.

Other considerations enable us to verify the theorem, when once it is obtained. These verifications are merely algebraic, and would scarcely be intelligible if read aloud.

I have made use of my formulæ in the reduction of one class of the elliptic integral of the third kind for the purposes of tabulation. In its ordinary form, this function has three variables, and, as Legendre justly remarks, a table of treble entry would be intolerable. If, therefore, it is to be tabulated at all, the first step in the question is to reduce it to a form involving two variables. By the help of some theorems of Jacobi, Legendre succeeded in effecting this where the elliptic function of the third kind has its parameter negative and less than the square of the modulus.

By the help of my formula, I have succeeded in reducing the case where the parameter is negative and greater than unity. The steps are the mere counterpart of Legendre's work in the second supplement of the treatise on Elliptic Functions.

Both Legendre's case and mine are of the logarithmic form, and can therefore be reduced to one another by algebraic transformation. The cases where the parameter is positive, or negative and intermediate between unity and the square of the modulus, are still unreduced. The difficulty is exactly analogous to that between the two cases of cubic equations, and this analogy is even carried into the very form of the solution.

Dr. Booth's application of the trigonometry of the parabola to the reducible case of the cubic equation, affords some hope that a correlative calculus may exist, particular cases of which may solve the cases now irreducible, just as the calculus of elliptic functions includes the trigonometry both of the parabola and the circle. My own investigations on this subject are still without any useful result.

Let

$$\cos\phi_1 = \cos\phi_2\cos\phi_3 - \sin\phi_2\sin\phi_3\sqrt{1-\sin^2\theta\sin^2\phi_1}\,;$$

$$F\varphi = \int\frac{d\phi}{(1-\sin^2\theta\sin^2\phi)^{\frac{1}{2}}},$$

$$E\phi = \int(1-\sin^2\theta\sin^2\phi)^{\frac{1}{2}}d\phi,$$

$$\Upsilon\phi = \int\frac{\cos^2\theta d\phi}{(1-\sin^2\theta\sin^2\phi)^{\frac{1}{2}}\cos^2\phi d\phi},$$

$$\left.\begin{array}{ll}(1) & F\phi_1 - F\phi_2 + F\phi_3 = 0 \\ (2) & E\phi_1 - E\phi_2 + E\phi_3 = \sin^2\theta\sin\phi_1\sin\phi_2\sin\phi_3\end{array}\right\}\text{Euler.}$$

$$(3)\quad \Upsilon\phi_1 - \Upsilon\phi_2 + \Upsilon\phi_3 = -\cos^2\theta\tan\phi_1\tan\phi_2\tan\phi_3.$$

III. "On the Oxidation of Glycol, and on some Salts of Glyoxylic Acid." By H. DEBUS, Ph.D. Communicated by Dr. TYNDALL. Received March 16, 1859.

(Abstract.)

If glycol be oxidized with nitric acid, according to Wurtz*, it is converted into glycolic and oxalic acids.

$$\underbrace{C_2H_6O_2}_{\text{Glycol.}} + O_2 = \underbrace{C_2H_4O_3}_{\text{Glycolic acid.}} + \underbrace{H_2O}_{\text{Water.}}$$

$$\underbrace{C_2H_6O_2}_{\text{Glycol.}} + O_4 = \underbrace{C_2H_2O_4}_{\text{Oxalic acid.}} + \underbrace{2H_2O}_{\text{Water.}}\dagger$$

As the formation of these acids from glycol is analogous to the production of acetic acid from ethylic alcohol, it may be assumed that the bibasic oxalic acid stands to the biatomic glycol in the same relation as the fatty acids do to their corresponding alcohols. Alcohol is not converted at once into acetic acid, an intermediate substance—aldehyde—being formed. The formation of a similar body from glycol is highly probable. If two or four atoms of hydrogen be removed from glycol, an aldehyde of the composition of acetic acid or of the formula $C_2H_2O_2$ may be produced. Glyoxal, obtained by the oxidation of ethylic alcohol, has indeed the composition corresponding to the above formula and the characteristic properties of an aldehyde. Nitric acid converts it rapidly into oxalic acid. Consequently glycol, glyoxal, and oxalic acid bear a similar relation to each other as ethylic alcohol, aldehyde, and acetic do.

$$\underbrace{C_2H_6O}_{\text{Alcohol.}} \qquad \underbrace{C_2H_4O}_{\text{Aldehyde.}} \qquad \underbrace{C_2H_4O_2}_{\text{Acetic acid.}}$$

$$\underbrace{C_2H_6O_2}_{\text{Glycol.}} \qquad \underbrace{C_2H_2O_2}_{\text{Glyoxal.}} \qquad \underbrace{C_2H_2O_4}_{\text{Oxalic acid.}}$$

Another connexion between glycol and glyoxal is that they both produce glycolic acid, the former by oxidation, and the latter by combi-

* Comp. Rend. xliv. 1306.

† $C=12, H=1, O=16$.

nation with one atom of water. I treated glycol, which was diluted with water, with fuming nitric acid at about 30° C., and evaporated the acid liquid, as soon as the action was completed, on the water-bath until it assumed the consistency of syrup*. This residue was found to contain, as previously shown by Wurtz, oxalic and glycolic acids; but I also found therein glyoxylic acid and a body which comported itself with some reagents like glyoxal. Want of material, however, prevented its being identified with the latter.

Before I proceed to make some observations on these facts, I shall first mention a few salts of glyoxylic acid not yet described.

Glyoxylate of silver, $C_2HAgO_3+H_2O$,

is obtained as a white crystalline powder when nitrate of silver is precipitated with glyoxylate of ammonia. This salt is but slightly soluble in cold water, and is decomposed by light with great rapidity.

Glyoxylate of baryta, $C_2HBaO_3+2H_2O$.

Diluted glyoxylic acid is digested at common temperatures with carbonate of baryta until the acid is completely neutralized, and the filtered solution evaporated *in vacuo*. As soon as the liquid has arrived at a certain degree of concentration, small white crystals of glyoxylate of baryta begin to separate. This compound is partially decomposed into oxalate of baryta and glycolic acid if it be heated to 120° C., or if the temperature of its watery solution be raised to the boiling-point. With nitrate of silver, acetate of lead, and lime-water, it comports itself like glyoxylate of lime.

Glyoxylate of zinc, $C_2Zn_2O_3+2H_2O$,

is produced as a white crystalline precipitate when a strong solution of glyoxylate of lime is precipitated with acetate of zinc. This compound is slightly soluble in water, but is easily dissolved by acetic and hydrochloric acids and by caustic potash. The two atoms of water cannot be removed without decomposing the salt.

Glyoxylate of ammonia, $C_2H(NH_4)O_3$.

This compound was prepared by precipitating glyoxylate of lime with its equivalent quantity of oxalate of ammonia, and evaporating

* "On the Action of Nitric Acid on Alcohol," Phil. Mag. Jan. 1857; Ann. der Chem. und Pharm. cii. 26.

the filtrate from the oxalate of lime over sulphuric acid *in vacuo*. The glyoxylate of ammonia is obtained in small, prismatic and colourless crystals, which dissolve easily in water. The concentrated solution turns yellow when it is boiled, or evaporated at 100° C. It produces with nitrate of silver and acetate of lead crystalline precipitates at once, but with sulphate of copper only after the lapse of some time. Solution of caustic potash, mixed with it, causes the evolution of ammonia even at common temperatures.

According to the composition of glyoxylate of ammonia, the formula of glyoxylic acid is $C_2 H_2 O_3$*. It is worthy of notice, that the other salts of this acid which have been examined, contain one or two atoms of water which cannot be expelled without decomposing the compound.

Glyoxal is oxidized, by treatment with dilute nitric acid, into oxalic acid, and as an intermediate substance glyoxylic acid is formed.

$$C_2 H_2 O_2 = \text{Glyoxal.}$$
$$C_2 H_2 O_3 = \text{Glyoxylic acid.}$$
$$C_2 H_2 O_4 = \text{Oxalic acid.}$$

Glyoxal has not been proved with certainty to be one of the products of the oxidation of glycol; nevertheless its formation from this alcohol may be foreseen with great probability, as it stands to glyoxylic and oxalic acids, both of which are formed by the oxidation of glycol, as ethylic aldehyde does to acetic acid. The relation of glyoxylic to oxalic acid is like that of sulphurous to sulphuric acid.

$$\underbrace{C_2 H_2 O_3}_{\text{Glyoxylic acid.}} \qquad \underbrace{SH_2 O_3}_{\text{Sulphurous acid.}}$$
$$\underbrace{C_2 H_2 O_4}_{\text{Oxalic acid.}} \qquad \underbrace{SH_2 O_4}_{\text{Sulphuric acid.}}$$

Glyoxylic acid resembles in many respects formic acid. Concentrated sulphuric acid separates from the salts of formic acid carbonic oxide, whilst it combines with the rest of the constituents. Glyoxylate of lime dissolves in an excess of sulphuric acid, and the solution evolves at a temperature of from 40° to 50° C., without blackening, pure carbonic oxide. At the end of the experiment, and when the

* "On Glyoxal," Phil. Mag. Jan. 1857; Ann. der Chem. und Pharm. cii. 29.

temperature of the liquid is raised, a little sulphurous acid makes its appearance.

Relying on Berthelot's experiments, formic acid we can conceive to be formed by the addition of the molecule carbonyle to the type water. In the same manner glyoxylic acid might be produced by adding oxalyle to one atom of water.

$$CO + H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}}$$

$$C_2O_2 + H_2O = \underbrace{C_2H_2O_3}_{\text{Glyoxylic acid.}}$$

Since both acids are easily oxidized, the one to carbonic, and the other to oxalic acid, we may say that, with regard to composition, formic acid stands to carbonic acid in a similar relation as glyoxylic acid does to oxalic acid.

$$CO + H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} \qquad C_2O_2 + H_2O = \underbrace{C_2H_2O_3}_{\text{Glyoxylic acid.}}$$

$$CO + H_2O_2 = \underbrace{CH_2O_3}_{\substack{\text{Carbonic acid}\\ \text{plus water.}}} \qquad C_2O_2 + H_2O_2 = \underbrace{C_2H_2O_4}_{\text{Oxalic acid.}}$$

But with regard to other properties, these acids do not correspond to each other: thus formic acid is monobasic and carbonic acid bibasic, whilst both glyoxylic and oxalic acids are biatomic. Further, the latter two are derived from the same alcohol, which is not the case with carbonic and formic acids. Amongst the products of the oxidation of glycol occurs also glycolic acid, which is produced from glyoxal by the assimilation of water.

$$\underbrace{C_2H_2O_2}_{\text{Glyoxal.}} + H_2O = \underbrace{C_2H_4O_3}_{\text{Glycolic acid.}}$$

Some regard glycolic acid ($C_2H_4O_3$) as a bibasic acid. According to this view, it would stand to glycol as acetic acid does to common alcohol.

$$\underbrace{C_2H_6O_2}_{\text{Glycol.}} \qquad \underbrace{C_2H_6O}_{\text{Alcohol.}}$$

$$\underbrace{C_2H_4O_3}_{\text{Glycolic acid.}} \qquad \underbrace{C_2H_4O_2}_{\text{Acetic acid.}}$$

But with this view the capacity of saturation of glycolic acid does not agree. For according to our present experience, if we adopt the formula $C_2\,H_4\,O_3$, it evidently combines only with one atom of base. Before we have discovered compounds represented by the general expression $C_2\,H_2\,M_2\,O_3$, I think we are not justified to place glycolic acid in the same relation to glycol as we do acetic acid to common alcohol. According to our present knowledge, this position must be assigned to oxalic acid.

We can represent to our minds the derivation of glycolic acid from acetic acid in the following manner. If in acetic acid one atom of hydrogen be replaced by peroxide of hydrogen, we obtain a body which has the composition of glycolic acid.

$$C_2\,H_4\,O_2 \qquad = \text{acetic acid.}$$
$$C_2\,H_3\,(HO)\,O_2 = \text{glycolic acid.}$$

But, according to the following observations, this view appears not to be correct; for glycolic acid can be formed from glycolide and water, and probably also again can be resolved into these two substances in the same way as lactid and water by their union produce lactic acid, and the decomposition of the latter by heat yields lactid again. If, therefore, peroxide of hydrogen be contained in glycolic acid, it must be present in glycolide.

If monochlorinated acetate of potash be decomposed, according to Kekule, glycolic acid and glycolide are formed. The formation of the latter precedes that of the glycolic acid; but if the monochlorinated acetate of potash could be obtained anhydrous, no doubt only glycolide would result from its decomposition. The monochlorinated acetate of potash can be represented as consisting of three parts,—of potassium, of oxygen, and of chlorinated acetyle.

$$\left.\begin{matrix} C_2\,H_2\,ClO \\ K \end{matrix}\right\}O.$$

At a higher temperature chlorine and potassium form chloride of potassium. After the removal of the chlorine, the chlorinated acetyle is converted into the biatomic radical $C_2\,H_2\,O$, which unites with one atom of oxygen, and forms glycolide.

$$\left.\begin{matrix} C_2\,H_2\,OCl \\ K \end{matrix}\right\}O$$

According to its formation, the glycolide cannot contain any peroxide of hydrogen, and consequently it appears that also glycolic acid cannot be considered as acetic acid wherein one atom of hydrogen has been replaced by peroxide of hydrogen.

I beg to conclude with an empirical derivation of several of the compounds mentioned from hydruret of ethyl.

$$C_2\,H_2\,H_2\,H_2 \qquad = \text{Hydruret of ethyl.}$$
$$C_2\,H_2\,H_2\,O \qquad = \text{Aldehyde.}$$
$$C_2\,H_2\,O\;\;O \qquad = \text{Glyoxal and glycolide.}$$
$$C_2\,O\;\;O\;\;O \qquad = \text{Anhydrous oxalic acid.}$$
$$C_2\,H_2\,H_2\,H_2+O \qquad = \text{Alcohol.}$$
$$C_2\,H_2\,H_2\,O+O \qquad = \text{Acetic acid.}$$
$$C_2\,H_2\,O\;\;O+O \qquad = \text{Glyoxylic acid.}$$
$$C_2\,O\;\;O\;\;O+O \qquad = \text{Carbonic acid.}$$
$$C_2\,H_2\,H_2\,H_2+H_2\,O = C_2\,H_8\,O.$$
$$C_2\,H_2\,H_2\,O+H_2\,O = \text{Glycol.}$$
$$C_2\,H_2\,O\;\;O+H_2\,O = \text{Glycolic acid.}$$
$$C_2\,O\;\;O\;\;O+H_2\,O = \text{Oxalic acid.}$$

April 7, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "On Colour-Blindness." By WILLIAM POLE, Esq. Communicated by Professor STOKES, Sec.R.S. Received March 24, 1859.

This paper consists principally of a revised and condensed version of a former one by the same author, which was read to the Society on the 19th of June, 1856, and of which an abstract is given in the 'Proceedings' under that date (vol. viii. p. 172). In the present communication there is added an account of numerous experiments subsequently made by the author with Professor Maxwell's Colour-top.

II. "On the Construction of Life-Tables; illustrated by a New Life-Table of the Healthy Districts of England." By WILLIAM FARR, M.D., F.R.S., Superintendent of the Statistical Department, General Register-Office. Received March 17, 1859.

(Abstract.)

The Transactions of the Royal Society contain the first Life-Table: it was constructed by Halley, who discovered its remarkable properties, and illustrated some of its applications.

With better data improved methods have been found out, and the form has been extended so as to facilitate the solution of various questions.

In deducing the English Life-Tables from the National Returns, I have had occasion to try various methods of construction; and I now propose to describe briefly the nature of the Life-Table, to lay down a simple method of construction, to describe an extension of its form, and to illustrate this by a new Table representing the vitality of the healthiest part of the population of England.

The Life-Table is an instrument of investigation; it may be called a *biometer*, for it gives the exact measure of the duration of life under given circumstances. Such a Table has to be constructed for each district and for each profession, to determine their degrees of salubrity. To multiply these constructions, then, it is necessary to lay down rules, which, while they involve a minimum amount of arithmetical labour, will yield results as correct as can be obtained in the present state of our observations.

A Life-Table represents a generation of men passing through time; and time under this aspect, dating from birth, is called age. In the first column of a Life-Table age is expressed in years, commencing at 0 (birth), and proceeding to 100 or to 110 years, the extreme limit of observed lifetime. Annexed is an outline of the two primary series of the Life-Table, representing the surviving at each year of age (l_x), and the first differences representing the dying (d_x), in annual intervals of age.

Age.	Dying.	Living.	Age.	Dying.	Living.
x.	d_x.	l_x.	x.	d_x.	l_x.
0	10,295	100,000	40	618	63,756
1	3,005	89,705	50	722	57,203
2	1,885	86,700	60	1123	48,855
3	1,305	84,815	70	1825	34,278
4	1,051	83,510	80	1803	14,971
5	847	82,459	90	555	2,265
10	347	79,525	100	19	46
20	552	75,600	106	1	1
30	598	69,792			

The Table may be read thus: of 100,000 children born, 10,295 die in the first year, 89,705 survive.

It will be observed that, upon the hypothesis that the annual births equal the annual deaths in number, and that the law of mortality remains invariable, the series of the living (l_x) can be constructed from the series of numbers (d_x) representing the dying, or from the numbers dying at different ages, as returned in the parish registers. That course was adopted by Halley, and afterwards by Dr. Price, in constructing the Northampton Table. But the hypothesis of an invariable annual number of births equalling the deaths has never been verified by observation, and consequently tables on the plan of Halley's are often exceedingly erroneous. In the healthiest districts of England the births were 29,715, the deaths 17,469 annually: a Table constructed upon that plan, like Dr. Price's, makes the *mean lifetime*—or as it is sometimes called, the expectation of life—for Northampton, 25 years, while the mean lifetime by a correct Northampton Table is 38 years.

It is shown by a diagram that if age (x) is represented by the abscissas, the numbers living (b_x) will be represented by the ordinates of a curve. De Moivre constructed this curve by assuming that the series l_x is from the age 12 to 86, in arithmetical progression; decreasing thus, 74, 73, 72 . . . 3, 2, 1, 0. By another hypothesis, the rate of mortality is assumed to decrease or to increase in geometrical progression at different rates in different periods of life; and it is found that this hypothesis represents the results deduced from the observed facts approximatively.

As v, the velocity, expresses a ratio, so m, the rate of mortality, is the ratio of the number dying to the number living in a unit of

time. Now if y represent the living at a definite age, and r the rate at which the mortality increases at that point of age, then mr^z will be the rate of mortality after the lapse of z units of time. The decrement of y in an infinitely short time will be $dy=ymr^z dz$. This was pointed out by Mr. Gompertz, and Mr. Edmonds subsequently extended the theory. This expression can be integrated, and the final equation of the corrected integral is $y=10^{\frac{k^2m}{\lambda r}(1-r^z)}$; where λ is put for the common logarithm, and k for its modulus.

Either of the hypotheses gives a close approximation to the exact result, within short intervals of time; and the results by the two hypotheses agree at the principal ages after 20, when they can be fairly tested. Thus, if the rate of mortality in any year of age (x to $x+1$) is m, then

$$1+\tfrac{1}{2}m : 1-\tfrac{1}{2}m :: l_x : \frac{1-\frac{1}{2}m}{1+\frac{1}{2}m} . l_x = l_{x+1}.$$

It is here assumed that m is known; and putting p_x for $\frac{1-\frac{1}{2}m}{1+\frac{1}{2}m}$, we have $p_x l_x = l_{x+1}$; and have thus the means of passing from the numbers living at the age x to the numbers living at the age $x+1$. But upon the other hypothesis, y_0 being $=1$, then

$$p_x = y_1 = 10^{\frac{k^2m}{\lambda r}(1-r)}.$$

Upon the two hypotheses,

$\lambda p_{20} = \bar{1}\cdot9966528$ by the one, and $\bar{1}\cdot9966527$ by the other;

$\lambda p_{40} = \bar{1}\cdot9956263$ by the one hypothesis, and $\bar{1}\cdot9956264$ by the other.

At 80 there is some divergence. I have adopted the latter hypothesis generally; but the other hypothesis is at some of the earlier ages preferred. I have only adopted these hypotheses within the safe limits of a single year in determining eleven values of λp_x, which I have afterwards interpolated by the method of finite differences; thus assuming that the third difference was constant. This gives, I conceive, as near an approximation as we can obtain in the present state of the observations.

λp_x is the first difference of the series λl_x; and consequently it can be constructed by four orders of differences, on the assumption that no error of consequence is caused by assuming that within given limits the fourth difference is constant.

Healthy Districts.

Age.	Persons Dying in each year of age 0—1, 1—2, &c.	Persons Born and Living at each age.	Sum of the Numbers Born and Living at each age (x), from x to the last age in the Table.	Population, or the Living in each year of age 0—1, 1—2, &c.	(1) Sum of the Living in every year, and of the Living of every age (x) and upwards to the last age in the Table, also (2) the Years which the persons (l_x) will live.	(1) The Years which the persons at the age (x) and upwards will live; also (2) the Years which they have lived over x.	Age.
x.	d_x.	l_x.	L_x.	P_x.	Q_x.	Y_x.	x.
0	10,295	100,000	4,951,908	92,611	4,899,665	166,209,701	0
1	3,005	89,705	4,851,908	88,202	4,807,054	161,356,341	1
2	1,885	86,700	4,762,203	85,758	4,718,852	156,593,388	2
3	1,305	84,815	4,675,503	84,162	4,633,094	151,917,415	3
4	1,051	83,510	4,590,688	82,985	4,548,932	147,326,402	4

Mean After-lifetime.

Age.	Persons.				Males.		Females.	
	Carlisle.	England, 1838—44.	63 Healthy Districts.	Sweden.	England.	63 Healthy Districts.	England.	63 Healthy Districts.
0	39	41	49	...	40	49	42	49
20	41	40	43	38	40	43	41	43
40	28	27	30	25	27	29	28	30
60	14	14	15	13	14	15	14	16
80	6	5	6	4	5	5	5	6

The new Life-Tables consist of three sections,—the first representing persons, the second males, and the third females, each section consisting of six columns. On the opposite page is an extract from the first of the sectional Tables.

The properties of these columns are described, and a collection of useful formulas is added. The curious and useful properties of the new column y are illustrated.

The mean annual mortality in England was at the rate of 22 in 1000; but in eighteen districts the mortality ranged from 28 to 36 in 1000, and in sixty-four districts the mortality ranged from 15 to 16 and 17 in 1000; in the other districts it was at intermediate rates. The Table has been constructed from sixty-three of the districts in which the mortality did not exceed 17 in 1000.

Halley first pointed out some of the financial applications of the Life-Table, and the new Table shows that the mean duration of life among large classes of the population exceeds considerably the expectations of life deduced from the ordinary Tables. The science of public health has been developed since Halley's day; and here a new application of the Life-Table is found. If we ascertain at what rate a generation of men die away under the least unfavourable existing circumstances, we obtain *a standard* by which the loss of life under other circumstances is measured; and this I have endeavoured to accomplish in the New Life-Table. And recollecting that the science of public health was almost inaugurated in England by a former President of this Society, who crowned the sanitary discoveries of Captain Cook, I feel assured that the Society will receive with favour this imperfect attempt to supply sanitary inquirers with a scientific instrument.

April 14, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read :—

I. "On the means by which the Actiniæ kill their Prey." By AUGUSTUS WALLER, M.D., F.R.S., Professor of Physiology in Queen's College, Birmingham. In a Letter to Dr. SHARPEY, Sec. R.S. Received March 11, 1859.

In the 'Proceedings of the Royal Society' for the 18th November, p. 478, I perceive that Dr. M'Donnell's fresh observations on the Actiniæ have led him to abandon the opinion which he had been disposed to entertain as to their possessing electrical powers similar to those of the torpedo. During a stay at the sea-side in the winter of 1857–58, I put in hand some experiments for the purpose of testing the supposed electrical powers of these animals, which, as I some months since mentioned to you, led me to negative conclusions relative to their siderant power. Dr. M'Donnell's recent observations having removed any occasion of controversy, I will briefly mention the results that I obtained.

The most interesting fact observed by Dr. M'Donnell is the contraction of the galvanoscopic frog when the Actinia seized upon the sciatic nerve. On repeating this experiment, I was particularly struck by the uncertainty and irregularity with which these contractions were obtained, being sometimes very strong, while at others they were imperceptible, notwithstanding all the precautions that I could take as to the frogs being fresh caught and irritable, besides attending to the rules laid down by Matteucci.

On the other hand, when, in lieu of a galvanoscopic frog, I presented a Nereis to the Actinia, the result was invariably the death of the animal. The effect of the Actinia's grasp upon the Annulata is mortal, although the retention may not have been allowed to exceed a few moments. The first symptom which I observed was that of writhing, as if the creature were in great pain, and which in the most marked cases was succeeded by paralysis with flaccidity of the muscles, like a frog acted upon by woorara. The action of the

dorsal vessel, which still persisted long after the loss of voluntary power, was very irregular and segmental, the vessel being bloodless and inert at intervals.

It appeared indifferent whether the cephalic or the caudal extremity of the Nereis was attacked by the Actinia, similar symptoms being produced in both cases.

In order to ascertain how far these symptoms were produced by electricity, I subjected the Nereis enclosed in a glass tube to some violent shocks by means of an electro-magnetic machine, which were merely productive of a slight temporary inconvenience to the animal, unattended by any after evil effects. It is most remarkable what powerful electric action these creatures are susceptible of enduring without injury; the strongest action of an electro-magnetic machine on Du Bois Reymond's principle, which affected myself violently up to the elbows, appeared to be easily endured by them.

The above experiment is quite sufficient to show how impossible it is to attribute the fatal influence of the Actiniæ to simple electrical action.

In order to elucidate the real power of the Actiniæ—after having in vain exposed the finger on which the cuticle had been softened by soaking in water—considering that the tongue was better adapted for the purpose in view, by reason of the thinness of its cuticle, I presented its apex to the tentacles of an *Actinia mesembryanthemum*, of about the size of a half-crown piece. The result was such as to satisfy the most sceptical respecting the offensive weapons with which it is furnished. The animal seized the organ most vigorously, and was detached from it with some difficulty after the lapse of about a minute. Immediately a pungent acrid pain commenced, which continued to increase for some minutes until it became extremely distressing. The point attacked felt inflamed and much swollen, although to the eye no change in the part could be detected. These symptoms continued unabated for about an hour, and a slight temporary relief was only obtained by immersing the tongue in cold or warm water. After this period the symptoms gradually abated, and about four hours later, they had entirely disappeared. A day or two after, a very minute ulceration was perceived over the apex of the tongue, which disappeared after being touched with nitrate of silver.

I have subsequently frequently repeated this experiment on myself

and others, using greater precaution, and have invariably obtained similar symptoms of urtication. In only one instance has a minute ulceration been the consequence.

It is very evident therefore that the Actiniæ act by means of an acrid irritant poison, similar in some respects to that of the wasp, or of snakes, which quickly spreads through the system of the Annelida, producing the above-mentioned results.

It remained to determine whether the poisoned weapons existing in such numbers over the surface of the Actiniæ were left in the part attacked. For this purpose I stretched a thin India-rubber membrane over a glass tube. After its seizure by the Actinia, I found that under the microscope it was studded in many points with the poison darts inserted slightly in the membrane, without their having penetrated through. In this respect my observations differ from those of Mr. Gosse, who considers that a fragment of cuticle from the hand was perforated by these darts.

I remain, &c.,

Augustus Waller.

II. "On the Double Tangent of a Plane Curve." By Arthur Cayley, Esq., F.R.S. Received March 17, 1859.

(Abstract.)

The author notices that the problem of finding the number of double tangents was first solved by Plücker in 1834 from geometrical considerations, and he gives a sketch of the subsequent history of the problem. The complete analytical determination of the double tangents was only obtained very recently by Mr. Salmon, and is given in a note by him in the Philosophical Magazine, October 1858: it is there shown that the $(n-2)$ points in which the tangent at any point of a curve of the order n again meets the curve, are given as the points of intersection of the tangent with a certain curve of the order $(n-2)$; if this curve be touched by the tangent, then the point of contact will be also a point of contact of the tangent and the curve of the order n, or the tangent will be a double tangent. The present memoir relates chiefly to the establishment of an identical equation, which puts in evidence the property of the curve of the

order $(n-2)$, and which the author considers to be also important in reference to the general theory of binary quantities: viz. if $YU=\mathrm{II}(*)(x, y, z)^n$, $DU=(X\partial_x+Y\partial_y+Z\partial_z)U$, and Υ, $\mathfrak{D}\Upsilon$ are what U, DU become when (x, y, z) and (X, Y, Z) are interchanged; then the equation is of the form $\mathrm{I}\,.\,\Upsilon+\mathrm{II}\,.\,\mathfrak{D}\Upsilon+\mathrm{III}\,.\,DU+\mathrm{IV}\,.\,U=0$. Taking (x, y, z) as current coordinates and $U=0$ as the equation of the curve, then if (X, Y, Z) are the coordinates of a point on the curve, $\Upsilon=0$, and we have for the equation of the tangent at the point in question $\mathfrak{D}\Upsilon=0$. The equation shows that the intersections of the curve $U=0$ and the tangent $\mathfrak{D}\Upsilon=0$, lie on one or other of the curves $\mathrm{III}=0$, $DU=0$, and that they do not lie on the curve $DU=0$; consequently they lie on the curve $\mathrm{III}=0$, which is in fact the before-mentioned curve of the order $(n-2)$.

III. "On the Action of Acids on Glycol." By Dr. MAXWELL SIMPSON. Communicated by Dr. FRANKLAND. Received March 24, 1859.

The glycol employed in the following research was prepared according to Dr. Atkinson's excellent method *, to whom is due the credit of having first substituted acetate of potash for acetate of silver in its preparation. He was not the first, however, to prepare it from bromide of ethylene, as M. Wurtz has been in the habit of preparing it from that body for the last two years.

The following slight modification of Dr. Atkinson's method will be found very convenient, particularly when large quantities of glycol are to be prepared. Instead of heating the materials for forming the monoacetate of glycol in a close vessel, they are heated in a large balloon, connected with a Liebig's condenser in such a manner as to cause the condensed vapours to flow back into the balloon.

Action of Sulphuric Acid on Glycol—Sulphoglycolate of Baryta.—Sulphuric acid forms an acid ether with glycol, which gives a soluble salt with baryta. This compound is readily prepared by exposing a mixture of equivalent quantities of glycol and sulphuric acid ($S_2H_2O_8$) to the temperature of 150° Cent., diluting with water and neutralizing with carbonate of baryta. This liquid, on being

* Philosophical Magazine, Dec. 1858.

filtered, and evaporated on a water-bath to the consistence of a syrup, gives on cooling a white solid mass, which is the body in question. This was pressed between folds of blotting-paper, dried *in vacuo* over sulphuric acid, and analysed. The numbers obtained on analysis lead to the formula

$$\left.\begin{matrix} C_4\,H_5\,O_2 \\ S_2\,O_4 \\ Ba \end{matrix}\right\} O_4,$$

as will be seen from the following per-centage Table :—

	Theory.		Experiment.			
			I.	II.	III.	IV.
C_4	24·00	11·45	10·71*	——	——	——
H_5	5·00	2·40	2·79	——	——	——
$S_2\,O_6$	80·00	38·15	——	38·09	——	——
BaO	76·50	36·51	——	——	36·50	36·10
O_3	24·00	11·49	——	——	——	——
	209·5	109·00				

The formation of this compound may be thus explained :—

$$C_4 \left.\begin{matrix} H_4 \\ H_2 \end{matrix}\right\} O_4 + S_2\,H_2\,O_8 = \left.\begin{matrix} C_4\,H_5\,O_2 \\ S_2\,O_4 \\ H \end{matrix}\right\} O_4 + H_2\,O_2.$$

On neutralizing this compound with carbonate of baryta, the basic hydrogen is replaced by one atom of barium. I propose to call this salt sulphoglycolate of baryta. It is analogous in composition to the sulphoglycerate of baryta obtained by M. Pelouze. This salt does not readily crystallize. It is almost insoluble in ether and in absolute alcohol, but freely soluble in water. It is somewhat deliquescent. Exposure to the temperature of 100° Cent. causes slight decomposition. From its solution in water, sulphuric acid precipitates sulphate of barytes. Baryta-water occasions no precipitate, at least in the cold; on heating, however, for some time, it becomes turbid, from the separation of the same salt.

Action of Hydrochloric and Acetic Acids on Glycol—Chloracetine of Glycol.—A mixture of equivalent quantities of glycol and glacial acetic acid was introduced into a long tube and saturated with dry hydrochloric acid. The tube was then hermetically sealed, and exposed to the temperature of a water-bath for about four hours. On

* Chromate of lead was employed in this analysis.

opening the tube and adding water to its contents, a heavy oil separated, which was well washed with the water, in order to remove any acetic acid or undecomposed glycol it might contain, dried over chloride of calcium, and distilled. Almost the entire quantity passed over between 144° and 146°. Specimens obtained at different times gave the following numbers on analysis, which lead to the formula

$$\left.\begin{array}{l} C_4\,H_4 \\ C_4\,H_3\,O_2 \\ \quad\quad\;\; Cl \end{array}\right\} O_2.$$

C_8	48·00	39·18	38·96	38·98	——
H_7	7·00	5·71	6·05	5·99	——
O_4	32·00	26·14	——	——	——
Cl	35·50	28·97	——	——	27·48*
	122·50	100·00			

I propose to call this body chloracetine of glycol. It is the intermediate compound between Dutch liquid and diacetate of glycol. Its formation may be thus explained:—

$$\left.\begin{array}{l} C_4\,H_4 \\ \;\;\; H_2 \end{array}\right\} O_4 + \left.\begin{array}{l} C_4\,H_3\,O_2 \\ \;\;\;\; H \end{array}\right\} O_2 + HCl = \left.\begin{array}{l} C_4\,H_4 \\ C_4\,H_3\,O_2 \\ \quad\quad\;\; Cl \end{array}\right\} O_2 + 4\,HO.$$

Chloracetine of glycol is a colourless liquid, heavier than water, its specific gravity being 1·1783 at 0° Cent. It boils at 145°, distilling without decomposition. It is not decomposed by cold water, at least not to any great extent; even boiling water effects its decomposition with difficulty. Heated with potash, it gives chloride of potassium and acetate of potash. It is probable that the ether of glycol is also formed in this reaction, or perhaps glycol itself. This point I have not yet been able to determine. Chloracetine is isomeric with a compound I obtained a short time ago, by exposing ordinary aldehyde to the action of chloride of acetyle ($C_4\,H_3\,O_2\,Cl$)†. This body differs from the chloracetine in its boiling-point, which is about 23 degrees lower, and in being more readily decomposed both by water and potash. The products formed by the action of potash also establish a difference between these bodies. Both give chloride of potassium and acetate of potash, but the body from aldehyde gives, in addition, resin of aldehyde; whereas from chloracetine no resin

* A slight loss occurred in this analysis.

† Comptes Rendus, 29 Nov. 1858.

could be obtained. Chloracetine has since been prepared by M. Lorenzo in a manner analogous to that by which I prepared its isomer, namely, by treating glycol with chloride of acetyle. These compounds find their places in two very remarkable series of isomeric bodies, proceeding from ethylene (olefiant gas) and ethylidine (C_4H_4?), supposed to be contained in the chloride of ethylidine derived from aldehyde. The following is a list of these compounds:—

	Ethylidine. C_4H_4 (?)	Ethylene (olefiant gas). C_4H_4	
Aldehyde . .	$C_4H_4O_2$	$C_4H_4O_2$	Oxide of ethylene. Ether of glycol (Wurtz).
Chloride of ethylidine . (Wurtz)	$C_4H_4Cl_2$	$C_4H_4Cl_2$	Dutch liquid.
Geuther . .	$\left.\begin{matrix}C_4H_4\\C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}O_4$	$\left.\begin{matrix}C_4H_4\\C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}O_4$	Diacetate of glycol (Wurtz).
Chloracetine of ethylidine (Simpson)	$\left.\begin{matrix}C_4H_4\\C_4H_3O_2\end{matrix}\right\}\begin{matrix}O_2\\Cl\end{matrix}$	$\left.\begin{matrix}C_4H_4\\C_4H_3O_2\end{matrix}\right\}\begin{matrix}O_2\\Cl\end{matrix}$	Chlor-acetine of glycol (Simpson).
(Wurtz and Frapolli)	$\left.\begin{matrix}C_4H_4\\C_4H_5\end{matrix}\right\}\begin{matrix}O_2\\Cl\end{matrix}$	$\left.\begin{matrix}C_4H_4\\C_4H_5\end{matrix}\right\}\begin{matrix}O_2\\Cl\end{matrix}$	Not yet discovered.
Acetal (Döbereiner)	$\left.\begin{matrix}C_4H_4\\(C_4H_5)^2\end{matrix}\right\}O_4$	$\left.\begin{matrix}C_4H_4\\(C_4H_5)^2\end{matrix}\right\}O_4$	Diethyl-glycol (Wurtz).

I am still studying the action of acids on glycol, and hope soon to be able to communicate further results. The foregoing experiments were performed in M. Wurtz's laboratory.

The Society then adjourned over the Easter recess to Thursday, May 5.

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END OF THE NINTH VOLUME.

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